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*Classical Social Theory and Modern Sociology—Giddens*

*Social Structure from Multiple Networks. I—White, Boorman,  
and Breiger*

*Development Towns in Israel—Spilerman and Habib*

*Simmel's Influence on American Sociology. I—Levine, Carter,  
and Gorman*

*Race Composition of Neighborhood Businesses—Aldrich and Reiss*

*On the Concept of Face—Ho*

*Review Essay by Elkana on Merton*

*Review Essay by Hauser on Boudon*

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## IN THIS ISSUE

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RALPH H. TURNER is professor of sociology at the University of California, Los Angeles. He is studying the relationship between person and role as part of a more comprehensive examination of role theory. Recently he has also been engaged in trying to use the sociological understanding of collective behavior to estimate probable community response to the prediction of an earthquake.

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AVERY M. GUEST, associate professor of sociology at the University of Washington, is currently engaged in research on the relationship between home and workplace in cities and on the historical development of American suburban communities.

JAMES A. WEED, assistant professor of sociology at Indiana University, is primarily interested in the demographic aspects of changing family formation in the United States. Both he and Professor Guest were connected with the Center for Studies in Demography and Ecology at the University of Washington when they wrote the article in this issue.

DONALD N. LEVINE is professor of sociology at the University of Chicago. He is the editor and co-translator of *Georg Simmel on Individuality and Social Forms* and the author of *Wax and Gold: Tradition and Innovation in Ethiopian Culture* and *Greater Ethiopia: The Evolution of a Multiethnic Society*.

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# Classical Social Theory and the Origins of Modern Sociology

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The object of this paper is both critical and constructive. The first section contains a critical account of some leading interpretations of the rise of modern sociology in the late 19th and early 20th centuries in Europe, concentrating primarily on Durkheim but referring also to Max Weber. I try to show that these interpretations, still very commonly held, have to be abandoned as *myths*. Like all myths, however, they contain a rational kernel, and in seeking to show what this might be, I indicate some lines of development which I consider to be important in sociology in the present day. Most characterizations of the current travails of social theory are concerned with issues of epistemology, that is, with problems of the sorts of "truth claims" that can be made in sociology. These matters, undeniably of pressing importance, are related to legacies from 19th-century social thought which we have to disavow. But there is another residue of the 19th century with which we have also to break: this is represented by what I call the *theory of industrial society*. An essential task facing contemporary social theory is that of reconciling a revised epistemology of social science with new frameworks for the analysis of the development of advanced societies.

My aims in this paper are both iconoclastic and constructive. An iconoclast, according to the *Oxford English Dictionary*, is a "breaker of images," "one who assails cherished beliefs." I begin by taking to task a series of widely held views, relating above all to Durkheim's writings, of the past development of social theory. These views, as I have tried to show elsewhere (Giddens 1972b), are *myths*; here I try not so much to shatter their images of the intellectual origins of sociology as to show that they are like reflections in a hall of distorting mirrors. I do not, however, propose to analyze the development of classical 19th- and early 20th-century social theory for its own sake alone, but wish to draw out some implications for problems of sociology today.

## SOME MYTHS IDENTIFIED

There are obviously many different interpretations of the rise of social theory from its origins in 19th-century Europe to the present day; the views and controversies they express cannot readily be compressed within any simple analytical scheme. But at least certain influential ones are in-

formed by a particular perspective which I call that of "the great divide." This is the idea that a fundamental watershed separates the prehistory of social theory, when it had not yet been disentangled from speculative philosophy or the philosophy of history, from its foundation as a distinctive and novel science of society. The most prominent of the cherished versions of this notion locates the great divide in the writings of certain European authors whose major works appeared between 1890 and 1920—especially in the writings of Durkheim and Weber, closely followed by those of Pareto, Michels, Simmel, and others. While it may be misleading to mention one particular secondary work developing this version of the great divide, since it represents an orthodoxy which crops up almost everywhere, it would be hard to dispute that Parsons's *The Structure of Social Action* (1937), perhaps the most influential study of European social theory published in English over the last half century, has played the largest part in establishing it as an orthodoxy.

It has often been pointed out that in the above work only minimal reference is made to the writings of Marx and Engels, which are cursorily relegated to limbo. But of course Marxism has its own rendition of the great divide, offering a very different analysis of the ideas produced by the writers of the 1890–1920 generation. Like the version mentioned above, the Marxist view has been stated with varying degrees of sophistication. Essentially, however, it runs that the foundations for a science of society were established by Marx and Engels when they forsook the speculative philosophy of history, as represented by Hegel and Feuerbach. (Perhaps the most technically precise account of this nature is that offered by Althusser's [1969] thesis of the supposed "epistemological break" in Marx's intellectual career, which separates philosophy from science in the development of Marxism as a coherent body of thought.) While I do not, in this article, examine this sort of claim directly, I do discuss the view that considers the works of Durkheim, Weber, and their non-Marxist contemporaries to be a response to the challenge posed by Marxism or by revolutionary socialism more generally. In its more extreme guise, the Marxist version of the great divide provides a rationale for dismissing the ideas of the "bourgeois" writers of the 1890–1920 period as mere ideology. But, stated in subtler form, this kind of view holds that Marx's writings represent the great divide in the history of social thought because they are the axis about which the work of the subsequent generation of thinkers (and perhaps each later generation up to and including the present one) has turned. Such a perspective has been developed in a European context by Georg Lukács, Lucien Goldmann, and others, but has also found expression among certain recent American authors (see, e.g., Zeitlin 1968).

I want to show that each of these competing versions of the significance

of the 1890–1920 generation is mistaken and that the whole notion of the great divide as formulated by Parsons, and is a less sophisticated way by many others, is a myth. I do not suggest that major divides cannot be found in the history of social thought; nor indeed that there are no elements of truth in the accounts I shall analyze. But we must extract the rational kernel from its shell. Before specifying the elements of truth in the thesis of the great divide, I wish to discuss certain other notions about the history of European social thought which have been quite often closely associated with one or another account of the great divide, and which are particularly, though not exclusively, connected with Durkheim's writings: notions which also involve mythologies (Giddens 1972*b*, pp. 357–58).<sup>1</sup> They are:

1. *The myth of the problem of order.*—According to this idea, the work of some or even most of the outstanding non-Marxist authors of the 1890–1920 period (but especially that of Durkheim) can profitably be understood as being preoccupied with an abstract “problem of order” that was a residue of utilitarianism in social philosophy.

2. *The myth of the conservative origins of sociology.*—Although this theme has been developed in varying ways by different authors, it relies primarily upon the thesis that some, or most, of the principal intellectual perspectives in sociology today can be traced fairly directly to a group of early 19th-century authors who reacted against the changes resulting from two great revolutions in 18th-century Europe, the 1789 Revolution in France and the Industrial Revolution in Britain.

3. *The myth of schism.*—This in fact derives from attempts to effect a critique of the idea that a concern with the problem of order has played a vital role in the past development of social thought. According to this view, a preoccupation with order distinguishes only certain traditions in social theory; the history of social thought since the middle of the 19th century, it is supposed, can profitably be regarded as involving a persisting split between “order theory” (alternatively called “consensus” or “integration” theory) on the one side and “conflict” theory (sometimes referred to as “coercion” theory) on the other.

More qualifications are in order. Although these four sets of ideas—the myths of the great divide, the problem of order, the conservative

<sup>1</sup> For a long while the literature on Durkheim (in English, at least) was, with certain notable exceptions, considerably inferior in terms of the level of scholarship to that on Max Weber. But, in very recent years, a flood of new publications has corrected that imbalance. The following deserve particular mention: *Émile Durkheim: His Life and Work* (Lukes 1973), *Émile Durkheim: Sociologist and Philosopher* (LaCapra 1972), *Durkheim: Morality and Milieu* (Wallwork 1972), *Émile Durkheim on Morality and Society* (Bellah 1973), *The Sociology of Émile Durkheim* (Nisbet 1974), *Images of Society* (Poggi 1973), and “Classic on Classic: Parsons’ Interpretation of Durkheim” (Pope 1973).

origins of sociology, and schism—have met with widespread acceptance, none of them has gone undisputed. Nor are they necessarily the most persuasive accounts that have been produced. I claim only that they have been sufficiently influential to be worth refuting. I do not attempt to trace out how far they have in fact become conventional wisdom, but address myself only to those authors (Parsons, Nisbet, and Dahrendorf) whose writings have been most important in advocating the views in question. Also, it would be wrong to say that the four myths provide a unitary perspective on the past or that the proponents of any one of them have necessarily sought to defend the others. Nisbet, for example, who has probably done most to foster the myth of the conservative origins of sociology, has specifically questioned that of the great divide.<sup>2</sup> Nonetheless there are important points of connection. The thesis that the problem of order was one, or perhaps *the*, major issue through which the concerns of contemporary sociology were shaped bolsters both the conviction that these concerns are connected in some privileged way with “conservatism” and the belief that there is some definable historical counterpart to “order theory” that can properly be labeled “conflict theory.” Moreover, as I shall show subsequently, the myths of the problem of order and of conservatism, suitably interpreted, can provide ammunition for certain more naïve Marxist versions of the great divide.

#### THE PROBLEM OF ORDER

As a result of its frequent appearance in Parsons's major works, “the problem of order” has become a catch-phrase in contemporary social theory. Introduced as an interpretative theme in *The Structure of Social Action*, it became firmly established as a key notion in Parsons's subsequent elaboration of his own theory. Below I comment briefly upon the ambiguity of the concept of the problem of order in his later writings; for the moment I wish to examine only its formulation in *The Structure of Social Action*, where it was introduced in relation to Hobbes. The problem of order, according to Parsons, “in the sense in which Hobbes posed it, constitutes the most fundamental empirical difficulty of utilitarian thought” (Parsons 1937, p. 91). The rudiments of the “Hobbesian problem,” as Parsons presents it, are not difficult to express. In a state of nature, each man would be pitted against every other, in a “war of all against all”; Hobbes supposes that by forming a compact with a sovereign authority, men in society escape from this prospect of unremitting struggle. This formulation is essentially inadequate, however, because it rests upon

<sup>2</sup> *The Sociological Tradition* (Nisbet 1967) emphasizes the continuity of European social thought throughout the 19th and early 20th centuries. See Parsons's (1967) review of the work in which Nisbet is taken to task on just this point.



the assumption of social contract, as if actors at some point come to perceive that it is in their best interests to combine to recognize a sovereign authority. Inadequate though it may have been, Parsons says, it nevertheless was accepted unquestioningly as long as utilitarianism remained dominant in social philosophy, that is, until the late 19th century, when it was subjected to a massive reexamination and critique (above all in Durkheim's writings) in which the problem of order became of fundamental concern.<sup>3</sup>

On this basis Parsons incorporated into *The Structure of Social Action* and subsequent writings (see, e.g., Parsons 1960) an analysis of Durkheim's thought which has met with widespread acceptance. But this analysis, influential as it has been, is not an accurate representation of the main thrust of Durkheim's work (for more detailed discussion, see Giddens 1971a, pp. 65 ff.; 1972a, pp. 38–48). The textual evidence against the interpretation that Durkheim was concerned throughout his career with the "Hobbesian problem of order," as formulated by Parsons, is overwhelming. First of all, at a relatively early stage of his intellectual development, Durkheim specifically, though rather casually, dismissed the "Hobbesian problem" as being of no significance for sociology, saying that it depends upon a hypothetical state of affairs (man in a state of nature) which is of no interest to social theory, because it is wrongly posed in the first place (Durkheim 1964b, pp. 122–24). Second, Parsons's account is based upon a misleading identification of the residues of prior intellectual traditions which Durkheim sought to criticize. As Parsons makes clear, the problem of order is tied to the utilitarianism of Hobbes and his successors. Now utilitarianism, not as represented in Hobbes's work but in the considerably more sophisticated guise of the writings of Herbert Spencer, was only one of the polemical targets at which Durkheim aimed his critical salvos in the early part of his intellectual career. Parsons's account concentrates almost exclusively upon utilitarianism as Durkheim's critical foil; but just as important—perhaps more so, since it supplied the main underlying parameters of *The Division of Labour*—was the latter's critical response to German idealism, both the "holism" of Wundt and Schäffle and neo-Kantian philosophy. These schools of thought preoccupied Durkheim in his very first writings, and, as I have tried to show elsewhere, various important ideas appear in those writings which Parsons supposes that Durkheim only arrived at much later, as a consequence of his struggles with the problem of order (see Giddens 1970).

<sup>3</sup> It should be pointed out that Parsons also identified a second major trend in utilitarian theory, resting upon the postulate of the "natural identity of interests" of men in society, and associated with Locke rather than Hobbes; this later became of particular importance in classical economics.

*The Division of Labour* itself is treated by Parsons as an early, and radically flawed, disquisition upon the problem of order. Two consequences flow from this: First, the work is severed from Durkheim's subsequent writings, which are regarded as successive, and progressively more acceptable, attempts to resolve the problem of order, culminating in *The Elementary Forms of the Religious Life*—by which time Durkheim is supposed to have made the full traverse from "positivism" to "idealism" (in the specific senses in which Parsons uses those terms). Second, *The Division of Labour* is exposed as hiding an essential, unresolved ambiguity. By showing that there is a "non-contractual element in contract," Durkheim had, according to Parsons, demonstrated the inadequacy of the Hobbesian solution to the problem of order; but at the same time he had created a dilemma for himself, for where does the "non-contractual element" derive from if the progress of organic solidarity, in terms of which contractual relations are formed, *ipso facto* entails the disappearance of collective values?

I deal with the second point first, since it relates to what I have already said about the intellectual traditions of which Durkheim sought to effect a critique. There is no ambiguity in the argument of *The Division of Labour* if it is viewed, not as an analysis of the problem of order, but as an attempt to reconcile "individualism" (which from the beginning Durkheim disavowed in its original utilitarian form) with "holism," on the basis of a critique of *both*. Durkheim sought to show—as he pointed out clearly enough in the preamble to the book—that the ideals of "individualism," which set a premium upon the freedom and dignity of the individual, are themselves social products and therefore cannot, in the manner of utilitarianism, be treated as the premises of human society in general; and that, since these ideals are both the moral expression and the foundation of organic solidarity, they are not "pathological" (as many idealist writers had suggested) but, on the contrary, represent the incipient moral order of the future (Durkheim 1964a, pp. 41–44). This theme is further developed throughout Durkheim's writings.

It is true that there were important developments in Durkheim's work subsequent to *The Division of Labour*. One of the most significant was his discovery of the work of the English anthropologists which, together with that of Spencer and Gillen, prompted the researches culminating in *The Elementary Forms*. But although he came to have doubts about some of the views expressed in *The Division of Labour*,<sup>4</sup> Durkheim continued to regard the general form of the framework set out therein as valid (adding a famous preface to the second edition in 1902), and drew

<sup>4</sup> See, for instance, the introductory comments in the preface to the second edition of *The Division of Labour*.

upon it extensively in his later writings and lecture courses. If the central place accorded the problem of order in Parsons's exposition of Durkheim's works is abandoned, it becomes clear that the latter's main preoccupation, which of course he shared with many of his contemporaries, was with the contrasts and continuities between "traditional" and "modern" societies. The theory developed in *The Elementary Forms* both elaborates upon the received idea of "mechanical solidarity" as it was originally set out and offers an account of the social sources of morality that is broad enough to include the emergent ideals of "moral individualism," as well as connecting them to traditional theism.<sup>5</sup>

Finally, concentration upon the problem of order as Durkheim's guiding problem leads Parsons to represent the former's work as becoming more and more dominated by the notion of moral consensus, which thus almost completely blanks out his parallel concern with institutional analysis and institutional change. The latter aspect of Durkheim's thought is, however, highly important, not least because it constitutes a major point of connection between it and socialism. Although consistently resistant to the claims of revolutionary socialism, especially Marxism, Durkheim was equally consistently sympathetic to reformist socialism and specifically embodied some of its principles in his own theory. According to him, the moral regeneration necessary for the transcendence of anomie could come about only through a process of profound institutional change. In *The Division of Labour* this process was discussed in terms of the "forced division of labour"; later Durkheim reshaped and expanded the idea as the theory of occupational groups (*corporations*) and the modern State. (For a further analysis, see Giddens 1971*b*.)

I assess below some further implications of the "problem of order"; for the moment it is sufficient to say that, far from supplying the guiding theme of Durkheim's sociology, it was not, in the terms in which Parsons formulates it, a problem for Durkheim at all. Since it was primarily on his interpretation of Durkheim's writings that Parsons rested his case for the significance of the problem of order in the evolution of modern social theory, we can make the further claim that that "problem of order" was not of particular importance in European social thought in the 19th and early 20th centuries. The implications of such a claim cannot be fully worked out without considering afresh the significance of utilitarian philosophy for the development of social thought in the 19th century, as I shall undertake to do, albeit briefly, later in the paper.

<sup>5</sup> Interesting glosses on this appear in passages of Durkheim's discussion of the development of educational systems in Europe from the Middle Ages to modern times (see Durkheim 1969).

THE MYTH OF CONSERVATISM

The notion that the origins of modern sociology are bound up in some special way with conservative ideology has been advanced by various authors, including von Hayek (1964), Salomon (1955), and, nearer to the present day, Robert Nisbet.<sup>6</sup> Here I concentrate on Nisbet's account, again taking Durkheim's works as the main basis of my discussion, since the thesis of the significance of conservatism seems initially to be particularly persuasive when applied to Durkheim's thought and has been taken up in this regard by other interpreters (see, e.g., Coser 1960). But I also refer briefly to the writings of Max Weber.

In *The Sociological Tradition*, Nisbet formulates a powerful and comprehensive interpretation of the rise of European social theory, focusing upon the key role played by conservatism, especially by the "counter-reaction" to the French Revolution as manifest in the doctrines of de Maistre, Bonald, and Chateaubriand. "Conservatism" here does not refer to directly political attitudes but to a series of major analytic concepts which, in Nisbet's view, became established as—and still remain—basic to the sociological tradition. As applied to the elucidation of Durkheim's thought, this is an altogether more subtle and interesting thesis than the one, occasionally expressed several decades ago, that Durkheim was a conservative in his immediate political attitudes and involvements (see Mitchell 1931). If there was ever any doubt about the matter, it rested upon ignorance. Durkheim's sympathies never lay with right-wing nationalism or with its philosophy, and his work was (rightly) regarded by conservative Catholic apologists as highly inimical to their interests.<sup>7</sup> Although he normally remained distant from the day-to-day events of politics, his affiliation was above all to liberal Republicanism (his influence upon Jaurès is well known); and he took an active role in support of the *dreyfusards*.

While acknowledging, then, Durkheim's liberalism in politics, Nisbet wishes to argue that nonetheless the main intellectual parameters of his social theory were formed through the adoption of a frame of concepts drawn from the conservative revolt against the legacy of the 18th-century rationalist philosophers whose ideas inspired the 1789 Revolution. This argument, however, can be taken in either of two possible ways, which are not separated by Nisbet. I call them the "weak" and the "strong" versions of the thesis of conservatism. We may, and ordinarily must, distinguish between the intellectual *antecedents* of a man's thought, the

<sup>6</sup> In mentioning these authors in the same breath as Nisbet, I do not, of course, wish to say that the burden of their message shares much in common with his.

<sup>7</sup> See the bitter attack upon Durkheim's theory in *Le Conflit de la morale et de la sociologie* (Deploige 1911).

traditions he draws upon in forming his views, and the intellectual *content* of his work, what he *makes* of the ideas he takes from the traditions. For a thinker may draw upon a specific range of sources but may sculpture from them an intellectual system quite different from that or those whence they derived. It is entirely possible for a corpus of work to be "conservative" in terms of the schools of thought on which it draws (the weak sense), without being "conservative" as such (the strong sense)—and vice versa. I argue, in fact, that Durkheim's writings cannot be distinctively linked to conservatism in either sense. But I wish to make a further point which can be easily illustrated by reference to the works of other leading social theorists as well: that the work of any outstanding thinker—and this is what makes it outstanding—normally both *synthesizes*, yet also thereby significantly *breaks with*, several apparently divergent intellectual traditions. With regard to the three European thinkers of the 19th and early 20th centuries who did most to frame the development of social theory up to the present time—Marx, Durkheim, and Weber—conservatism, in some sense, appears as an important fragment of their intellectual inheritance. But all of them also sought to transcend what they saw as its particular limitations by synthesizing ideas drawn from it with ideas drawn from competing traditions.

I have already referred to Durkheim's intellectual inheritance, from the point of view of utilitarianism, with regard to the myth of the problem of order. This is not, however, the main focus of Nisbet's account, which concentrates upon Durkheim's indebtedness to the luminaries of the "counter-reaction." As the former expresses it: "It was Durkheim's feat to translate into the hard methodology of science ideas and values that had first made their appearance in the polemics of Bonald, Maistre, Haller and others opposed to reason and rationalism" (Nisbet 1965, p. 25). I think this is quite easily shown to be wrong if it is understood as the weak version of the thesis of conservatism. It is not accurate to say, as Nisbet does, that Durkheim's debt to the rationalism of the 18th-century *philosophes* was wholly a methodological one. Although he rejected major aspects of Rousseau's theory of the State, for example, his critical evaluation of Rousseau's philosophy was certainly not completely negative (see Durkheim 1960). It is evident enough, however, that within the spectrum of French social thought, the contributions of later authors, notably Saint-Simon and Comte, bulked larger on Durkheim's intellectual horizons, and it is through indicating Durkheim's debt to Comte that Nisbet seeks in substantial part to demonstrate the influence of the Catholic reactionary thinkers upon Durkheim's writings. Now Comte acknowledged the importance of the "retrograde school," and there is a clear imprint of the ideas of the latter in the hierocracy envisaged as the corporate society of the future in the *Positive Polity*. But Durkheim explicitly rejected the

basic features of this model; it was Comte's methodological writing, as manifest in the *Positive Philosophy*, which particularly influenced him (together with the more proximate influence of Boutroux). In evaluating and rejecting what he saw as the reactionary implications of the Comteian hierocratic model, Durkheim drew upon elements of the overlapping, yet distinctively different, analysis of the emergent society of the future foreseen by Comte's erstwhile mentor, Saint-Simon (while seeking to effect a critique of the latter also). This is of more than marginal interest, since Saint-Simon's works, inchoate and wild though they frequently were, are vital in the development of 19th-century social theory. Two paths open out from them, one leading to Comte and Durkheim and thence to contemporary "structural-functionalism," the other leading to Marx.<sup>8</sup> I have already alluded to the significance of socialism in Durkheim's intellectual background. This was also mediated through his early exposure, during a period of study in Germany, to the ideas of the "socialists of the chair"; it was undoubtedly partly in response to their work that the underlying themes of *The Division of Labour* were elaborated (see Lukes 1973, pp. 86-95).

I conclude from this analysis that Durkheim's work was not conservative in the weak sense. But one further body of work from which he drew is worthy of mention and immediately relevant to the question whether Durkheim's writings may be regarded legitimately as conservative in the strong sense. This is neo-Kantianism, particularly as developed by Renouvier. One constantly finds Kantian formulations in Durkheim's works, often explicitly acknowledged as such by him. If there is any single problem with which Durkheim was preoccupied, rather than the "Hobbesian problem of order," it was the Kantian problem of the moral imperative. From the early stages of his intellectual career up to and including the publication of *The Elementary Forms*, he was concerned with reformulating some of the key concepts of Kant's philosophy in a social context, seeking to show that both the moral imperative and, in the above-mentioned work, the very categories of the mind, are not to be taken as a priori but, on the contrary, can and should be explained sociologically. In conjunction with the other intellectual emphases that I have mentioned previously, this fact supplies the essential interpretative background for understanding how misleading it is to regard Durkheim's thought as having an inherently conservative cast. For Durkheim was concerned to show, first, that the forms of "individualism" stressed in Kantian and in utilitarian philosophy were the products of an extended sequence of social evolution, rather than primitive and necessary assumptions of social analysis as such; and, second, that individualism is to be

<sup>8</sup> Gurvitch (1950) has argued the case for the influence of Saint-Simon over the subsequent evolution of Marxism.

the moral counterpart of the emerging differentiated society founded upon a diverse division of labor.

One of the main props of the thesis that Durkheim's thought is inherently conservative is that, as Nisbet puts it, it constituted an all-out offensive against individualism (Nisbet 1965, p. 28). But this view rests on a confusion of two senses of "individualism" between which it was precisely Durkheim's object to distinguish: *methodological* individualism and *moral* individualism. One important strand of his writing is a critique of those forms of method—especially utilitarianism—which treat the individual as the starting-point of sociological analysis. But he wished to show that the rejection of individualism as a methodology does not preclude analyzing the development of moral individualism sociologically—on the contrary, the latter process cannot be accomplished otherwise. The rise and significance of moral individualism cannot be understood via the ontological premises of methodological individualism. "The condemnation of individualism," Durkheim says, "has been facilitated by its confusion with the narrow utilitarianism and utilitarian egoism of Spencer and the economists. But this is very facile . . . what is unacceptable is that this individualism should be presented as the only one that there is, or ever could be. . . . A verbal similarity has made possible the belief that *individualism* necessarily resulted from individual, and thus egoistic, sentiments" (Durkheim 1898, pp. 7–8). I do not say that Durkheim's attempt to distinguish between methodological and moral individualism was successful; some of the principal difficulties with, and ambiguities in, his work derive from unresolved dilemmas in this respect.<sup>9</sup> But that it clearly distances his writings from conservatism cannot be disputed. The article from which the above quotation comes was written in relation to the Dreyfus affair specifically as an attack upon contemporary adherents of the sort of hierocratic reactionism prefigured in the writings of de Maistre and others in an earlier generation. In opposition to the conservative ideologists, Durkheim consistently argued that there can be no reversion to the sort of moral discipline that prevailed in former ages (which he sometimes referred to as the "tyranny of the group," and under which there is only a feeble development of individual faculties and capabilities): freedom does not result from escape from moral authority but from its transformation through the emergence of the values of moral individualism.

In *The Sociological Tradition* Nisbet differentiates between conservatism and two other "ideological currents" that helped shape European social thought in the 19th century, "radicalism" and "liberalism." He maintains that each of these also served to mold the thought of major thinkers of

<sup>9</sup> For an analysis of some of the residual difficulties in Durkheim's view, see Giddens (1971c).



the period, for example, Marx ("radicalism") and Mill and Spencer ("liberalism"). But as I seek to indicate below, if any such general labels are to be attached to them, Durkheim's writings are distinctively connected to "liberalism"—although not of the utilitarian variety—rather than to "conservatism." One of the main shortcomings of the thesis of the conservative origins of sociology is that "conservatism" means different things in different countries and at different periods, as do "liberalism" and "radicalism." Thus it would seem reasonable to hold that *one* of the traditions that shaped Marx's own writings was a conservative one, namely, Hegel's philosophy. Similarly, in the accounts of various interpreters, Weber is held to have been a conservative—in the strong sense—because of an irrationalism that ties his thought to that of the ideologist of National Socialism Carl Schmitt (see in particular Lukács 1955 and Mommsen 1959). In actual fact, I do not think this view to be any less partial and inaccurate than that which links Durkheim's writings in a privileged way to the "counter-reaction"; irrationalism, particularly in the form of Nietzsche's ideas, is only one component of Weber's intellectual inheritance and of his thought, and one which he tried to synthesize with other, quite different, elements and thereby to transcend (Giddens [1972*c*] argues the case for this in detail).

#### THE MYTH OF SCHISM

Although it is perhaps even more pervasive than the others, the myth of schism can be dealt with more briefly, because in some part it depends upon them. It was invented, to put the matter crudely, by Dahrendorf, looking back over his shoulder to Parsons's "problem of order." According to Dahrendorf (1959), not one, but two resolutions of the problem of order can be found in social theory. One is that which Parsons extracted from Durkheim, stressing the significance of consensus; the other, most clearly expressed by Marx, resolves the problem of order through the coercive control that a minority can exert over the rest of society.<sup>10</sup> Dahrendorf compares Marx directly to Parsons; others, however, have looked back to Durkheim as the main founder of "order theory." Horton (1964, 1966), for example, traces the differences between Marx and Durkheim to divergent conceptions of man in a state of nature, linking Marx to Rousseau, and Durkheim once more to Hobbes. According to this sort of view, while in the first such conception the evils in the human condition stem from the repressive effects of man's incorporation in society—from which he must be liberated—in the second they originate

<sup>10</sup> For a more recent and more interesting version of the idea of schism, see Dawe (1970).

in the opposite state of affairs: a lack of adequate social or moral regulation.

But it is easy to show how misleading this conception is. One cannot make sense of Marx's writings, even his early ones, by supposing that he was thinking in terms of an abstract contrast between "man in nature" (nonalienated, free) and "man in society" (alienated, unfree). Marx, like Durkheim, dismissed this as a residue of utilitarianism from the outset; both saw that the freeing of man from the limitations of his bondage to nature and from his own self-ignorance is a product of social development. Human faculties are both produced and sustained by society. Alienation is maximized by the specific mechanisms of the capitalist mode of production and is transcended, neither by the destruction of society nor by reversion to a more primitive way of life but by the transformation of society itself. Durkheim's vision was undoubtedly at odds with Marx's; the discrepancies do not derive, however, from two different versions of man in a state of nature but are anchored in divergent analyses of the development of a definite *form* of society due to the rise of industrial capitalism in Europe. For Durkheim, man in a state of nature would not be anomic because his needs, like those of animals, would then be wholly organic, and such needs are adjusted to fixed levels of satiation. It is precisely because most of man's needs are socially created that their limits, or their definition as bounded needs, must also be set by society. The correlate of this, obviously, is that the concept of anomie can only be properly understood, as Durkheim intended it to be, in the context of the destruction of traditional society and the emergence of moral individualism (see Giddens 1971a, pp. 224-32).

#### SOME IMPLICATIONS

It is time to take stock. In pouring cold water on the myths of the problem of order, conservatism, and schism, I have talked mainly of Durkheim. The analysis could readily be extended to other authors, however, and I want to deny that these ideas illuminate the development of European social theory in the 19th and early 20th centuries. On the contrary, I seek to indicate that the false images of the past which they have fostered have had an unhappy influence on the contemporary debate about the present concerns and aims of sociology (and the social sciences as a whole). There can be few who do not have a sense of unease about the current condition of social theory, and it is not hard to see that the social sciences today stand at a crossroads; the difficulty is to see which path or paths to take, amid the welter of apparently clashing theoretical perspectives that have suddenly sprung into prominence. I accept that we are today at an important stage of transition in social theory—our

own great divide, as it were. Within the confines of such a paper as this, it is not possible to draw up a detailed proposal about the likely or proper future orientations of sociology; but undermining the myths of the past can help to illuminate some of the major tasks of today.

Broadly speaking, within "academic sociology," as differentiated from "Marxism," we can distinguish at least three responses to the current *malaise* of social theory: (1) A resurgent critique of positivism in the social sciences and an attempt to rework their foundations so as to escape from its toils; (2) the argument that sociology is tied to ideologies which legitimate the status quo, and hence a call for a new *radical sociology*; (3) the thesis that in the schism between "order" and "conflict theory," the former has won out, and hence a demand for new attempts to develop conflict theory in a more adequate fashion.

I do not deny that certain advocates of one or another of these directions in social theory may have made valuable contributions. I do claim, however, that some recent versions—particularly within American sociology as distinct from European social theory and philosophy—are in some part tied to the three myths criticized above and share their inadequacies. I shall first discuss the latter two of the trends I have mentioned above, which I shall connect to what I want to call *the theory of industrial society*, and shall revert later to the problem of "positivism," which I shall connect to the myth of the great divide in the context of the *epistemological status of social theory*.

#### THE THEORY OF INDUSTRIAL SOCIETY

I think it would be true to say that the myth of schism was originally fostered by critics whose attentions were directed at Parsons's mature theories, as set out principally in *The Social System*. These critics were mainly European (Dahrendorf, Lockwood, and Rex); although they themselves were not Marxists, they sought to complement Parsons's ideas with others supposedly drawn from Marxist thought. Even though they rejected the problem of order as *the* problem of sociology, they tended to accept it in a relatively unexamined way as a fundamental basis for social theory, claiming that it should be complemented with notions of coercion, power, and conflict (see Lockwood 1956, 1964 and Rex 1961). Now to deny that the idea of schism is of much value in illuminating the past development of social theory is not the same as showing that the "order" versus "conflict" dichotomy is not a useful way of formulating the tasks of social theory today (or that the idea, as a logical extension, that the dichotomy should be overcome by "combining" or "integrating" the two in some way is of no value). But I wish to make the extension and to claim that such is the case. The myth of schism not only rests upon

misleading interpretations of the past but also is a wholly inadequate way of conceiving our present tasks. This is so for two reasons. First, it perpetuates an ambiguity in the idea of the problem of order itself—a dual meaning which Parsons himself pointed out when he first introduced the notion in *The Structure of Social Action* but which, because of the standpoint he wished to elaborate there, he treated as of no significance. “Order” *can* refer, Parsons pointed out initially, to the antithesis of “randomness or chance,” where “chance or randomness is the name for that which is incomprehensible, not capable of intelligible analysis”—a very general sense of the term indeed. In Parsons’s formulation of the “Hobbesian problem of order,” on the other hand, the term “means that process takes place in conformity with the paths laid down in the normative system” (Parsons 1937, p. 91). By suggesting subsequently, however, that for purposes of social theory these two formulations of order may be treated as one and the same, Parsons was able to develop the view that the “Hobbesian problem” is the generic way in which “the problem of order” *has* to be presented in sociology. But this second sense of “order,” normative integration or consensus, is a very special use of the term; and it does indeed appear the contrary of “conflict,” “coercion,” etc. We have to recognize, in other words, that while, in the first, very general, sense no one could deny that the task of social theory is certainly to account for “order,” the Parsonian formulation is *one specific approach* to this—and it is one that can be questioned in a much more profound way than is suggested by the critics mentioned above.

Second, the idea of schism is a crude and unsatisfactory way of representing the issues separating “structural-functionalism” and “Marxism”; the views supposed to be derived from the latter (stressing the primacy of “conflict,” “coercion,” and “change”) are purely formal and actually have no particular connection with Marxism at all. Thus it is quite mistaken to suggest that Marx was unconcerned with normative “consensus,” although of course he disliked that specific term. “Common values” appear in Marxian theory in the guise of “ideology,” and what differentiates the latter concept from the former cannot be understood without reference to other concepts integral to Marxism, namely, those of modes of production and class interests. It is interesting to note that, although it originated in the writings of European authors, the idea of schism seems to have been more influential in American sociology subsequently than in Europe. One reason may be that, even though the bearing of conflict theory on Marxism is minimal, it has helped to supply in a somewhat covert way what is absent from the American intellectual scene but strongly developed in Europe: a vital and sophisticated tradition of Marxist thought itself.

The idea of schism is a sterile one and has to be abandoned. But because the call for a “radical sociology” is tied both to it and to the myth

of conservatism, it is appropriate to subject it to brief scrutiny. It is easy to see that, just as the notion that the chief focus of social theory should be the "problem of order" calls forth the demand that this focus should be complemented by an analysis bringing conflict, coercion, and change to the forefront, so the view that the roots of modern sociology are bound up in some special sense with conservatism tends to call forth the response that the conservative bias needs to be complemented by a "radical" one. Now it may well be the case that some schools of social thought show elements of conservatism in either the weak or the strong sense (although I have already indicated the difficulties involved in the use of blanket terms like "conservatism" or "radicalism"). But this type of argument is quite different from showing that sociology is in some *intrinsic* way bound up with conservative views. Even if the latter view could be shown to be plausible, which I do not believe it can, it would still leave the epistemological basis of "radical sociology" obscure. Marxism itself has always had trouble with its own epistemological status, that is, to what extent it is a neutral science and to what extent it is a critical theory linked to the interests of the labor movement, and those difficulties are surely only compounded by the diffusely expressed ideals of "radical sociology." I do not deny that social theory is linked in subtle and ramified ways to criticism, but only reject that sort of formulation of "radical sociology" which I consider tied to the myths I have sought to undermine (see Lindenfeld 1973).

I can now move to the main point of this section: the acceptance of these myths of the past has generated a series of controversies, involving attacks upon "structural-functionalism," which have concentrated almost solely upon its abstract or epistemological shortcomings. I refer to these again below. What I wish to show at present is that, since the debate has concentrated upon these issues, it has almost completely ignored what has been the substantive correlate of "structural-functionalism": *the theory of industrial society*. This type of theory is, I think, expressed in the writings of Parsons himself, but the view is also broadly shared by authors as diverse otherwise as Dahrendorf, Aron, and Clark Kerr.

Before I sketch in what I mean, some qualifications are once more in order. I do not wish to say that, even among non-Marxist authors, the ideas I describe immediately below have been without their critics, or that the alternative approach outlined subsequently has not already been partially anticipated by others. I do, however, want to suggest that the critics of the theory of industrial society have neither identified it exactly as I do nor connected the elements of their critiques to an alternative program.

The theory of industrial society runs roughly as follows: The fundamental contrast in the modern world, it is held, is between traditional,

agrarian society, normally based upon the dominance of land-owning elites, sanctioned by religion, though in reality often deriving from military power and coordinated within an authoritarian state; and industrial, urban society, fluid and "meritocratic" in its structure, characterized by a diffusion of power among competitive elites, in which social solidarity is based upon secular exchange transactions rather than upon religious-ethics or coercive military power, and in which government is transformed into a mass democratic state. The theory of industrial society recognizes the phenomenon of class conflict but holds that it is characteristic of the *transitional* phase in the emergence of industrialism out of traditional society and that it becomes transcended (read "regulated" or "institutionalized") when the industrial order reaches maturity. In some versions—including the original Saint-Simonian one—it is held that the very concept of "class" loses its relevance once the transition to industrialism has been achieved. Further, an end of class conflict in the contemporary era means an end of ideology, save in a few industrialized countries, such as France or Italy, where the continuing existence of an archaic, peasant sector means that the old class conflicts and ideological movements have not yet dropped away. Conceptually, the theory of industrial society involves a polar typology of forms of societal organization made familiar under a variety of names: "status" versus "contract," "mechanical" versus "organic" solidarity, "*Gemeinschaft*" versus "*Gesellschaft*," and so forth.

The theory of industrial society, as it has come down to us today, must be *abandoned*, or at least *dismantled* and its assumptions and premises subjected to scrutiny. Insofar as we apply it, in some guise or another, to the patterns of development of the industrialized societies in the present, we are operating within the sorts of assumptions made by most of those in the classic tradition of social theory when they sought to encompass theoretically the encounter of the post-feudal world with the coruscating influences of political democracy, urbanism, and industrialization. But some or most of these assumptions are obsolete in an era when the main "internal" divisions and strains in the advanced societies are no longer, as in the 19th and early 20th centuries, based upon the tensions between urban-industrial centers and the still strong centrifugal pull of a rural hinterland. Moreover, and just as important, the theory of industrial society is time-bound within certain characteristic intellectual biases of 19th-century social thought. The most important of these is an *anti-political* bias (see Wolin 1960). Throughout the 19th century one can trace the imprint of the view, or the covert assumption, that the State is subordinate to society, and that consequently politics can be explained, or, more accurately, explained away, by reference to more deeply layered social phenomena. This, to borrow one of Marx's phrases, was the "illu-

sion of the epoch," reflecting an optimism about the pacific and consensual implications of industrialism, as contrasted with "military" feudalism, questioned only by the few (including, most notably, Max Weber)—and shared in no small degree by Marx himself. The affinities between Marxism and orthodox sociology on this point have been obscured by the tendency to compare them on the abstract level of "conflict" and "order" theory. The threefold scheme of feudalism-capitalism-socialism certainly differs in a fundamental way from the traditional society-industrial society dichotomy, which treats capitalism not as a distinctive type of society but, for reasons already mentioned, as merely a transitional phase between the two main types (thereby precluding the possibility of the transformation of society through socialism, which is treated as of the past rather than of the future). But this should not be allowed to divert attention from the fact that in Marx's writings, as in the theory of industrial society, there is only a rudimentary and highly inadequate theory of the State, no theory of military power, and no anticipation of the resurgent nationalism which, not many years after Marx's death, was to ruin the hopes of socialists for an international socialist commonwealth.

From the assumption of the impotence of politics, shared by the theory of industrial society and by Marxism, other assumptions flow which must be questioned radically, as indeed they have been by a diversity of authors, in spite of whom their influence remains dominant. These assumptions are:

1. Social development or change can be conceived of above all as the unfolding of endogenous influences within a given society (or, more often, a type of society); external factors are then treated merely as an environment to which the society "adapts." But society has *never* been the isolated, "internally unfolding" system which this abstract model implies. This lesson should hardly need teaching in the contemporary world, with its intimate and intricate interdependencies and tensions spanning the globe. Were it not for the dominance of the endogenous model in sociology, one would not need to emphasize the extent to which politico-military power has shaped the character of the advanced societies. Successive world wars have brought about what internal industrial development failed to achieve in Germany and Japan—the disintegration of the hegemony of traditional land-owning elites. They have also provided the theater for the processes of political change which created state socialism, first in the Soviet Union, and then in the other societies of Eastern Europe.

2. The characteristic nature of any society is primarily (read "ultimately") governed by its level of technological or economic development; specifically, in the theory of industrial society, by the level of maturity of industrialization.

3. Consequently, the most economically advanced society (however



defined) in the world at any one point in time shows to other societies an image of their own future. As Marx wrote to those of his countrymen who might doubt that the analysis of *Capital* might apply to them, based as it was upon the most industrially advanced society of the time, Britain: "*De te fabula narratur!*" (it is of you that the story is told!). In the closing part of the 20th century, it might appear somewhat curious to take Britain as offering to the industrialized world an image of its future. But the underlying idea is alive and well: today it is trends in the United States which are most often taken—by non-Marxist thinkers, however—as demonstrating the future in the present for the rest of the world.

A breaking away from these stale ideas, the residue of the 19th century, offers prospects of exciting new perspectives and constitutes one of the immanent tasks of social theory in the present day. The need for substantial rethinking is, I think, evident in the rash of speculative ideas suggesting that we are in the throes of a major process of social transformation in the industrialized world: theories of "post-industrial," "post-modern," "technotronic" society, and so forth, abound. By and large, however, such theories continue the assumptions of previous times, holding, for instance, that "industrial society" is in the process of being superseded by "post-industrial society" (a process which, it is suggested, has proceeded very far only in the United States!). I wish to propose that our rethinking must be more profound and must break with the covert assumptions I have previously mentioned. This, I believe, implies a whole new theoretical and research program for sociology, informed by the following presuppositions:

1. The differentiation between sociology (as the study of social structure) and political science (as the study of government or political power) which has grown up over the years and become institutionally sanctified should be abandoned. It should be one of the major tasks of sociology to create a theory of the modern State and to explore its significance for problems of social theory in general.

2. Sociology should come to terms theoretically with the unitary yet diverse international community which is a "global community" in a literal sense: a world in which the industrial and political transformations of 19th-century Europe have become transferred to the international plane in the confrontation of rich and poor nations.

3. We should take seriously and explore the possibilities inherent in the idea that there are differing "paths" of development among the industrialized countries which cannot be squeezed between the confines of the old theory of industrial society. It has been shown and is generally recognized that there are differing paths to industrialization. In addition, however, these possibly establish differing, chronic patterns of industrial and political organization within the general type of "industrial society"

(I have tried to develop this line of reasoning in Giddens [1973]). We should neither leave the exploration of *differences* between societies to the historian nor merely explain them away by some idea of developmental lag.

4. We should abandon the practice, which would in any case scarcely be defended by anyone in principle, of constructing theories of development on the basis of single cases (Britain in the 19th century, the United States in the 20th). This is a clarion call for a *revitalized comparative sociology of the advanced societies*.

#### THE EPISTEMOLOGICAL STATUS OF SOCIAL THEORY

There is an apparent contradiction in what I have said so far. Although I have accentuated that we are in the middle of a major phase of transition in social theory, I have also argued that the idea of a "great divide" in the development of social thought in the 19th century is a myth. In concluding, therefore, it is necessary to clarify the argument. To do so, however, I must first examine the principal versions of the great divide as they are conventionally advanced; I again refer principally to Durkheim and secondarily to Weber, since no other thinkers of the 1890–1920 period have exerted a comparable influence upon the later development of sociology.

We might well have some initial skepticism about the notion of the great divide if we consider the frequency with which the claim has been advanced in the past that in the study of society science has finally triumphed over philosophy. After all, Saint-Simon made this claim of his works as compared with those of the earlier 18th-century philosophers; Comte and Marx made the same claim in relation to Saint-Simon; Durkheim and Weber made it concerning Comte and Marx; and Parsons made it, one might go on to say, concerning Durkheim, Weber, and their generation! But let us look again briefly at Durkheim's writings. Durkheim is frequently seen, especially by American sociologists, as the leading figure involved in laying the foundations of empirical social science, the first author to apply systematic empirical method to definite sociological issues. This is held to be particularly manifest in *Suicide*, which is often regarded as the first statistical and empirical monograph of its kind to be published.<sup>11</sup> But such a view is simply wrong, and is held in ignorance of the prior history of empirical research, in this area especially, but in other areas too, in the 19th century. The idea of developing a "social physics" (Comte's term also, until he coined the neologism "sociology") involving the sys-

<sup>11</sup> Thus Henry and Short (1957, p. 58) write: "Sociological study of suicide began in a systematic fashion with the publication of Émile Durkheim's *Le Suicide* in 1897. Durkheim's was the first theoretical and empirical exploration of the persistent variations of suicide in relation to sociological variables."

tematic use of "moral statistics" in order to study social life in a supposedly "objective" fashion dates back at least as far as Quetelet. Most of the generalizations, in fact, whereby Durkheim sought to relate variations in suicide rates to social factors were in no way original, nor was there anything particularly novel in his statistical methods (see Giddens [1964] for a fuller discussion). The distinctive character of Durkheim's work, in other words, did not lie in his method or materials but in his theories; and these were worked out within the context of, and can only be fully evaluated against the background of, the broad spectrum of issues which occupied him in *The Division of Labour* and other writings.

Now it is true that, in his methodological writings, Durkheim often emphasized the slow and partial way in which scientific progress comes about; and his efforts to define precisely the scope of the subject matter of sociology had as their object the achievement of the break with philosophy that writers such as Comte and Spencer had advocated but, as he saw it, had failed to bring about. But we can no more accept Durkheim's programmatic statements at their face value than he did those of the authors he took to task. We might admit that *Suicide* conforms to the methodological prescription that sociology should concern itself with restricted, clearly delimited problems; we should perhaps have more difficulty in reconciling this with the far-reaching claims made in *The Elementary Forms*, even though that work is based upon an intensive study of one particular form of "religion," Australian totemism. But the distance between methodological prescription and the themes actually developed by Durkheim surely becomes embarrassingly wide when we consider *The Division of Labour*. If it is not actually a philosophy of history, it is nonetheless of a sweeping and all-embracing character that is by no means alien to the sort of evolutionary schemes produced by previous 19th-century thinkers. Much of what Durkheim wrote in his work as a whole, in fact, hovers over that ill-defined borderline between moral philosophy and social theory. To be sure, he tried to show that age-old philosophical questions could be seen in a new light and thereby transformed; but this, after all, is no more than was claimed by many of his predecessors, including both Comte and Marx.

I shall not deal in detail with the technically elaborate version of the great divide set out in Parsons's *The Structure of Social Action*; some of my earlier comments obviously bear directly upon it. The Parsonian account of the unacknowledged "convergence" of ideas in the thought of Durkheim and Weber (and others whose writings are discussed in the book) has little plausibility, if at any rate it is read as any sort of historical interpretation rather than as a documentation of Parsons's own formulae for the future development of social theory. Durkheim's methodological ideas, as I have already mentioned, are in direct line of

descent from the *Positive Philosophy*, however critical he was of Comte in other ways. Weber had no such immediately available tradition and would certainly have rejected much of it, as he did the views of Menger within economic theory; his methodological position represents an uneasy and brittle synthesis of the sort of views espoused by the latter and the anti-generalism of the Historical School. For Weber, sociology always remained in an important sense the handmaiden of historical analysis. The Durkheimian version of sociological method would have been abhorrent to Weber, and since we can be fairly sure that Weber was well acquainted with the works of Durkheim and some of his prominent disciples, it is reasonable to suspect that Weber's tirade against the use of "holistic" concepts in social analysis was directed in some part against the *Année sociologique* school, although no specific reference is made to that school. In order to explain the divergencies as well as the parallels between the writings of Durkheim and Weber, we have to look at the socio-political background of their writings, conspicuously absent from *The Structure of Social Action*, but figuring prominently in Marxist or Marxist-inspired versions of the great divide, to which I now turn.

The cruder variants of the latter are scarcely worth bothering with. The dismissal of the writings of the 1890–1920 generation of thinkers as merely an "ideological defence of bourgeois society" is inconsistent with Marx's own method: for him, if bourgeois political economy was "ideological," it nonetheless contained a good deal that was valid, which he wrote into *Capital* and made the cornerstone of his own economic theories. But that version of the thesis which sees "sociology"—in the shape of the writings of Durkheim, Weber, and their generation—as having been formed out of a massive confrontation with Marxism is less easily shrugged aside. Thus Zeitlin has lodged the claim that "the outstanding sociologists of the late 19th and early 20th centuries developed their theories by taking account of, and coming to terms with the intellectual challenge of Marxism." Some (like Weber) "adopted a reconstructed or revised version of 'Marxism,'" while others (like Durkheim) "sought to mediate between Marxism and other systems of thought" (Zeitlin 1968, p. 321). I think this claim is wrong: first, it was not just Marxism which played a key role in this respect but socialism more generally, both reformist and revolutionary; second, Marxism was influential not just as an "intellectual challenge" but also as an active political challenge in the form of the rise of militant labor movements toward the end of the 19th century; and third, there was another fundamental intellectual and political challenge which occupied Durkheim, Weber, and others of their time—that deriving from ultranationalistic conservatism.

By means of the third point, we can start to sort out the elements of validity in the myth of the conservative origins of sociology. The "con-

servatism" of Bonald and his contemporaries in France was first and foremost a response to the aftermath of the events of 1789 and the political philosophy that had inspired them. In France, throughout the 19th century, including the period at which Durkheim came to maturity, social thought continued to be dominated by the legacy of 1789, which was manifest in a succession of revolutionary outbreaks culminating in the Paris Commune almost 100 years later. Now the 1789 Revolution sent shock waves through the complacency of ruling groups in Germany and Britain, and provided the backdrop to Hegel's philosophy in the former country. But although the fear of revolution (later, in Germany at least, concentrated against the specter of Marxism) continued to haunt the dominant elites in those countries for decades, other trends of development separated them quite decisively from the French experience. In Britain, the burgeoning of industrialism took place within the context of a mutual accommodation and interpenetration of landed aristocracy and ascendant commercial and industrial leaders that was unmatched elsewhere. This relatively even tenor of development, disturbed only briefly by Chartism, produced a society which gave rise neither to a large-scale socialist movement of a revolutionary kind nor to its counterpart, an aggressive theocratic or irrationalist conservatism. Nor, significantly, did that society produce a global sociology comparable to that of Durkheim, Weber, and their contemporaries (see Anderson 1969). Spencer's formulation of the principles of sociology did not break significantly with utilitarianism, and the latter remained the dominant form of social theory in Britain throughout the 19th century. But, even in the guise of political economy, utilitarianism never enjoyed a similar preeminence in France and Germany. In the former it was overshadowed by the writings of the 18th-century *philosophes* and the reaction to them. In Germany the strongly historical and speculative bent in social philosophy and economics blunted its impact. "Conservatism" in Germany meant primarily a nostalgic and romantic attachment to an idealized village community; its French counterpart, by contrast, was always linked to Catholicism and to the claims of embattled but militantly tenacious landowners, rentiers, and independent peasantry. While for German thinkers of Max Weber's generation the overwhelming problem was that of the antecedents and consequences of capitalism (analyzed above all in terms of the destruction of traditionalism by technical rationalization), in France the comparable debate centered upon the fate of the ideals of individualism for which the revolution had been fought, in the face of the continuing assaults of the Catholic hierarchy.

Both conservatism and socialism thus figure in the political and intellectual backgrounds of both Durkheim and Weber. In this regard, the work of each is an attempt *to rethink the foundations of liberalism in conditions in which liberal individualism and its base in social theory,*

*namely, utilitarian philosophy, developed in the British situation, were manifestly inappropriate.* But this very task helped to distance the main themes of their writings from one another. Weber worked against the backdrop not of a successful revolution (1789) but of a failed one (1848) and in the shadow of Bismarck's unification of the German state through military triumph. The rapid period of industrialization from the top which ensued took place in social and political circumstances very different from those in France. Durkheim's *Division of Labour* and the theory of the state which he later elaborated were directed toward resolving the "legacy of the Revolution" as he saw it: the distance between the ideals of freedom and equality heralded in 1789, and the reality of social stagnation and resistance to change which he thought were epitomized by the disasters of the war of 1870—the very war which sealed German unity—and by the repression of the Commune. Durkheim, like Weber, sought to borrow elements from socialism (and Marxian socialism was more prominent in Weber's intellectual horizon during the formative years of his career than in Durkheim's) and conservatism, but in order to transcend both.

In making these points, I do not want to fall into the sort of view, which I have already rejected, that sees the validity or usefulness of social theories as dependent upon the context in which they are produced, but simply to claim that such analysis helps us to understand more fully the theories and their distinctive qualities. The writings of the 1890–1920 generation *did* differ from much of what went before; but the elements which in a very general way exemplify the contrast do not accord either with Parsons's account or with the more naïve versions of the great divide between social philosophy and social science. What, then, does distinguish the writings of the above-mentioned generation from what went before?

First, as I have already tried to show, an attempt to rethink the foundations of liberalism in the face of the twin challenge of revolutionary socialism and conservatism. Second, the successful beginning of sociology as an accepted subject in the university curriculum (one should remember, however, that Durkheim first came to Paris as a professor of education and that Weber never occupied a chair whose title included "sociology"). Third, a greatly heightened sensitivity to the study of other cultures and a breaking away from European ethnocentrism. Fourth, and not unconnected with the third point, a general resurgence of concern with the sources of unreason in human social existence (see Hughes 1958).

These factors helped to give the writings of Durkheim, Weber, and some of their more prominent contemporaries an intellectual power well beyond those of most of their predecessors in social theory. I do not deny that such intellectual advance is possible; but I do think it pressing to spell out some of the implications of acceptance of the myth of the great divide in the writings of some of the recent critics of positivism in sociol-

ogy. Such a task cannot be successfully executed unless it is, first, philosophically informed and, second, aimed at the right polemical foils. Neither condition is adequately met in much recent writing on these matters, especially by American authors. This is in some part, I think, precisely because they themselves attribute a fairly naive version of the great divide to their opponents; and when they knock it down, they are merely crushing men of straw.<sup>12</sup> The problem of positivism in social science is a complex one, and my remarks here must be cursory. The term "positivism" has become in most quarters more of a derogatory epithet than one with a precise reference (see Giddens 1974). I take it, however, to have at least two connotations: the empiricist notion that there exists a neutral or theory-free observation language, in terms of which observations of objects or events can be made and generalizations inductively established, and the thesis that such a model, derived initially from natural science, is appropriate for the study of social phenomena, so that we may consider sociology a "natural science of society." A good deal of social thought, especially in the United States, has been dominated by this sort of view; and it is, I consider, a view which has to be rejected.

Rejection of it is certainly going to involve a fundamental reappraisal of social theory, its pretensions and achievements. Social science as we know it today, I believe, was brought into being not primarily by the 1890-1920 generation but by that earlier generation of 19th-century thinkers among whom Marx, Comte, and Spencer may be distinguished as most prominent. They, more than anyone else, gave modern social theory its impetus under the impact of the rise of physical science. Sociology was created as an apparently direct extension of the realm of natural science which, beginning with mathematics and classical dynamics, had marched through the theory of evolution up to the gates of the human world itself; Comte gave this vision its most immediate expression in his formula of the historical sequence of scientific development, whereby science begins as applied to phenomena most remote from man's involvement and control but is brought ever nearer to man himself, culminating in sociology, the science of human social conduct. The modern philosophy of science, stimulated by a revolution in that bastion of classical physics, Newtonian mechanics, has radically overturned the view of natural science which inspired the rise of sociology. The implications for social science have yet to be worked out. They constitute a background to the recent attempts of English-speaking authors to follow Scheler, Schutz, and Sartre in trying to marry ideas drawn from phenomenology to those current in the more orthodox traditions of sociology, together with a resurgence of

<sup>12</sup> See Jack D. Douglas's account of "absolutist sociologies" in Douglas (1971, pp. 3 ff.), which completely ignores the works of 19th-century European thinkers whose ideas contrast with those of Durkheim.



interest in critical theory. From this seeming turmoil, I think, new forms of social theory will be born. But what is crucial is that they should not be allowed to foster a *retreat from institutional analysis*; that is to say, the abandonment of the classic concern of social theory with issues of macroscopic social organization and social change. For herein lies the greatest challenge to those who would undertake a rethinking of the major tasks of social theory today: to break with the classical traditions in a dual way, substantively on the level of the theory of industrial society, and abstractly on the level of epistemology.

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# Social Structure from Multiple Networks.

## I. Blockmodels of Roles and Positions<sup>1</sup>

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Networks of several distinct types of social tie are aggregated by a dual model that partitions a population while simultaneously identifying patterns of relations. Concepts and algorithms are demonstrated in five case studies involving up to 100 persons and up to eight types of tie, over as many as 15 time periods. In each case the model identifies a concrete social structure. Role and position concepts are then identified and interpreted in terms of these new models of concrete social structure. Part II, to be published in the May issue of this *Journal* (Boorman and White 1976), will show how the operational meaning of role structures in small populations can be generated from the sociometric blockmodels of Part I.

During the past decade, the network metaphor has become increasingly popular with social scientists;<sup>2</sup> it has even penetrated the conservative

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<sup>2</sup> Network metaphors date back at least to Simmel (1950, 1955; first published in 1908) and the so-called formal school of German sociologists. Simmel emphasized the ubiquity of social networks based on "the actual similarity of [individuals'] talents, inclinations, activities, and so on" (1955, p. 128) and which cross-cut the categorical attributes of persons. Von Wiese, strongly influenced by Simmel, stressed the multiplicity of types of social ties and the analytic desirability of reducing network structures. If the "constantly flowing stream of interhuman activity" were halted in its course for one moment, von Wiese (1941, pp. 29-30) suggested, we would observe

precincts of economics (Boorman 1975; Marschak and Radner 1972; Schelling 1971; see also Leijonhufvud 1968). Sociologists' and anthropologists' attempts to develop the metaphor into operational concepts have taken two directions. One has emphasized the paths or "threads" in a single network: the manner in which long chains of contact wind their way through large social systems (Milgram 1967; Pool and Kochen 1958; Rapoport 1963; Coleman 1964; Hunter and Shotland 1974; White 1970*a*, 1970*b*; Lee 1969; Granovetter 1973, 1974). The second has emphasized the "knittedness" of interconnections within a network and the overlaps between multiple (many-stranded) types of networks for a given population (typically small; see Theoretical Background section, below). Our operational concepts follow the second tradition but are consistent with the first.

After demonstrating the utility of these concepts as applied to five case studies, we redefine the classic concepts of role and position so that they apply to concrete, observable interactions, ordered by a new framework. We take as given the incidence of each of several distinct types of tie across all pairs in a population (see for example figs. 1 and 3 below). Ties of each given type are treated as a separate entity (a matrix). Each is a separate network to be contrasted with other such networks, rather than merged with them to form a complex bond between each pair of actors. This analytic segregation of network types is basic to our framework. From it, aggregation emerges as a concept with dual aspects: actors are partitioned into structurally equivalent sets within each network; simultaneously, though, networks are mapped into a set of images that can be specifically interpreted for specific populations. The resulting "blockmodel" is a view of social structure obtained directly from aggregation of the relational data without imposing any *a priori* categories or attributes for actors. Our fundamental argument is that the enormous variety of concrete social structures is reflected in the variety of possible blockmodels; furthermore, blockmodels provide tools for ordering this diversity.

The essential phenomenon portrayed in network imagery, we argue, is the *absence* of connections between named individuals. The logical symmetry between ties that are "present" and ties that are "absent" (i.e., all others) has encouraged proponents of graph theory to overlook the

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"an apparently impenetrable network of lines between men. There is not only a line connecting A with B, and B with C, etc., but C is directly connected with A, and, moreover, A, B, and C are enclosed within a circle. Not only is there *one* line connecting A with B, and not only *one* circle in which they are both enclosed, but there are many connecting lines. . . . A static analysis of the sphere of the interhuman will . . . consist in the dismemberment and reconstruction of this system of relations. Outside this network, above and below it, there can be nothing that is social, unless we leave the plane of empirical observation."

social asymmetry that exists between social action and its complement (Harary, Norman, and Cartwright 1965; cf. Simmel 1950, pp. 311-16).<sup>3</sup>

This paper and its forthcoming companion, Part II, present no models of processes over time; there are neither predictions of other behavior nor explications of a stochastic process of tie formation and dissolution that would sustain an observed blockmodel. In this paper the arguments for a blockmodel as a picture of social structure are specific to the context of, and the data available for, each case study.<sup>4</sup> Yet blockmodels provide a natural framework for discussing various types of structural change: numerous changes in individual ties can still be consistent with an unchanged structural pattern; changes in the "circulation" of actors among the structurally equivalent sets can still reflect the same structural pattern for a given network, and changes in network patterns can occur and yet leave sets of actors unchanged.

The next section of this paper examines the broad theoretical underpinnings of our research. The second major section presents definitions and the methods of analysis. The third section exhibits analyses based on five case studies. The fourth section provides an interpretation of "role" and "position."

#### THEORETICAL BACKGROUND

Insightful expositions of recent work on network interrelations are those by Mitchell (1969, chap. 1) and Barnes (1972). While we use them as central references, we want to state one fundamental disagreement. Both see network analysis to date as, at best, an eclectic bag of techniques (Barnes 1972, p. 3) for studying the details of individuals' variability around some basic ordering by categories and concrete organizations (Mitchell 1969, p. 10). We would like the reader to entertain instead the idea that the presently existing, largely categorical descriptions of social structure have no solid theoretical grounding; furthermore, network concepts may provide the only way to construct a theory of social structure.

Perhaps *the* major thrust of classical social theory was its recognition of the historical dissolution of categorical boundaries for social relations, whether the change was perceived as a transition from status to contract (Maier), from *Gemeinschaft* to *Gesellschaft* (Tönnies), from mechanical

<sup>3</sup> Recognizing that the "holes" in a network may define its structure was a primary substantive motivation for the work reported here. There are obvious analogies with homology theory in algebra (Hilton and Wylie 1960), though the relevant mathematics is quite different.

<sup>4</sup> In addition, White (1974b) has calculated probabilities for the occurrence purely by chance of the simplest blockmodels.

to organic solidarity (Durkheim), from traditional to means-rational orientation (Weber), or from ascribed to achieved status (Linton). In our view, the major problem with postclassical social theory has been that its concepts remain wedded to categorical imagery. All sociologists' discourse rests on primitive terms—"status," "role," "group," "social control," "interaction," and "society" do not begin to exhaust the list—which *require* an aggregation principle in that their referents are aggregates of persons, collectivities, interrelated "positions," or "generalized actors." However, sociologists have been largely content to aggregate in only two ways: either by positing categorical aggregates (e.g., "functional subsystems," "classes") whose relation to concrete social structure has been tenuous; or by cross-tabulating individuals according to their attributes (e.g., lower-middle-class white Protestants who live in inner city areas and vote Democrat). Both methods have "often led to the neglect of social structure and of the relations among individuals" (Coleman 1958).<sup>5</sup> In contrast to the standard wisdom, there is a growing list of empirical findings regarding the effect (and frequency) of "accidents" and "luck" in the actual functioning of societies: the transmission of useful information among scientists (Menzel 1962), the attainment of general economic success (Jencks et al. 1972), and the location of desirable jobs (Granovetter 1974; see also Boorman 1975). These findings force us to ask whether the stuff of social action is, in fact, waiting to be discovered in the network of interstices that exist outside the normative constructs and the attribute breakdowns of our everyday categories.

### Overall Social Structure

Nadel's *The Theory of Social Structure* (1957), one of the few pieces of sustained analytical exegesis in sociology, inspired the work (White 1963; Lorrain and White 1971) from which these papers grew. His focus was the interrelations of roles. In dealing with role "frames" and their interlock,<sup>6</sup> he confronted the interaction of cultural systems and concrete social structure, a topic on which we spend little time. However, we do develop, in a limited context, two of Nadel's most important ideas. First, social structure is regularities in the patterns of relations among concrete entities; it is *not* a harmony among abstract norms and values or a classi-

<sup>5</sup> There are some exceptions to these tendencies, e.g., reference-group theory (Merton 1959, pp. 281–86), and Znaniecki's (1940) embedding of "role" concepts in "social circles"; nevertheless, there is a remarkable lack of attention to aggregation as a central problem for sociological theory. Leijonhufvud's (1968, chap. 3) critique of neoclassical economics for avoiding similar questions is relevant here. See also Green (1964) for a more orthodox review of economic aggregation concepts.

<sup>6</sup> This topic, of course, entails the attendant complexities of interrelating the multiple perspectives of actors in actual societies.

fication of concrete entities by their attributes. Second, to describe social structure, we must aggregate these regularities in a fashion consistent with their inherent nature as networks.

The cultural and social-psychological meanings of actual ties are largely bypassed in the development. We focus instead on interpreting the patterns among types of tie found in blockmodels. Our sole assumption here is that all ties of a given observed type share a common signification (whatever their content may be). From these patterns, we develop below (and in Part II) operational concepts of role and position.<sup>7</sup>

In our view "position," in the concrete sense of office in a formal organization or membership on a committee, is a concept quite independent from "role." The blockmodels of this paper can be said to identify positions, but only in an elementary sense. In Part II we extend the analysis to encompass multiple *egos* and, thence, role *structures*; we hope also that this extension can describe, in the language of Mitchell (1969, pp. 45-49), the existence and interrelations of "institutions."

At best, blockmodels can make only a partial contribution to the analysis of formal organizations as structures of offices. The network metaphor is unavoidable in developing models of formal organization, even of the simplest kind (Williamson 1970, chap. 2). However, fundamentally new developments of the metaphor are needed, such as that proposed by Friedell (1967) and that implied by the argument of Cohen and March (1974).

Analyzing systems of formal organizations will require still further developments of network imagery, and these cannot be divorced from models of elites and the ways in which they may control large social systems through the structure of network access. Recent work on director interlocks (e.g., Levine 1972) and on advisory systems (Mullins 1972), as well as formally analogous models of interlocks in dual individual-position systems (Breiger 1974*b*; Bonacich 1972), may point in the right direction.

One practical reason for the caution of Mitchell and Barnes in using network concepts was the lack of satisfactory methods for aggregating networks among individuals. A related reason was the paucity of research on networks among nodes that represented collectivities and organizations. There are few systematic analyses of networks among such nodes (but see Fortes 1945, Mayer 1960, and Savage and Deutsch 1960); however, engineering and operations research have huge descriptive and normative

<sup>7</sup> Our stress on relationships among patterns suggested to one of our referees an analogy to Lévi-Strauss's work on "meaning." He thereby credited us with too much and too little. We use that term without the rich ethnographic insight of Lévi-Strauss; however, we do discuss the falsifiability of an ideal-type pattern and (White 1974*b*) its null expectation. In our view, the delineation of concrete social structure should be analytically divorced from symbolic and cultural analysis.

literatures on flows within networks (see Ford and Fulkerson 1962) that may prove suggestive (see White 1973).<sup>8</sup>

Both Mitchell and Barnes emphasized "anchored" networks (networks seen from the perspective of a particular member), because they wanted to show how network concepts illuminate the manipulative activities of concrete persons in real situations. Their conceptual approach differs from our observer viewpoint in this paper and the multiple-ego viewpoint of Part II. In particular, they merged different types of tie and inferred a complex, overall quality from the multiplex bond between the anchor person and each of his contacts.<sup>9</sup> In contrast, we argue for the value (from the observer's viewpoint) of treating the network based on each analytically separable type of tie as a separate entity. Furthermore, Mitchell and Barnes paid more attention to the different facets of meaning measured for each type of tie; they also stressed the importance of rich observation by a participant observer as, for example, in Kapferer's work (1972).<sup>10</sup> In contrast, we argue, following Durkheim, that a theory should be developed only in terms of the overall structure that is the context for particular transactions. We cite as evidence Boorman's (1975) model of job information exchange, noting that his results regarding stability and optimality were obtained from postulates of a very simple, overall network structure.<sup>11</sup>

Mitchell and Barnes treated sociometry, especially balance theory, with some disdain.<sup>12</sup> Although many powerful analyses of data have used a variety of sociometric concepts (see, e.g., the excellent survey articles by Glanzer and Glaser [1959, 1961]), many of the data are from experimental "groups" and other populations aggregated within a sociological vacuum. Moreover, with the crucial exception of the analyses by Davis, Holland, and Leinhardt (see Part II), balance theorists have had little

<sup>8</sup> Both this paper and Part II deal wholly with data on individuals, but our motive for developing the methods reported here was partly that we think they will be fruitful for analyzing data on networks among collectivities (see Breiger, Boorman, and Arabie [1975] reanalyzing data of Levine [1972]).

<sup>9</sup> But note the lament of Mitchell's colleague Boissevain (1973, p. xi) that "the problem of handling multiplex or many-stranded relationships remains, in spite of the increasingly sophisticated analytical apparatus provided by network analysis."

<sup>10</sup> Kapferer went even further, attempting to develop an exchange theory for transactions between pairs in a network.

<sup>11</sup> We believe that blockmodels, which represent static structure, will be a useful framework for developing social exchange theory. Ekeh's (1974) recent review of "the two traditions" in social exchange theory urged the importance of interaction between restricted exchange (Homans) and generalized exchange (Lévi-Strauss). Blockmodels seem a natural context for such a merger.

<sup>12</sup> Mitchell (1969, p. 7) ventured so far as to term balance theory, the most interesting analytic development in this tradition (see e.g., Harary et al. [1965, chap. 13]), "a toy of the lecture-room theoretician."

stomach for actual data. Yet we think Mitchell and Barnes too hasty. Our own approach owes much to sociometry, particularly its encouragement of systematic data reports in contrast to rich intuitive observation, and blockmodels include various forms of balance theory as special cases (see Part II).

### Contrasts with Sociometry

Sociometry's most common goal for a single type of tie was the identification of cliques (or similar configurations) of tightly clustered individuals; a secondary goal was chains of connectivity. The clique concept embodies the root idea of aggregation by relations rather than by attributes that is indispensable to blockmodels. Sociometry's other major goal (most notable in balance theory) has been to interpret the interpenetration, or overlap, among different types of tie.

We now draw five contrasts between sociometry and blockmodels. The first two are prompted by the restrictive nature of the clique concept. First, persons not in cliques are usually disregarded (i.e., treated as outside the effective sociometric system). In contrast, blockmodeling requires searching for a complete partition, such that sets of persons can be structurally important regardless of whether the sets resemble cliques. Second, even when (as in MacRae 1960) cliques are defined as we define structurally equivalent sets (i.e., by similarity in ties to third parties rather than by choices of one another), the clique imagery is retained and is often allowed to limit the interpretation. In blockmodels, on the other hand, partitioning of individuals is only one side of a dual problem; the other is to interpret the pattern formed on the one or more networks by the partition.<sup>18</sup>

The third contrast is in use of spatial imagery. Most sociometry deals with only one type of tie, sometimes an overall type constructed from separate kinds of data. Several investigators (e.g., Laumann and Pappi 1973) eschew the crudity of clique description, preferring instead to view the population as embedded in some abstract space (usually Euclidean). Even ordinal measures of similarity between pairs can be converted into quantitative measures of location and distance through some variant of multidimensional scaling (Shepard 1962; Kruskal 1964a, 1964b; McFarland and Brown 1973; Arabie and Boorman 1973; Shepard 1974). Cliques, as well as many other sociometric concepts (e.g., connectivity), can then be expressed in terms of locations and distances within the space. In contrast, blockmodels assume no such spatial embedding. Presumably each

<sup>18</sup> Basic to our work has been our desire to conceptualize many ideal-type patterns, each suggestive of a different form of social organization, and to perform tests that reveal which (of all conceivable patterns) actually *exist* in a population.



network, each distinct quality of tie, requires its own space, whereas blockmodels are able to sidestep this matter. Perhaps a more basic question, though, is whether *any* spatial representation is suitable for a network, since the essential feature of social networks may well be the sharp breaks in patterns—the “holes” in the networks.<sup>14</sup>

The fourth contrast concerns boundaries. Sociometry usually takes as given some split between the population studied and the rest of the world (i.e., it assumes a clearcut enclave). However, Barnes emphasized the artificiality of this presupposition and urged instead a distinction between the finite “reach” of network effects and the notion of a sharp boundary around a particular set of people (1972, p. 16). In two of the case studies below (the biomedical and the Firth-Sterling), we argue that blockmodels apply to networks among people who are embedded in a larger world and who thus comprise an “open” population.

The fifth contrast involves a basic methodological issue. Moreno’s original emphasis on concrete diagrams of ties among individuals was sound. As sociometric analysis “advanced,” though, it became more and more wedded to approximations by indices (of which spatial embeddings and triad inventories [Holland and Leinhardt 1976] are among the most sophisticated). Balance theory initially signaled a reversal of this trend; however, as soon as it became clear that no real data sets were “balanced” in the classical sense, researchers began an unrewarding search for indices of the degree of deviation from classical balance (see, e.g., Flament 1963). We argue, instead, that sociological analysis needs explicit models of the structures in observed populations, *not* measures or statistical indices of deviations from some convenient ideal structure. Blockmodels were developed to meet this need.

## METHODS: PHENOMENOLOGY AND ALGORITHMS

### Structural Equivalence and Blockmodels

Consider ties of one type, from one person to another, arrayed as a square matrix, with a row *and the corresponding column* of the matrix assigned to each person in the population. Create a separate matrix for each type of tie. Figure 1 presents three kinds of tie among biomedical researchers specializing in the neural control of food and water intake (Griffith, Maier, and Miller 1973). An entry of X means that a tie is “present”; a blank space, that a tie is “absent.” The left-hand matrix represents all ties of mutual personal contact. The other two matrices

<sup>14</sup> Breiger et al. (1975) compared blockmodels with multidimensional scaling solutions in considerable detail for several sets of data (using both the MDSAL and INDSCAL algorithms); they argue that there is agreement between results obtained by the various approaches.



represent "unawareness of man or his work," distinguishing pairs of individuals who reciprocate "unawareness" (the right-hand matrix) from pairs in which only one individual indicated an "unawareness" tie (the middle matrix). Only an arbitrarily chosen subset of 28 members of the full sample ( $N = 107$ ) is included.

Blockmodeling begins with weakening and extending the algebraic concept of "structurally equivalent" actors in a network (Lorrain and White 1971). A self-consistent search procedure is used to partition a population into sets of structurally equivalent actors—*blocks*. In each data matrix, we rearrange the row and column of each individual, so that the members of a block are grouped together. We also use the term *block* for a rectangular submatrix in which ties of the given type from members of one block to members of another block are reported. (The context will specify which of the two meanings is intended.) Attention is focused particularly on blocks which have no, or very few, instances of ties: these are termed *zeroblocks*.

Look ahead to figure 3, in which the 28 persons in figure 1 have been partitioned into four blocks. (For example, the first block has five members: individuals numbered 9, 26, 23, 4, 1.) The rows and columns for individuals have been rearranged so that each of the three matrices can be seen as 16 blocks displaying ties from one of the four sets (blocks) of individuals to another. For example, in each matrix of figure 3 the upper left block reports any ties among the first five individuals; adjoining it on the right is the block reporting any ties *from* these five *to* the second set [block] of six individuals; and so on. There are eight zeroblocks in the left matrix, five in the middle one, and four in the right-hand one. The pattern of zeroblocks in this figure is interpreted in the next section, where case studies are discussed.

A *blockmodel* is a hypothesis about a set of data matrices: it specifies for each matrix which blocks will be zeroblocks when some common partition of the population is imposed on all the matrices (as in fig. 3). A blockmodel consists of a square binary matrix, called an *image*, for each type of tie. Each image has a row and a corresponding column for each block (in fig. 3, the top panel of three  $4 \times 4$  matrices shows an image for each type of tie). The ordering of blocks within the blocked matrices is arbitrary, as is the ordering of members within a block.

Five ideas are basic to blockmodels. First, structural equivalence requires that members of the population be partitioned into distinct sets, each treated homogeneously not only in its internal relations but also in its relations to each other such set. Second, the primary indicator of a relation between sets is not the occurrence but the absence of ties between individuals in the sets. Third, many different types of tie are needed to portray the social structure of a population. Fourth, the nature of a type

of tie is inferred from the pattern, in a given population, of all ties of that type.

The fifth idea embodies our basic claim about aggregation. A model of social structure requires specifying, for each pair of sets on each type of tie, whether or not a zeroblock exists. Thus, a given set—a "block" in our terminology—may be debarred, by the model, from ties of one type with several other blocks, and from ties of a second type with a different collection of other blocks, and so on.

### Bonds and Segregation

Suppose the networks for a given population satisfy a particular block-model hypothesis. When these matrices are rearranged and partitioned accordingly, those blocks which are not zeroblocks are usually not completely filled with ties. They have a speckled appearance because choices from members of the row block to members of the column block are interspersed with blanks showing no ties. There are several reasons for this situation. First, if the data are sociometric responses, then the number of responses per person is usually limited to an arbitrary number that is normally insufficient to yield solid blocks of entries. If an observer infers ties (instead of asking the subjects about them), he will be unable to monitor all possible pair interactions continuously; thus he may miss brief occasional contacts that are, in fact, enough to maintain a tie (if not to originate it). At any given time, chance fluctuations (who has been talking to whom, etc.) may determine which particular ties are coded as present.

Second, aside from limitations in data collection, the persons being studied may not be motivated to report all their ties. At some times, they may even wish to conceal some ties from others, or even from themselves—hence also from any investigator. The act of revealing a tie—one's asymmetric contribution to a pair connection—is a tactical decision in an on-going situation.<sup>15</sup>

Third, there is no need for a person to maintain every tie to all individuals who belong to his own or another block, even though the number of ties between (or within) these blocks may be considerable. Maintaining a tie requires time and energy; in addition, it makes a claim on another person's attention. Blockmodeling requires that ties of a given type from any person in one block to any person in another be equivalent in structural significance; however, not everyone need choose to mobilize all such ties all the time (White 1974a).

Finally, the *population* need not be a natural group in mutual face-

<sup>15</sup> This theme is further developed in Schelling (1960). That self-censorship actually occurs is further suggested by the Firth-Sterling Corporation data analyzed below.

to-face contact, all of whose members are automatically acquainted with one another. It may be a contact network in which a particular person may never even have heard of half the others, as in figure 1. Blockmodels are wholly applicable to such cases.

The blockmodel hypothesized for a set of matrices is an interrelated set of inferences from those data to an aggregated pattern of ties among certain sets of persons. The memberships of these sets (the blocks) are influenced by each other through the incidence of ties of every type across the whole population. *Bonds* is the term<sup>16</sup> assigned to those blocks which are not zeroblocks, even though many or most of the entries are blanks.

Sociometric stars and other concepts that try to capture individuals' popularity have no direct analogue in blockmodels.<sup>17</sup> Segregation of choices, as between boys and girls in grammar school classrooms (Bjerkstedt 1956), has often been noticed in sociometric analyses. This phenomenon can be described by zeroblocks in a blockmodel, but has apparently been investigated only for a priori categories of persons, such as male and female. With reference to less extreme forms of segregation, think of zeroblocks (such as those in the mutual contact matrix of fig. 3) as marking the boundaries of choices by subgroups. Because individual popularity depends on the size and composition of the group or unit under consideration, it may be argued that this class of sociometric concepts depends logically on blockmodels (or some closely related apparatus) for delimiting the boundaries of such units. *Within* the top left block of the figure 3 mutual contact matrix, for example, it is apparent that the first individual (#9) is most popular (he is chosen by each of his blockmates) while the second individual (#26) receives fewer choices. This fact is masked, however, in the unpermuted data of figure 1, which shows individual #26 receiving more choices overall than individual #9.

### Images from Reciprocity and Reflexivity

Much of sociometry emphasizes the distinction between a reciprocated tie and an asymmetric tie (Davis 1970). This distinction is used in blockmodeling, but usually between blocks and not between individuals. On the other hand, reflexivity is merely a technical question in sociometry, whereas in blockmodels the existence of diagonal entries for a given image involves a crucial substantive question. In sociometry there are

<sup>16</sup> Breiger et al. (1975) used the term "1-block" instead of bond.

<sup>17</sup> Connectivity in a sociometric graph may depend crucially on a single tie between two individuals and is therefore hard to relate to blockmodel ideas (compare also the concept of a sociometric "bridge" suggested in Granovetter 1973). Holland and Leinhardt (1973) explored the sensitivity of sociometric models to measurement error in sociometry, but they came to unjustifiably pessimistic conclusions.

only four possibilities for any pair of individuals on a given type of tie: reciprocal, null, or one-way (two possibilities). In contrast, blockmodels permit 16 images on two blocks; these are shown, and fixed labels assigned, in figure 2. In sociometry, three of the four possibilities are structurally distinct; in blockmodeling, 10 of the 16 images are distinct (see fig. 2).

In the blockmodels for five of our case studies, images *E* and *V* (and *F* and *W*) occurred frequently and are substantively important. *V* can capture, in a crude  $2 \times 2$  pattern, the structure of a hierarchy as usually idealized: ties of deference extended within each block to one's immediate superiors are also observed from the lower to the higher blocks in the hierarchy. In contrast, image *V* can be seen as aggregate deference accorded *only* by persons in the lower block *only* to persons in the higher block. Call *E* the "hangers-on" pattern. It suggests differential standing, but in a different way. Here the second block is not internally coherent but is part of an overall deference structure. The *E* image suggests a distinction between the center and the periphery.

Images *P* and *N* represent pure reflexivity and pure symmetry, respectively. Sociometric balance theory may be expressed by these two images: positive binding within each of two cliques (reflexivity—*P*) and hostility between the two cliques (symmetry—*N*). Element *C* singles out one clique from the remaining isolated individuals. If the type of tie represented here has negative connotations, then either *C* or *D* shows a concentration of hostility within a subset.

Images *H* and *T* are patterns for perceptions held by members of both blocks: all ties from both blocks go to one block. Images *S* and *G* show people split into a passive block on the one hand and an active block that does not discriminate between itself and the passives in ties of the type under study. The usual forced-choice procedures for gathering sociometric data would preclude discovery of patterns *S* and *G*.

### The Aggregation of Blocks

The image of a blockmodel may be portrayed to any desired degree of refinement by combining blocks to achieve a "coarser" image (one with a smaller number of rows and columns) or by further splitting the existing blocks to achieve a "finer" (larger) image. Any blockmodel on three blocks, for example, can be collapsed formally into a blockmodel on two blocks by combining any two of the blocks. Since the order of blocks is arbitrary, there are  $(2^n - 1)$  ways to collapse an  $n$ -block blockmodel into possible blockmodels on two blocks.<sup>18</sup> Of course, the rule for collapsing

<sup>18</sup> Each of the original data matrices reports information only about ties of that type for all pairs in the population. Permuting the rows (and corresponding columns)

must require that if any one of the refined blocks being combined into a coarse block is a bond, the coarse block must also be a bond. Often two blocks are not sufficient to capture even the gross patterns in a block-model for a case study.<sup>19</sup>

Observe that each bond in the  $C, F$  pair of images for two blocks is also a bond in the  $E, F$  pair: even when the number of blocks is the same, one blockmodel may be a refined (i.e., more demanding) version of another. In principle, one can construct an inclusion lattice<sup>20</sup> of blockmodels, on a given number of types of tie, beginning with those on two blocks and then extending the lattice to three and more blocks. In practice, the possible blockmodels are far too numerous for this to be useful. For example, there are 104 single images with three blocks, which are distinct under permutation of blocks.

Formally, the pair of images  $C, F$  is simply a more demanding version of  $V, F$ , but (as will become apparent) the social structures described have quite different qualities. Blockmodels provide a framework for making substantive judgments and interpretations; they supply a set of formal answers. However, the solution must be proposed, as well as validated, on substantive grounds.

## Two Algorithms

For the cases studied to date, up to half the blocks have been zeroblocks. Intuitively, it is surprising to find any partition of rows and columns for a set of arbitrary matrices which fit a blockmodel containing many zeroblocks, but in principle there could be many. The number of possible partitions is astronomical. G. H. Heil has devised an efficient computer algorithm for constructing all assignments (if any) of men to blocks for which the rearranged data matrices obey the given blockmodel. This algorithm, called BLOCKER, is described in detail elsewhere (Heil and White 1974).

In carrying out BLOCKER, one can identify persons whose assignment to one or more particular blocks in the blockmodel effectively determines the placement of many other persons. Such individuals may be

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does not change the information. After a partition is imposed, the blocked matrix for a given relation contains the same pair data as before. The use of blockmodels can be urged on the purely methodological grounds that they permit flexible aggregation which retains this permutation invariance (see White 1974a).

<sup>19</sup> True for the biomedical and the monastery cases examined below. In contrast, the essential patterns, and thus qualities, in the Newcomb fraternity *can* be described by a pair of images ( $V, F$ ).

<sup>20</sup> Szász (1963) provides relevant lattice-theoretic background. When building such a lattice, the order of blocks is obviously significant in comparing matrices.

	0	0
Z	0	0

	1	0
C	0	0

	0	0
D	0	1

	0	1
X	0	0

	0	0
Y	1	0

	1	0
P	0	1

	0	1
N	1	0

	1	1
G	0	0

	0	0
S	1	1

	1	0
H	1	0

	0	1
T	0	1

	1	1
E	1	0

	0	1
F	1	1

	1	0
V	1	1

	1	1
W	0	1

	1	1
U	1	1

FIG. 2.—The 16 possible  $2 \times 2$  binary matrices. Grouped into 10 rows, one for each set that is equivalent under permutation of the two blocks. The letter labels used here and in the text are applied only to these matrices (never to designate other images or to data matrices).



termed *crystallizers*: they resemble sociometric stars in importance, not because of the number of choices they receive but because of their strategic, "structural" position in the overall matrix following from the hypothesized model. Other persons are allowed multiple, alternative assignments by the BLOCKER algorithm; these are termed *floaters*. They are somewhat analogous to the sociometric isolate, who receives few or no choices.<sup>21</sup>

The number of different blockmodel hypotheses, even with just two or three blocks and just two types of tie, is so large that some other approach is desirable for initial exploration. Breiger has developed a hierarchical clustering algorithm that partitions men into possible blocks and then finds a blockmodel by inspecting the data matrices rearranged according to the partition.<sup>22</sup> It is called CONCOR; its formal behavior is analyzed in Breiger et al. (1975); some mathematical properties are described in Schwartz (1974).

The difference between the two algorithms is as follows. CONCOR produces from raw data an assignment of individuals to blocks, and thence suggests a blockmodel hypothesis. BLOCKER demands a blockmodel hypothesis and derives from it any assignment of men to blocks that satisfies the hypothesis for the given set of data matrices. Matrix entries in any numerical form can be direct input to CONCOR, whereas each tie must be coded as either 0 or 1 before BLOCKER can be used. Substantive judgment is required in both: in CONCOR, on when to stop the further splitting of blocks; in BLOCKER, on what blockmodels constitute appropriate hypotheses.

BLOCKER searches for pure zeroblocks; an assignment is rejected for a blockmodel hypothesis if even one tie thereby appears in any zeroblock. CONCOR partitions the population in a way that may be intuitively characterized as yielding sharp contrasts in densities of ties between different blocks. The image suggested by CONCOR can be refined by varying the cutoff level of tie density below which a block is coded as a zeroblock.

<sup>21</sup> See Appendix A for more specific information about the assignment of men to blocks.

<sup>22</sup> Specifically, given  $k$  matrices, each of size  $n \times n$  and reporting ties among a population of  $n$  actors, a two-dimensional matrix ( $M_0$ ) with  $k \times n$  rows and  $n$  columns is formed by "stacking" each of the  $k$  matrices one above the other, taking care to preserve column ordering. (Alternatively, the  $2nk \times n$  array of each matrix and its transpose may be formed.) The  $n \times n$  correlation matrix ( $M_1$ ) of product-moment correlation coefficients among columns of  $M_0$  is then formed. This process is iterated ( $M_{j+1}$  is the matrix of correlations among all pairs of columns of  $M_j$ ) until a limit matrix is obtained, which may be permuted to yield a bipartite division of the actors (columns) into exactly two subsets (blocks) of sizes  $s$  and  $n-s$ . A refinement to any desired number of blocks may be obtained by creating a new array  $M_0$  with  $k \times n$  rows and  $s$  columns,  $k \times n$  rows and  $n-s$  columns, etc. In their detailed description of the algorithm, Breiger et al. (1975) include extensions and comparisons with multidimensional scaling and hierarchical clustering methods in the literature. We wish to thank A. Tagg of the University of Surrey for calling our attention to the anticipation of this algorithm in McQuitty and Clark (1968).

The image required by BLOCKER can be refined by following the inclusion lattice for images.

In the theory and interpretation of blockmodels, neither BLOCKER nor CONCOR is indispensable. It is possible, though laborious, to find and test blockmodel hypotheses by simply inspecting many permutations of the data matrices.

#### FIVE CASE STUDIES

This section reports five case studies on which we tested blockmodels. Four concern adults in work situations; only one (the Firth-Sterling Corporation management) included all of the population's relevant authority figures. Two are panel studies using at least four time periods. Four of the populations are primarily face-to-face groups of fewer than 20 members, and one is a subsample from a larger sample ( $N = 107$ ). Four are exposed to normal turnover in members. All five include systematic data on individual attributes. All the populations are twentieth-century and American. This section concludes by illustrating the use of blockmodels with overtime data for two of the studies.

For four of our case studies (all but the biomedical research network), detailed independent analyses are presented in the original studies of the interactions. These discussions have informed our search for blockmodel hypotheses used as input to BLOCKER and are also used to validate some of our findings. We cite here five general findings from the case studies which illustrate this validation of the blockmodels and also additional insights obtained. (1) CONCOR, a mechanical search algorithm not dependent on our perceptions of the "meaning" of the data, produces a partition of individuals into blocks which is *equivalent* at the three- or four-block level of refinement to the identification of major groupings in the original study of the interactions, in all four cases for which such comparisons may be made (see results reported below and in Breiger et al. 1975). This strong finding suggests the validity of our approach to network aggregation. (2) Even though many of our hypotheses which are tested by BLOCKER are informed by our reading of the original studies, blockmodels constructed across several different types of social relation in each study are valuable in portraying the overall social structure. (3) BLOCKER partitions for coarse (two- and three-block) models agree with the partitions independently derived by CONCOR. (4) In each case study, we suggest additional interpretation (based on a finer partition of individuals into blocks and/or a consideration of the patterns of relations brought out in the blockmodel) which goes beyond the analyses of the original accounts. (5) The case of the biomedical research network, for which a detailed analysis of the interactions among the population's members does not exist,

illustrates the utility of blockmodels as an exploratory procedure in searching for structure when the only available clues consist of reports of interaction among pairs of individuals.

#### A Biomedical Research Network

*Data.*—Griffith et al. (1973) identified 173 scientists studying the neural control of hunger and thirst. Of these, 107 responded to Griffith's questionnaire. In more than half the possible instances (as fig. 1 shows) one respondent was unaware of another, as can be expected in an open population.

*Blockmodel.*—In order to apply BLOCKER to these data, each entry in the matrices must be coded in binary form. Only a reciprocated choice on "mutual contact" was regarded as strong enough by itself to prevent a zeroblock; thus symmetric choices on mutual contact were coded "X" and the rest coded "blank." "Unaware of" is not like other, substantive, types of tie, so unreciprocated choices on it were treated as a distinct type of tie, with reciprocated choices constituting a third type. A blockmodel hypothesis, stated in the top panel of figure 3, for these three types of tie was developed by systematically exploring intuitively plausible blockmodels. At the left in figure 3 is the partition—the unique assignment (defined in Appendix A)—of men to blocks that BLOCKER yields when the data are tested against the hypothesis. The bottom panel shows the data matrices with rows and columns blocked in conformity with this partition: inspection shows the blockmodel is confirmed.

*Interpretation.*—Interpret the blockmodel in status terms. (Blocks are ordered from high to low status.) On symmetric "mutual contact," the bottom two blocks are connected neither internally nor to one another, and the bottom block has no connections with any block, including itself. The bottom block belongs to the population only in the cultural sense that it has no asymmetric unawareness of the block that is obviously the leading set of researchers. No block has asymmetric unawareness ties to the top block; yet that block has asymmetric unawareness entries to each of the others; we might call this a snob effect.

*Further tests.*—CONCOR was applied (not shown here) to the data matrices for "mutual contact" and "unaware." Its first split of the 28 men yielded a partition similar to BLOCKER's: the first two groups in the latter became one group, and the last two a second, except for two interchanges. After two more splits, the partition was

(1 4 9 23 2 10 26)	(24 19 12 14)	(6 7 28 11 15 13 16)	
	(18 22 3 5 8 17 20 21 25 27).		[1].

The extreme blocks are close to those of figure 3; the middle two are

p 0000  
c 1100  
q 0110  
e 1110

0111  
0011  
0111  
0111

0011 0011 1111 1111

**MUTUAL CONTACT**

**ONE UNAWARE**

**BOTH UNAWARE**

[illegible]

FIG. 3.—Blockmodel for biomedical network: Images and blocked data matrices. In the left margin are letter labels for blocks and the rearranged numerical labels for individuals from fig. 1.

more mixed. For all three types of tie, we constructed a blockmodel from the CONCOR partition by defining a zeroblock as any block with less than a specified fraction of the average density of entries for the given type of tie. For any cutoff fraction between  $1/10$  and  $1/2$ , the resulting blockmodel was very close to that of figure 3.

Each of two additional arbitrarily chosen sets of 28 persons (not overlapping with each other or with the first) was then blockmodeled in exactly the same way, again using CONCOR. The two resulting blockmodels were close to each other and to the blockmodel in figure 3. Although the full sample of 107 had not yet been simultaneously partitioned (see Breiger 1976), we inferred that in the larger study both the blockmodel pattern and the block memberships would correspond with the results reported for these subsamples. Everyone in the network knows the top dogs (block *a*), but although these top dogs collaborate with some researchers in lower strata, they appear to remain ignorant of most lesser mortals. Members of block *b* appear to be very active researchers, aware of one another. Unlike those in the bottom block, members of the third block (*c*) are not just on the sidelines; they frequently see at least some researchers in higher blocks. Clearly, the complete interconnectedness of a face-to-face group or other community is not necessary for the coherence of this social structure. Neither these blocks nor (more importantly) the global pattern of relations over the network would emerge from counts of individual "popularity" or from conventional clique analysis.

### A Monastery in Crisis

*Data.*—Sampson's (1969) detailed account of social relations in an isolated American monastery should become a classic. During a 12-month period, much of it in residence as an "experimenter on vision," Sampson developed an extraordinary variety of observational, interview, and experimental information on the monastery's social structure. Toward the end of his study, a major blowup in the monastery culminated in a mass exodus of members by expulsion and resignation.

Sampson defined four sorts of relation—Affect, Esteem, Influence, and Sanction—on which respondents were to give their first three choices, first on the positive side and then on the negative. Figure 4 shows these choices for his fourth time period, before the blowup but after a new cohort of novices had settled in. For example, novice #3 liked novice #1 best, and therefore a 3 (representing highest choice) is entered in the intersection of the #3 row and the #1 column of the top left matrix (an entry of 2 means second choice and a 1 means third choice). We assign each monk a number from 1 to 18 (roughly in their order of joining the monastery—the same order Sampson used). We give each of the eight positive and

	111 011 111	100 110 111	110 010 111	100 010 111
	LIKE	ESTEEM	INFLUENCE	PRaise
10	11 3	1 32	32 1	
5	1 32	3 3 1	1 32	
9	12 3	32	2 3 1	1 32
6	3 1 2	2 1 3	2 1 3	2 1 3
4	2 3 1	23	32 1	3 2 1
11	2 3 1	1 23	1 22	231
8	22 1	321	32 1	2 3 1
12	1 2 3	1 3 2	2 3 1	23 1
2	23 1	1 2 3	23 1	23 1
14	13 2	132 2	132 2	33 2 1
15	2 3 1	132	32 1	132
7	1 3 2	1 3 1 2	1 3 2	3 1 2
16	3 12	1 3 22	3 12	2 12
13	2 3 1	3 1 2	3 1	3 2
3	3 1 2	3 2	3 2 1	3 2 1
17	1 2 3	1 12 3	1 2 3	1 2 3
18	1 22	2 21	2 21	11 1
	011 111 110	011 111 110	011 101 110	011 111 111
	ANTAGONISM	DISESTEEM	NEGINFL	BLAME
10			132	
5		1 32	1 32	
9	1 2 321	1 3 22	1 3 21	3 3 1 21
6	231	231	231	33 1 1 23
4	13 2	13 1 2	1 3 12	1 23
11	31 2	1 23	1 3 2	123
8	2 3 1	2 3 1	2 3 1	13
12	1 3 2	1 3 2	1 3 2	3 1
2	3 2 1	3 2 1	3 2 1	3 1
14	1 32	31 1	31 1	1 32 1
15	23 1	32 2	32 2	12 3
7	3 12	3 12	3 12	123
16	2 1 3	2 1 3	2 1 3	123
13	2 3 1	2 3 1	2 3 1	123
3	3 1 2	3 1 2	3 1 2	123
17	31 2	32 1	32 1	123
18	2 1 3	1 2 3	1 2 3	123

FIG. 4.—Blockmodel for the monastery, time 4: images and data matrices. In the latter, 3 stands for first choice, 2 for second, and 1 for third on each type of tie. Source: Sampson (1969).

negative relations a distinct name. In all of our case studies we use "Like" and "Antagonism" for positive and negative affect, respectively.

*Blockmodeling.*—CONCOR was applied to all eight matrices of figure 4.<sup>23</sup> This algorithm is not explicitly concerned with locating zeroblocks; data of all three choice rankings are processed by it. After two splits the partition into three blocks was

$$(10 \ 5 \ 9 \ 6 \ 4 \ 11 \ 8) \ (12 \ 1 \ 2 \ 14 \ 15 \ 7 \ 16) \ (13 \ 3 \ 17 \ 18). \ [2]$$

This partition was then imposed on the eight data matrices (the results are shown in fig. 4). We assumed that in a population of 18 persons, only the top two choices were strong enough to invalidate a zeroblock (establish a bond), so each block which contained no entry greater than 1 was

<sup>23</sup> Slightly different results are reported in Breiger et al. (1975) because they, like Sampson, summed plus and minus entries to combine the eight types of tie into four matrices for CONCOR input. Our choice is perhaps preferable because it makes less strong measurement assumptions; see also n. 24 below.

represented as a zeroblock. The resulting blockmodel is shown in the top panel of figure 4.

To apply BLOCKER, which requires binary input data, values of 2 and 3 were coded as 1, the rest as 0. When the blockmodel shown in figure 4 was tested on the data using BLOCKER, the unique assignment found was *exactly* that derived from CONCOR! Direct inspection of figure 4 confirms this finding (note that third choices, coded as 1, should be ignored).

In the image for esteem, the three blocks are ordered in a complete linear hierarchy, which is certainly plausible in a monastery. The bottom block has a bond to itself and to each of the higher blocks on every kind of positive relation; yet it also has reciprocated bonds with both of the other blocks on all four negative relations. Liking bonds are universal with one exception: the second block does not match its esteem bond for the first with a liking bond. The two top blocks exchange no positive sanction ("praise" in fig. 4); however, the first block, top on esteem, concedes influence to the second.

*Refinements and interpretations.*—If we return to the CONCOR approach and raise the cutoff density for zeroblocks to half the average density, the resulting blockmodel is

1 0 0	1 0 0	1 1 0	1 0 0
0 1 0	0 1 0	0 1 0	0 1 0
0 1 1	0 1 1	0 1 1	0 0 1
Like	Esteem	Influence	Praise
0 1 1	0 1 1	0 1 1	0 1 1
1 0 1	1 0 1	1 0 1	1 0 1
1 1 0	1 0 0	1 0 0	0 0 1
Antagonism	Disesteem	Neg. Infl.	Blame

The Like image is identical with Esteem, and Disesteem with Negative Influence. As in the previous blockmodel, the top two blocks exchange negative bonds of all four types, but there is only one kind of negative bond from the bottom block to the second block. The concrete social structure suggested is much the same for either version: a top-esteemed block unambivalently positive toward itself, in conflict with but conceding influence to a second, more ambivalent, block, to which is attached a block of losers.

For this small population, further refinement of the partition by mechanical application of algorithms is not justified, but hints in Sampson's historical account and inspection of the matrices suggested a refinement

of the partition into five blocks, with each of the two left blocks in [2] being split as:

(10 5 9) (6 4 11 8) (12 1 2) (14 15 7 16) (13 3 17 18). [3]

This is the ordering of rows and columns used in figure 4. BLOCKER, applied as before, verified that this partition provided the unique assignment for a certain blockmodel; the question is: Does the blockmodel make sense? Consider first the esteem image from BLOCKER in this refined blockmodel (the lines show divisions into the blocks of [2]):

	<i>a</i>	<i>b</i>	<i>c</i>	<i>d</i>	<i>e</i>
<i>a</i>	0	1	0	0	0
<i>b</i>	1	1	0	0	0
<i>c</i>	0	1	1	0	0
<i>d</i>	0	0	1	1	0
<i>e</i>	1	0	1	1	1

(Recall that this image fits the data with 2's and 3's coded 1 and all other entries 0.) Name the blocks from top to bottom and from left to right *a*, *b*, *c*, *d*, *e*. In brief, within the old top block, *a* is now a hanger-on to *b*; within the old second block, *d* defers to the core block *c*. It was *c*, but not *d*, that esteemed the initial top block, and then it only esteemed the core (*b*). Moreover, it was the hanger-on, *a*, who conceded influence to the initial second block, but only to its core, *c*. (The old bottom block of losers remains the same; it [*e*] esteemed only the hangers-on [*a*] of the old top block.)

The other images in the refined blockmodel can be read from the data matrices in figure 4. All four positive images confirm the hangers-on and deference structures within the former blocks. There are three more liking bonds than esteem bonds, but except for this fact and the special asymmetry in esteem and influence among the top blocks (already mentioned), the four positive images are almost identical. The refinement of negative bonds is simpler: in each of the four negative images there are reciprocal bonds between *b* and the bottom three blocks (*c*, *d*, *e*) but almost none to *b*'s hangers-on (*a*). And the loser, *e*, though receiving many or all types of negative bonds from the other four blocks and reciprocating to both *a* and *b*, sends no negative bond to *d*; however, *e* sends all four types of negative bond to *d*'s masters (*c*).

*Comparison with Sampson's analysis.*—Both the blockmodels for three blocks and that for five blocks can be compared with Sampson's own analysis. Sampson (1969, p. 370) posited a definite clique structure for the monastery at time T4, on the basis of sociometric graphs (drawn from the data shown in fig. 4), his own observation, and his interpretation of events and personal attributes. His Young Turks are led by monks 2,



1, and 12 (in descending order of leadership), with 14, 15, 7, and 16 as followers. His Loyal Opposition is led by 4, with 5 a popular member, 6 and 11 as members, and 9 less fully attached. He saw three Outcasts: 3, 17, and 18. The other three monks (10, 8, and 13) wavered between the two cliques, which he described as being in intense conflict.

Both CONCOR and BLOCKER agreed on the split into three blocks (shown in fig. 4). Sampson's Loyal Opposition is wholly contained in the first block; the Young Turks are exactly the men in the second block; the Outcasts are wholly contained in the third block. Sampson's Waverers 8 and 10 are in the Loyal Opposition block, whereas Waverer 13 is in the Outcast block.

Our refined five-block blockmodel splits the Young Turks exactly as did Sampson, with 1, 2, and 12 as leaders; however, the first two blocks split the Loyal Opposition differently, as well as enlarging it. Monk 5 is changed from Sampson's "socio-emotional leader" (p. 360) to membership in the Loyal Opposition's hangers-on block. Sampson earlier observed (p. 322, n. 32): "His [monk 5's] circumspect aloofness from interpersonal conflicts served to preserve his relatively high ranking on most measures throughout the study, but as a consequence, his influence on others was more that of a detached role model than a framer of opinion or action." And Waverer 8 is in the leading block (*b*) of the Loyal Opposition according to the blockmodel.

The pattern of relations given in figure 4's blockmodel—the eight images on three blocks—accords with Sampson's basic contention of a fight between the Loyal Opposition and the Young Turks. The blockmodel reports strong ambivalence within the Young Turks—simultaneous positive and negative bonds of many types—but none within the enlarged Loyal Opposition; all of these reports accord with the detailed statements in Sampson's analysis. The blockmodel makes three further important assertions: the Young Turks are conceded top position in a linear hierarchy of the three blocks on influence, while the enlarged Loyal Opposition is conceded top spot in a hierarchy on esteem. Third, the bottom block has but one internal type of bond which is negative; it also receives Like bonds from above, as well as the negative bonds that its members return in kind: this implies that the bottom block is a meaningful social unit in a sense different from that of Sampson's "pseudo-group."

Bits of evidence in Sampson's detailed textual account support these further assertions; for example, monk 13 nominates, and is the only man to vote for, 3 as chairman of an important meeting (p. 354).<sup>24</sup> Our

<sup>24</sup> Monk 13 is placed in the Loyal Opposition by Breiger et al. (1975) because of the way they apply the Breiger algorithm; see n. 23 above. Yet when they did a multidimensional scaling analysis for comparison, using Kruskal's MDSAL algorithm,

assertions contradict some of Sampson's summary statements, but this fact per se is not as important as the fact that blockmodeling has permitted us to move *beyond* the picture Sampson drew—beyond the kinds of inferences that are technically feasible from sociometric diagrams. The blockmodel on five blocks necessarily differs from Sampson's conclusions; it analyzes units finer than his factions but ignores the distinctive behavior of individuals that he emphasized. Until the matrices for earlier time periods are taken up below, the main support for the refined blockmodel is the consistency across images of the pattern within each faction.

One week after the period to which these data refer, an explosion started. The Superior and the Novice Master, together with the handful of senior monks (none of them included in the sociometric population of the 18 monks in training), decided in their regular review process to expel monks 2, 3, 17, and 18. The reasons given by the senior staff were, for the latter three, that they were "too immature" and had "personality problems," while monk 2 was considered "too independent, questioning and arrogant" (Sampson 1969, p. 373). Only monks 1, 2, and 3 of the 18 had been to college and were candidates to be full clerical monks; they were older and restless even under the drastically reduced discipline their seniors had instituted a year earlier, before their arrival and that of monks 10 through 18.

Almost at once, monk 1 voluntarily departed. Then, within a week, monks 16, 15, 14, and 7 left, in that order. A few days later, 13 and 8 left, also voluntarily. A month later still, monk 10 left. Of the six remaining from the 18, note that four had been there in the old days before the change of discipline, and five were in the Loyal Opposition. A puzzle in both Sampson's picture and the blockmodel is why monk 12 remained. Otherwise the blocks found in building the refined blockmodel from pure sociometric data fit the initial departures perfectly: monk 1 followed his blockmate immediately, and the next wave of four was precisely the block asserted to be their subsidiary; only after them did monk 13 leave, and he preceded the two from the Loyal Opposition.

### Cliques and Strata in the Bank Wiring Room

*Data.*—Homans's (1950) classic account of the Bank Wiring Room suggested a six-block blockmodel;<sup>25</sup> only after assessing this hunch will we apply the two algorithms in the usual way. The original monographic treatment (in Roethlisberger and Dickson 1939; hereafter abbreviated as

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monk 13 was placed substantially closer to other Outcasts, a placement consistent with our present blocking.

<sup>25</sup> This is a slight simplification of an earlier blockmodel on seven blocks, reported in White (1974a). Inspector I3, a separate block there, has been combined with the third block (Wiremen W2 and W5).

R-D) is itself a classic. A production section that wired switchboard banks in a Western Electric plant was transferred to a separate room; for a year men shared this room with an observer who, with other researchers, repeatedly interviewed the 14 men, monitored official records, and so forth.

The observers reported *their* judgment of the incidence of five types of tie<sup>26</sup> (shown in the matrices of fig. 5). Two are the familiar Like and

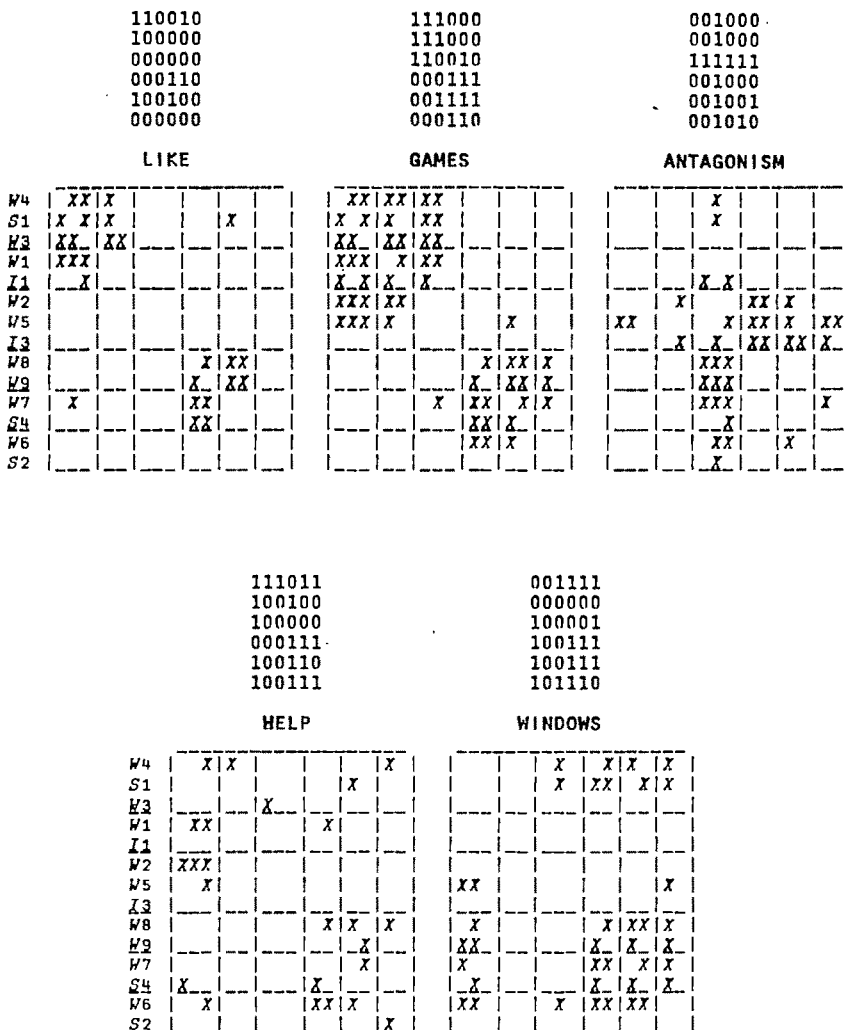


FIG. 5.—Blockmodel for the Bank Wiring Room: images and data matrices

<sup>26</sup> An additional type of tie is not reported here because it could be received only by soldermen.

Antagonism, but three others report context-specific types: "Games" is the designation for a kind of affectionate horseplay (including "pinging" the upper arm); "Help" (with production tasks) is our name for a second type; "Windows" reports chronic quarrels over opening windows. The observers saw these as stable interaction patterns established when the section had settled down. In all types but Help, each tie is reciprocated. A man need not "send" any ties of a given type. When applying BLOCKER, each tie reported as present was coded "1."

In addition to two inspectors (called hereafter I1 and I3, after Homans), there were nine wiremen (numbered from W1 to W9 by position in the room from front to back) and three soldermen (S1, S2, S4). Layout was fixed: W1-W3 were assisted by the solderman at the front of the room, S1; W4-W6 by S2; and W7-W9 by S4; I1 and I3 shared inspection of banks from the middle team.

Interpreting all the kinds of evidence, R-D, followed by Homans, concluded that the social structure was based on two cliques, located mainly in the front and the back of the room, respectively. They listed the members in the front as W1, W3, W4, S1, and I1, and those in the back as W7, W8, W9, and S4; but they also saw nuances within each group and discussed other individuals as fringe members. At other places in their accounts, they emphasized that individuals have differential standing or prestige in the informal social structure. To us, their account strongly suggested a hangers-on pattern within each clique as well as strata cutting across the cliques.

*Developing a blockmodel.*—Games ties were described as friendly, and for both them and Like ties the hangers-on pattern (the *E* element within the clique) seems appropriate. But Games ties were much more numerous than Like ties, so it seemed likely there should be more men in a core group on Games than in a core group on Like. Like and Games between them thus should differentiate each clique into three blocks. Antagonism was concentrated on, and within, the set of men whom both R-D and Homans judged to be, at best, marginal to the cliques, and who would appear in the bottom blocks of the two cliques. These three types of tie suggested a blockmodel for six blocks on Like, Games, and Antagonism; it was not clear what pattern to expect on Help or on Windows, except that the latter should be concentrated in the back of the room. This approach suggests not only the images but also the memberships of at least the higher blocks.

The initial blockmodel was adjusted by inspection of the actual choices; it is shown in the top panel of figure 5 as the first three images. The data, also shown in figure 5, fit this blockmodel: BLOCKER indeed yields the unique assignment shown (see Appendix A). Two important points should

be made. This partition is consistent with—indeed a refinement of—Homans's cliques. The partition is

$$\begin{array}{lll} (W4\ S1\ W3) & (W1\ I1) & (W2\ W5\ I3) \\ (W8\ W9) & (W7\ S4) & (W6\ S2). \end{array} \quad [4]$$

Furthermore, each of the three images is close to our first guess for it. Nested hangers-on patterns over Like and Games, for each clique, are shown, together with one symmetric bond joining the cliques. All Antagonism ties are within or with the bottom block in each clique; in addition, the front clique's marginal men receive most of the negative bonds from both the front and the back cliques. The bonds in the last two images in figure 5 (Help and Windows) were simply read from the data matrices upon which the given partition was imposed.

*Testing the blockmodel.*—When CONCOR is applied to all five data matrices, the first split is *exactly* between the first three blocks and the last three. When each of the two sets of blocks is routinely split, the result is

$$\begin{array}{ll} (W4\ S1\ W3\ W1\ I1) & (W2\ W5\ I3) \\ (W8\ W9\ W7\ S4) & (W6\ S2), \end{array} \quad [5]$$

again perfect conformation to boundaries in the full six-block blockmodel! CONCOR first distinguished between the cliques and then, within each set positively bound together, distinguished strata. A blockmodel on these four blocks can be aggregated from the one on six blocks in figure 5 by taking the logical union of the first two rows and the first two columns, and then doing likewise for the fourth and fifth rows and columns.

One can also emphasize the differentiation into strata as the overriding feature, rather than the split between cliques. Suppose we combine the first and fourth blocks of figure 5, the second and fifth, and the third and sixth. When unions of the corresponding rows and columns are computed, the five images become

<i>L</i>	<i>G</i>	<i>A</i>	<i>H</i>	<i>W</i>
1 1 0	1 1 1	0 0 1	1 1 1	1 1 1
1 0 0	1 1 1	0 0 1	1 1 0	1 1 1
0 0 0	1 1 0	1 1 1	1 1 1	1 1 1

When this blockmodel was applied by BLOCKER to the five data matrices, we obtained a single solution: the top block is the union of the first and fourth blocks of figure 5, and so forth.

The analyses of the Bank Wiring Room in R-D and in Homans are the basis for the blockmodel and so can hardly be cited as independent evidence.

*Test based on reasons for work stoppage.*—In the original report, Roethlisberger and Dickson (1939, pp. 428–32) stated that the bank wiring department allowed the nine wiremen to claim allowances for unusual work stoppages “beyond their control.” Wiremen frequently claimed more time allowances than were necessary (contrary to the intent of the wage-incentive scheme) because they were willing to trade some loss of income for some gain in security (expressed as uniformity in output curves). Each time a wireman claimed a time allowance, he was supposed to give the reason for the delay. R-D coded 12 classes of reasons and then cross-tabulated the claims by reason and wireman. Generalized attitudes need have little relation to specific position in a particular population, but context-specific attitudes such as these reasons should be affected by one’s position and should, hence, resemble those of others in equivalent positions.

In this cross-tabulation, each wireman has a column and each reason a row. We used CONCOR to split the wiremen on the basis of their respective columns of counts.<sup>27</sup> The result is

(W1 W2 W3 W4 W5) (W6 W7 W8 W9). [6]

It is at once apparent that the split is precisely that between the blocks of the front clique and those of the back (see [4] or [5]). In particular, the marginal members W2, W5, and W6, whom Homans did not place, are each grouped with the wiremen from the appropriate upper blocks.

In only two of our case studies did the population have specific job assignments, and only the Bank Wiring Room also had fixed locations. Let us return to the six-block partition given in figure 5. We already know that joint membership in a block follows neither from having the same one of the three jobs nor from having different jobs. Sayles (1958, a monograph on industrial work groups) criticized the earlier literature for giving too much independent importance to dynamics in small groups as such; the keys to social structure and process, he argued, were kinds of job and the flow of work imposed among jobs. At first sight, it seems that our analysis provides a counterexample; however, close examination shows that the split between cliques is as important a determinant of the six blocks as is the division among strata, and the cliques clearly emerge from the layout of the room. Similarly, for all types of tie except Antagonism, the pattern of bonds between blocks depends on the clique split as much as on strata.

#### Newcomb’s Second Fraternity

*Data.*—Newcomb (1961) analyzed two experiments in which 17 previously unacquainted male undergraduates lived together in a fraternity-

<sup>27</sup> See Appendix B for further details.

style house, expenses paid. They were subject to observation and required to supply many self-reports, including a complete rank ordering of the other 16 by "favorableness of feeling" during each of the 16 weeks of the experiment. Newcomb reported only measures of association for the rank orderings (which contributed only indirectly to his account of the social-psychological dynamics). Here we take polar types of tie (Like and Antagonism), abstracted from the rank order for the last week of the second experiment, and suggest a blockmodel for three blocks. The individual-level data were described by Newcomb's associate, Nordlie (1958), who developed an independent interpretation that is more explicit than Newcomb's. Individuals are numbered as in Nordlie's Appendix A, as are ranks (from 1 for most favorable to 16 for least; no ties permitted). (The parallel first experiment will be discussed in Part II.)

*Developing a blockmodel.*—In a population of this size, the top two ranks were believed to represent strong friendship choices; the bottom two, strong antagonism. There is a scapegoat in this group (man 10), who received one of the bottom three choices of each of the other 16 persons. For application of BLOCKER, the top two choices were coded as Like ties; the bottom three, as Antagonism ties. There seemed to be a top group that disdained the others, so the  $V,F$  blockmodel (for two blocks) was hypothesized. The result was a split of men into blocks for which Like and Antagonism satisfy the blockmodel; men 13, 9, 17, 1, 8, 6, and 4 were in the top block. This is the only split that yields a solution, and it stipulates two floaters (men 2 and 5) who can be placed separately or together in either block (see also Appendix A).

An obvious refinement is a split of the bottom block into (1) losers and (2) a stratum not internally antagonistic and ambivalently oriented to the top block of seven: the blockmodel is

1	0	0	0	0	1
1	1	0	1	0	1
1	1	1	1	1	1
Like			Antagonism		

*Developing a blockmodel.*—BLOCKER, using this model, yielded the following: a second block (men 7, 11, 12, 2) and a bottom block (14, 3, 10, 16, 5, 15); the top block is unchanged, but the two floaters are now more restricted.

CONCOR was then applied to the complete rankings (each rank treated as an integer) to yield three blocks. These were identical with the three blocks BLOCKER yielded from the top two and bottom three choices. To define the blockmodel, we state a *cutoff density* (the highest average density of "choices" in a block that permit it to be coded as a zeroblock

for that type of tie).<sup>28</sup> With the top two ranks defined as Like choices and the bottom three as Antagonism, for any cutoff density below one-fifth the average density on that type of tie, the resulting blockmodel is the same as the one presented above. When the cutoff density is raised further, the first bonds to disappear are the bottom block's positive bond to itself and its negative bond to the second block.

*Comparison of the blockmodel with Nordlie's interpretation.*—Nordlie's (1958, pp. 67–77) study of "subgrouping" yields an assignment of men to "clusters" for week 15 that is entirely consistent with our blockmodel. For each pair of men, Nordlie computed the rank correlation coefficient of the choices *sent* by each to the 15 others, excluding themselves. This matrix of "intra-pair agreement" was then clustered as described by Nordlie (1958, pp. 70–71) to produce subgroups of nonoverlapping membership and a residue of men assigned to no cluster. Nordlie discovers five subgroups for week 15 of the second experiment: (1, 5, 6, 8, 13), (2, 4, 17), (7, 11, 12), (3, 14), (15, 16). Men 9 and 10 are not assigned. Each of the five subgroups is contained within a single block of our three-block blockmodel, if the two floaters (men 2 and 5) are allowed either of their assignments.

Nordlie does not distinguish Like and Antagonism, and he does not go on to examine and interpret concrete patterns of ties among his clusters. The blockmodel above suggests a combination of balance theory (in some form) with hierarchy as the forces at work.

### Management Conflict in a Company

*Data.*—This section develops a blockmodel on eight types of tie specifically relevant to the business activities of the 16 top managers (in 1958) of Firth-Sterling, a company with about 2,000 employees producing specialty alloy and abrasives products for industrial use (for a full description of the data, see White 1961). The blocks and patterns fit well with independent evidence (White 1960, 1961) of chronic conflicts over research and development of new products (the prime management issue in a company whose product line turned over every few years in the era of the R&D fever). Managers are numbered as in the original report (White 1961, table 1): 1–5 are R&D managers; 6–8, sales managers; 9 and 10, production managers; 11 and 12, executives; and 13–16, top staff managers; in addition, 6, 9, and 12 are vice-presidents, and 11 is the president (a

<sup>28</sup> It would not be satisfactory to use average rank order in a block in place of density of choices. The former measure averages Like and Antagonism choices numerically. Ambivalence would be excluded; in contrast, a blockmodel permits a given image to contain both a bond on Like and a bond on Antagonism.



man of elite social and business standing brought into the ailing company a few years earlier).

*Developing a blockmodel.*—With such ties, there were no precedents for hypothesizing a blockmodel. CONCOR was applied to the data matrices for all eight detailed types of relation (the seven reported by White [1961, p. 192], with choices on the fifth type—Respect for Knowledge and Respect for Decisions—separated into two matrices). The partition into three blocks found<sup>20</sup> was

$$(11 \ 9 \ 13) \ (2 \ 4 \ 6 \ 7 \ 8 \ 1 \ 15) \ (5 \ 10 \ 12 \ 14 \ 3 \ 16). \quad [7]$$

Since each choice had been carefully considered by the respondents, the cutoff density chosen for zeroblocks was zero (i.e., a single choice in a block prevented its being coded as a zeroblock). The top panel of figure 6 reports our blockmodel on three images: Similar Business Policy, Personal Friendship, and Uncomfortableness. The blockmodel was obtained by imposing the above partition onto the matrices for these relations. The bottom panel shows the resulting blocked matrices. (Obviously—

	110	000	011
	110	111	101
	101	011	000
	SIMILAR POLICY	FRIENDSHIP	UNCOMFORTABLENESS
11	XX	X	X
9	X		
13	X	X	X
2	X	XX	
4	X		
6		XX	X
7	X	X	X
8	X	XX	X
1	XX	X	
15	XX	XX	
5		X	X
10	X		
12	XXX		
14			
3	X		
16	XX		

FIG. 6.—Blockmodel for Firth-Sterling management: images and data matrices. Source: White (1961).

<sup>20</sup> Following the usual procedure, all eight  $16 \times 16$  data matrices were first “stacked” into a  $128 \times 16$  array. It was then found that two of the 16 columns in this array—those for men 8 and 16—consisted solely of zeroes. As the product-moment correlation is undefined for vectors with no variance, these two columns were removed and CONCOR was applied to the remaining 14 columns. Men 8 and 16 were then placed arbitrarily into blocks. When BLOCKER was applied to test fig. 6’s model against the data shown there, men 8 and 16 appeared as the only “floaters.” We note that no other columns contain zero variance in any of the data matrices for the five case studies.

an implication of the zero cutoff density—this partitioning matches that produced by BLOCKER for this blockmodel.)

Included in both the Policy and Uncomfortableness matrices of choices are guesses about the question "who . . . might single you out . . . ?" (for each relation). Guesses were requested in order to increase the scope of choices, especially on uncomfortableness. Indeed, 19 of the 27 guesses coincided with direct choices, while few were reciprocated (an indication that these were surrogate choices rather than realistic perceptions of others' choices). Every guess that did not coincide with a choice fell into one of the 10 bonds shown in the images; none fell into any of the eight zeroblocks.

*Interpretation.*—We now turn to a substantive examination of the blockmodel, considering first the division of men into blocks. The partition is neither into departments nor into formal ranks. When CONCOR was applied twice more (to split the two big blocks), then men 4 and 15 were separated out within the left-hand block; and men 3, 16, and 5 were split from the rest of the right-hand block (see [7]). These five blocks, ordered as in the permutation shown above, correspond exactly to the grouping of managers by their attitudes to R&D (inferred from their questionnaire responses, interviews, and actions in specific conflicts over R&D [White 1960, 1961]). Indeed, the permutation classifies managers by degree of hardheadedness toward R&D. The Executive Vice-President (12), the Treasurer (14), and the Director of Abrasives Production (10) were utterly skeptical. The Personnel Manager (16) and the two R&D managers concerned with alloys (3 and 5)—the more routine side of production, in which only routine development was carried on—were close seconds. All three Sales Managers, plus the overall coordinator of R&D (1) and the man (2) who ran the most speculative research project, were most optimistic and favorable to R&D. The President's Trouble Shooter (15, in charge of expediting some new products) and manager 4 (a respected engineer with several inventions that he was trying to produce by a job shop operation) were favorable but not optimistic about results. In the middle were the President (11), the Vice President for Production (9), and the Accountant (13), in the observer's opinion the most realistic persons of the entire group; all three were acutely aware both of the poor record of Firth-Sterling's R&D and of the crucial importance (for the company's image and borrowing capacity) of having an R&D program.

The blockmodel on the three main blocks with the three specific images makes equal sense. In figure 6 the block of 11, 9, and 13 is put on top, consonant with its obvious preeminence in the eyes of all. It exchanges policy bonds with the Sales block (the middle block in the partition [7]) and receives a policy bond from the hardheaded block; in addition, each block has a policy bond to itself. On Personal Friendship, the top block

sends no bonds, and it is not chosen by the hardheaded block (a fact showing the realism of the latter); whereas the Sales block, in the most vulnerable position on R&D, claims bonds to all three blocks. There is no Uncomfortableness bond within any block. The top block and the Sales block are uncomfortable with each other, and each is ill at ease with the hardheaded block. The latter had repeated specific clashes with the Sales block (it seems likely, from the interviews, that the hardheads were simply unwilling to reveal specific negative choices for any reason).

We have not reproduced here the matrix of choices on "the managers with whom you have the most dealings." The image for these objective choices would contain only bonds, no zeroblocks. Indeed, if the densities of choices are computed by block, the results are

(11 9 13)	:	.50	.14	.11
(2 1 6 7 8 4 15)	:	.57	.17	.12
(3 16 5 10 12 14)	:	.33	.10	.10.

The striking feature is the homogeneity of entries within columns. Everyone claims most dealings with the President's block, next most with the Sales block, and least with the hardheaded block; but in this small population of top managers, each block shows some heavy contacts with managers in each other block.

#### Blockmodels over Time

Blockmodels also make sense out of data describing social structure over time. The possibilities are numerous. Blocks can be stable over time, with the blockmodel changing. On the other hand, a blockmodel may be stable, with the blocks' memberships changing as roles and positions rotate among individuals (of course, we would need independent confirmation of such changes). Or there can be complete stability, at least for the coarse partitions into blocks together with associated blockmodel images. Successive observations of choices existed for two of the cases analyzed above, the monastery and the fraternity. The results are quite similar.

*The fraternity data.*—The stability of both the blockmodel and the blocks, after the first few weeks of maneuvering, is the main result for the fraternity. We imposed the three-block partition found for the final week on the data for each earlier week. We also computed the density of the top two choices in each block for each week (number of choices divided by number of cells in which choices could occur); we then computed the density for the bottom three choices. Figure 7 shows the results of this procedure for selected weeks: 0, 3, 5, 8, 13, 15. The development seems clear. In the initial week, the blocks show little variation in density; thus, there is little justification for asserting that the three blocks exist as distinct

WEEK	LIKE			ANTAGONISM		
	.21	.18	0	.14	.18	.31
0	.14	.25	.05	.29	.08	.12
	.10	.20	.10	.24	.12	.17
	.25	.07	.04	.02	.21	.33
3	.20	.15	0	.25	0	.20
	.20	.08	.06	.21	.12	.20
	.28	.04	.02	.02	.07	.43
5	.11	.42	0	.11	0	.37
	.21	.08	.03	.14	.04	.37
	.29	.04	.02	0	.10	.43
8	.14	.33	0	.07	0	.42
	.20	.12	.03	.07	0	.50
	.31	.04	.02	.02	.07	.43
13	.10	.42	0	.03	0	.46
	.21	.08	.03	.02	.12	.43
	.33	0	0	0	0	.50
15	.10	.42	0	.07	0	.42
	.20	.12	.03	.05	.04	.50

FIG. 7.—Newcomb's second fraternity: density of choices in each block, for selected weeks. Partition of men into blocks: (13 9 17 1 8 6 4) (7 11 12 2) (14 3 10 16 5 15). Blocks are taken from the blockmodel tested and sustained on data for week 15.

structural entities. By the third week, though, a pattern is clearly discernible; the top two blocks clearly show no internal antagonism, and the third is clearly at the bottom of a three-part hierarchy. Then, by at least the fifth week, not only the final blocks but also the final blockmodel have emerged with remarkable clarity. Thereafter the stability is marked. Now if either BLOCKER or CONCOR is applied to the data for an intermediate week, much the same blocks and blockmodel are found, whereas neither a clear blockmodel nor clear blocks emerged from their application to data for the first two weeks.

This conclusion has little interest if men are simply repeating the same

choices week after week; the argument in our section on methods was that men should be expected to change their choices over time, but within the confines of blocks that contain bonds. For example, take choices made by the top block. In week 0, these seven men made 14 Like choices (top two ranks); eight were of different persons from those whom each chose in the final week. Of these eight choices, five created bonds not predicted in the coarse  $V,F$  model (that is, the bonds were from the top block to the other block of 10 men); three added to the existing bond. By chance, one would expect an outcome like this: 10/17 in the forbidden block and 7/17 in the block allowed according to the Like image.

In order to complete this example, we repeated the comparison with week 15 for each week in turn. Define a *change* for week  $i$  to be a choice in week  $i$  that is not matched by a choice in week 15. We then summed changes for weeks 0–4 and compared them with changes summed for the 10 weeks 5–15.<sup>30</sup> Data for the first five weeks show as many changes of individual Like choices (38) as were made in the last 10. In the first five weeks, 60% of the changes fall in the zeroblock—exactly the chance expectation—but in the last 10 only 34% of the (relatively fewer) changes are in the zeroblock of the week 15 model. When parallel counts were made for Antagonism choices (the bottom three ranks), a similar picture emerged: 45% of the early weeks' choices differed from those of week 15, compared with 22% for the late weeks. For the early weeks, exactly the chance expectation (40%) fell in the zeroblock in the  $F$  image, while only 14% of the (relatively infrequent) changes in late weeks lay in this forbidden block. There is some indication, then, that individuals' choices continue changing even after a blockmodel has stabilized; in addition, after week 4, the changes—both for positive and for negative ties—conform much better to the final blockmodel.<sup>31</sup>

*The monastery data.*—Sampson claimed that, during the period to which his data refer, groupings emerged within the monastery and polarization developed among them; blockmodeling does not show such clear-cut changes over time. At period T1, before the new cohort of novices arrived, monks 4, 5, and 6 were the core men, with 9 and 8 more peripheral; 7 was isolated, but immediately became friendly with monk 16 in the new group. By T2, according to Sampson's observation, monks 1 and 2 stood out as the most respected in the whole group of 18. Then incidents multiplied as traditional discipline surfaced (even though in much milder form): for

<sup>30</sup> No data were collected for week 9.

<sup>31</sup> In further work (not reported here) on both fraternity experiments, we distinguished five blocks in the late weeks. The pattern within the top block reported in the text, when split, is stable, but the personnel assigned to each half "circulate" over time. Within the split top block, the elements  $E$  and  $S$  are the images on Like and Antagonism: a hangers-on pattern reinforced by common rejection of the lower half.

example, some new novices were shocked when shown the waxed whip for the Order's traditional mortifications. About period T3, monk 2, without explicit disapproval from the officials, instigated meetings of the novices to discuss their routine. In the formal vote for chairman of the meeting, he received 11 votes while monk 1 received three (only monk 4 was not present).

Our analysis of change is confounded by the fact that the sociometric data used here are retrospective from T4. Thus the data for periods T2 through T4 show only moderate changes. Even when the refined partition into five blocks is imposed upon the earlier data matrices, the counts in blocks are quite uniform across time periods. The sums of weighted choices, by block, for Like are shown in the top panel of figure 8.

## Like:

T2					T3					T4				
2	12	0	3	1	2	10	4	0	2	4	11	2	0	2
11	5	6	2	0	10	12	1	1	0	9	14	0	1	0
3	0	8	5	2	1	3	11	4	0	0	0	11	4	3
0	2	15	9	0	1	0	16	9	0	0	0	16	8	0
2	0	10	2	10	2	0	5	1	16	2	0	5	1	18

## Antagonism:

T2					T3					T4				
0	0	0	0	6	0	0	0	0	6	0	0	0	0	6
0	1	2	8	13	0	1	16	0	6	0	0	13	4	9
4	7	0	1	6	3	10	0	0	5	1	5	2	3	1
1	7	0	0	17	3	12	0	0	10	0	15	0	0	9
4	15	4	1	0	0	19	5	0	0	2	17	6	1	0

FIG. 8.—Monastery data: sums for Like (top panel) and Antagonism (bottom panel) choices over three time periods. (Individual choices contribute either +3, +2, or +1 to the sums, depending on tie strength.)

Since the same block memberships are used for each period and the blocks are of nearly uniform size (with either three or four members), it was unnecessary to convert to a density measure. It is clear that the blocks from T4 also stand out as distinctive units at the earlier periods (i.e., the groupings existed from the beginning). Certainly there is a

consistent decline in Like between the top two blocks (roughly, Sampson's Loyal Opposition) and the middle two (the Young Turks); however, the buildup of liking within the bottom block (which Sampson dismissed as the Outcasts) is equally striking. So also is the increasing focus of liking ties within the Loyal Opposition (largely from ties withdrawn from the other blocks) upon their leaders.

The parallel sums for Antagonism are shown in the bottom panel of figure 8. Again, the five blocks are discernible at the earlier periods. The only notable pattern changes are the concentration of antagonism from the leaders we identify in the Loyal Opposition upon the Young Turks' leaders (these Loyal Opposition leaders, in turn, are increasingly disliked by followers within the Young Turks).

Much the same changes over time are seen in the other three positive types of tie and in the negative types. Even the refined partition into five blocks yields sharp discrimination in the two earlier periods. Sampson gave careful instructions for the retrospective choices, reminding each respondent of a salient event marking that period. But it is impossible to validate them as faithful representations of perceptions at T2 and T3.

We noted in the section on methods that constraints on data collection, random fluctuation in social relations, and the differential maintenance and concealment of ties over time give rise in a natural way to "speckled" blocks (bonds) that are only partly filled with choices. Our model allows for changes in the novices' ties over time, but our hypothesis was that such changes would generally confine themselves to bonds and would not affect zeroblocks (as defined by the blockmodel for period T4). If choices for T2 and T3 are really just surrogates for T4, they should fall within the bonds specified by the blockmodel of T4; the same outcome should also hold if they are truly for a different period but the same blockmodel is hypothesized. According to the "top-two, bottom-two" cutoffs used in applying BLOCKER, among the 25 blocks in each image, about half are zeroblocks. Of the 131 choices on the four positive types of tie at T3, a third (43 choices) do not coincide with some choice at T4. Of these 43, 11 created bonds within zeroblocks. For negative types of tie, the corresponding figures are: 128 choices, 48 T3 discrepancies from T4, and 7 entries falling at T3 into blocks which are zeroblocks at T4. To put this another way, of the 2,183 *entries* that are zero in the eight data matrices for T4, the majority (59%) are in *zeroblocks*; however, of the cells that are zero at T4 but not at T3, four-fifths (80%) are confined at T3 to blocks that are bonds in the T4 blockmodel. If T2 is compared to T4, the figures for positive ties are: 130 choices, 62 discrepancies; 20 of the latter appear in zeroblocks of the T4 blockmodel. For negative ties the figures are 121, 69, and 17, respectively.

The implications seem simple and agree in general with the Newcomb

analysis above: the Sampson blockmodel is largely stable across time with respect to both blocks of men and images, and it emerged quickly after the population was constituted. To bring out the significance of these conclusions, it is worth drawing a contrast with evolution in a more classic sense. There is little support here for a naive borrowing of ideas from biological evolution, where natural selection is classically viewed as producing its results gradually on a time scale of many generations (Mayr 1963; Levins 1968). Instead, the picture conveyed, at least by these data, is one of "saltationist" evolution, where adaptations arise rapidly and with finality (Ford 1955). To take another parallel, this finding is in part consistent with the economists' view of a system of actors who maximize profits or utility effortlessly and instantaneously, though with the crucial difference that no explicit maximizing behavior has yet been identified.

#### IMPLICATIONS: CONCRETE SOCIAL STRUCTURE

Sociologists have long been tortured by their inability to specify clearly the meaning of two fundamental terms, "role" and "position" (see the excellent brief review by Catton [1964, pp. 936-43]). All agree that no cogent theory of social structure can dispense with the concepts these terms try to capture (one reason Mitchell [1969] is so soft-spoken about networks is that they seem conceptually remote from role and position). Part of the trouble, we submit, is the lack of generally applicable operationalizations: no matter how cogent the prose discussion of role and position (or cognate terms), the sense of insight fades as the writer (or his reader) tries to apply them to various concrete social structures.<sup>32</sup>

We now suggest that the purposely neutral terms employed until now—block and blockmodel—provide operational definitions for the substantive concepts of role and position. We then suggest a way to interpret concrete social structures in these terms. We require three primitive terms:

(1) *claim*—the generic term for each instance of a tie from one member of a population to another. Claims may be made either by the members themselves (as in the biomedical scientists' data) or by an outside observer (as in the Bank Wiring Room data).

(2) *type of tie* (abbreviated *toft*)—a network of all claims of a specified type. The meaning of "type" is initially (and explicitly) left

<sup>32</sup> The study by Gross, Mason, and McEachern (1958) of the school superintendency is one of the very few systematic applications of such concepts to concrete situations. Because they deal with explicit offices in explicit formal organizations, they bypass the derivation of positions from networks of relations (at the cost of other conceptual difficulties). In our terms, they moved directly to the interpretation of types of tie, using data on perceptions of rights and duties among counterpart offices; in addition, they were aided by pooled data from different concrete populations that are, by definition, parallel in organization.



## Social Structure from Multiple Networks. I

open, the postulate being that a *shared* meaning is accorded each type throughout the population.

(3) *population*—the set of persons or other actors whose claims are reported. Its membership is the choice of the investigator; it need not be restricted to a natural enclave or a set in which each actor knows most of the others.

The data are a set of networks: for each toft, the claims issued by each person regarding all others in the population are reported as a binary matrix. Most commonly the data will be sociometric, and then the claims cannot be considered to be acknowledged by the targets. With respect to claims, the choice of jurisprudential language is not accidental: Blockmodels may have natural applications to the analysis of complex lawsuits, and they seem particularly adapted to handling counterclaims and cross-claims in multiple party litigation (e.g., *Lasa per L'Industria del Marmo Societa per Azioni v. Southern Builders, Inc.*, 45 F.R.D. 435 [W.D. Tenn. 1967], *reversed*, 414 F.2d 143 [6th Cir. 1969]).

### Blockmodels as Roles among Positions

A blockmodel is a hypothesis, a representation proposed for the social structure that exists in the population's claims. Three terms can be used to describe a blockmodel:

(4) *position*—each of the sets into which the population is partitioned is a position. The technical term "block" is a synonym for this substantive concept.<sup>33</sup>

(5) *bond*—a nonzero entry from one position to another in the image for a toft.

(6) *image*—the report, in the form of a binary matrix, of the bonds on a given toft among all positions.

By its definition, a blockmodel is a *simultaneous graph homomorphism* in mathematical terms (Heil and White 1974). Mapping the population into positions requires mapping each data graph matrix simultaneously onto the corresponding image. By the definition of a homomorphic mapping, there is no bond from one position to another if and only if there is no claim from any member of the first position to any member of the other position. Thus, in the terms used earlier, each image is fully specified from its zeroblocks when the persons in a data matrix are partitioned into positions.

<sup>33</sup> We agree with Catton (1964, p. 942) that there has been an evolution toward clarity about these concepts. Our definitions can be seen as operationalizing those proposed by Larsen and Catton (1962) after their thoughtful analysis of the literature: "position . . . location of a person or a category of persons in a set of social relationships . . ."; "role . . . a pattern of collectively held expectations which define appropriate behavior for persons in a given social position."



The theoretical content of a blockmodel hypothesis, then, is a transformation of individual claims into a statement of social bonds (i.e., ties within and among positions). Reflexivity (the existence of "self-choices" or unity entries on the diagonal of a matrix) now emerges as a major substantive question about each position, rather than (as in sociometry) a mere technicality of presentation for a claims matrix. We argue (and further elaborate in Part II) that a bond has truly social content—is "received" by the target position as well as "sent" by the row position—whether or not there is a reciprocal bond on that toft from the target to the sender position.

Not only have we left open the actual semantic content of the claims on a toft; we have also not specified the general nature of tofts. One or more of the tofts might refer to rights, another to duties, another to contracts, another to evaluations, and so on through the long list of general natures proposed in various analyses of roles (once again, legal applications are obvious). It seems to us worthwhile to define position and role within as abstract and flexible a cultural framework as possible, so that the cultural content of the social structure becomes a question for empirical research rather than a matter of definition. Sustained analysis of social structure, we reiterate, requires keeping the two sides of the analysis as distinct as possible.

Our next definition is abstract in cultural terms but specific to the population and its positions in the blockmodel:

(7) *role set*—a pair of vectors for all blockmodel images: one the ordered set of rows for that position, the other the columns for that position.<sup>34</sup>

Thus a role set is simply a statement of bonds sent and bonds received, by a position, on all the tofts. Observe that a position's bond (or lack of one) to itself on each toft is part of its role set. There is no need to report the role set for each position as a pair of vectors because the set of images for the blockmodel is a complete inventory of the role sets for all positions.

### Arrays

Without further data, a blockmodel cannot be validated. One simple form of validation is to establish stability, or coherent change, in the blockmodel over time, as was done in the previous section. Another is to test the existence of bonds on a given toft by means of direct observations on inter-

<sup>34</sup> This definition is compatible with Merton's usage (1959, p. 369). A person's "status set," in Merton's terms, includes his positions in distinct blockmodels for the different populations and contexts in which he participates. In a given blockmodel an individual is indistinguishable from other members of his position; his individuality rests in his unique "status set."

actions or flows of a suitable kind. A third involves assessing the homogeneity of persons in a position with regard to suitable characteristics.

The key words are "coherent" and "suitable": validation requires substantive judgment as well as further data. Even if, with minimal substantive argument, one became convinced that a blockmodel was a valid description of a given concrete social structure, there would still be a feeling of incompleteness. The remaining question is whether the roles among positions cohere in some intelligible way in an overall structure. In Part II we propose methods for answering this question from the perspective of persons in various positions: in brief, we propose criteria for role structures (cf. Nadel 1957).

One may also search for an overall interpretation of the blockmodel from an observer's perspective. Does every toft have substantive interpretation such that the positions can be seen as an *array*<sup>35</sup> intelligible on each toft? Perhaps the simplest example would be the blockmodel  $V, W$ ; that is, the images

$$\begin{array}{cc} 1 & 0 \\ 1 & 1 \end{array} \quad \text{and} \quad \begin{array}{cc} 1 & 1 \\ 0 & 1 \end{array}$$

Where the first toft has connotations of deference, and the second of domination, one interprets the first position as the superior of the second. Or, one could begin with no knowledge of the imputed quality of the tofts but with attribute data for the individual members: for example, the fact that members of the second position shared higher standing in various other contexts and populations. Then one would infer that the second toft had the quality of deference and the first connoted domination.

As we have seen in the case studies, one must usually work with a wide variety of clues as to possible standings of positions and qualities of tofts. As more case studies are analyzed, we can hope that regularities will emerge, clarifying what sort of array is to be found in what context. "Context" must include what tofts one can and does elicit as data.

An array can take many forms, as the case studies show. The linear hierarchy or partial order is only one class of possibilities, though an important one. The array may indicate hierarchy though all bonds are reciprocated: see, for example, the Bank Wiring Room blockmodel on like and antagonism, which is a refinement of an  $E, F$  blockmodel, not a  $V, W$  one. And the hierarchical array suggested for Newcomb's fraternity is a refinement of a blockmodel with both symmetric and asymmetric bonds, namely,  $V, F$ .

<sup>35</sup> We are tempted to include "status" in this term, since many writers give it connotations we intend for "array" (see Zelditch 1968); but the confusion would be great because "status" is commonly restricted to the context of vertical rankings.

Classic balance theory suggests the  $P,N$  array as the concrete social structure seen by an observer. Here there is no question of hierarchy; instead, we have the familiar idea of rigid polarization into two opposing parties.

If one finds the blockmodel  $C,D$  on a pair of tofts, it suggests that there is no array (no overall social structure on that population); the population falls into two unrelated cliques, each of which responds only on its "own" toft. On the other hand, the blockmodel  $X,Y$  suggests a highly integrated structure; for example, let  $X$  mean "seeks as husbands" and  $Y$  mean "seeks as wife," and partition the population by sex.

There are 57 distinct pairs of images on two positions; but we do not speculate about the significance of each one. The case studies illustrate how the investigator is usually led toward discriminating more than two positions in his blockmodel hypothesis. Taxonomies of multiposition blockmodels,<sup>36</sup> and taxonomies of tofts, must be evolved as the empirical base is widened; in addition, careful attention must be given to the many relevant conceptual analyses in the literature.<sup>37</sup> Some developments along these lines will be a byproduct of Part II.

## CONCLUSION

After placing our work in the context of other network analyses of social structure, we defined operational concepts and methods and then presented examples. The concept of a block—a set of persons structurally equivalent with respect to other such sets across several types of relation—has led to the development of blockmodels for varied relations in five quite different populations. The examples illustrate the reliability and robustness of both the algorithms and their results. The blockmodel for each example was interpretable as an abstract pattern among a few aggregate units that characterized the more detailed interaction among a larger population of individuals. Independent evidence available for four of the populations supports those blockmodels. Furthermore, in each case, the blockmodel identified regularities in relational structure that had not been proposed previously. In the two instances for which data were available, we discovered that changes over time were consistent with the blockmodels. We then interpreted familiar concepts in role theory within our conceptual framework. In Part II we propose ways to test for and describe role

<sup>36</sup> François Lorrain and Joseph E. Schwartz have prepared taxonomies, from the observer's viewpoint, of blockmodels on two positions for pairs of tofts, and on three positions for one toft.

<sup>37</sup> Breiger (1974a) showed how typologies of community power structures may be translated into blockmodel hypotheses and thus actually become open to disconfirmation.

structures, a more complex level of overall integration that need not accompany regularities of the concrete sort described here.

The approaches we have described are applicable to open network populations (e.g., a contact network composed of biomedical researchers) as well as to the small, closed groups that are the traditional preserve of sociometric investigators. Blockmodel applications to a third class of populations—those comprising groups as varied as informal associations and corporate boards of directors—are reported elsewhere (Breiger et al. 1975).<sup>38</sup> This broad applicability results from the ability of blockmodels to address the problematic issue of locating the boundaries of social interaction. In our view, large, loosely bounded, "open" populations are particularly interesting subjects for blockmodeling. We suggested that our sample of 28 scientists (from Griffith's 107 respondents) reflected the structural pattern evident in the larger population. This claim deserves serious scrutiny: if it is upheld in future work, network analysis can be generalized to the study of populations far larger than those studied by earlier network techniques.

Even if evolutionary or discontinuous changes of structure can be identified and congruence with personal and cultural perceptions established, models of structure are not sufficient unto themselves. Eventually one must be able to show how concrete social processes and individual manipulations shape and are shaped by structure. A natural next step, then, is to identify how flows of information and other transactions relate to images and their change. One fundamental problem here is that many social settings may admit not just a single equilibrium outcome, but multiple alternative equilibria, with which particular equilibrium is reached depending in part on accidents of early interaction (compare the earlier comments on the Newcomb fraternity data; compare also Simon's [1957] model of Homans's theory of small groups). In turn, the interesting questions may bear on what external forces may cause a social structure to pass from one equilibrium configuration to another. A number of quite distinct traditions of model building seem to be converging on this same set of problems, including Schelling's work on "tipping" phenomena (1971) as well as models of the genetic evolution of social behavior (Boorman and Levitt 1973; Boorman 1974).

There is an important limitation in the viewpoint urged thus far. As we stated in the very first paragraph, connectivity properties in networks receive only tangential attention. The contrast between weak and strong ties should be a major factor in connectivity analyses for large populations

<sup>38</sup> L. Groeneveld (1974) also reports such an analysis. He found blockmodels, for three successive time periods, among 34 program units (containing both staff and advisors) of the National Science Foundation. The five tofts used were objective measures of mobility, authority, and interlock among these supra-individual "nodes."

(Rapoport and Horvath 1961; Granovetter 1973; Boorman 1975). Blockmodels as developed thus far deal chiefly with strong ties, whether in open-network populations or in small enclaves. It may be harder to find nontrivial blockmodels for weak ties (e.g., sociometric choices in casual or temporary groups such as those used in social psychological experiments).<sup>89</sup>

Connectivity has also been a major concern in sociometric studies of closed populations. Overlaps among cliques often have been used (e.g., Coleman 1961; Young and Larson 1965) to indicate structure (although there is no agreed-upon or powerful way to model overlaps). Blockmodels impose a partition of a population into disjoint blocks or positions (except for floaters—see Appendix A). Clique overlap is captured only by the device of cross-cutting partitions (as in the Bank Wiring Room). We focus instead on modeling overlaps between independent networks and on aggregating multiple networks while retaining their network character (the incidence of “holes” as zeroblocks). The practical tradeoff is between developing statistical measures of clique overlap (e.g., Alba 1973) on the one hand, and on the other hand interpreting aggregate patterns across multiple networks. New techniques may provide new vistas. Of particular promise is a new nonhierarchical clustering model now in the process of development by Arabie and Shepard (Arabie and Shepard 1973; Shepard 1974). The idea underlying this model is one of allowing arbitrarily given proximity data to define their own lattice of overlapping clusters in a natural and unconstrained way.

Finally, we need to enlarge the base of case studies. We particularly need data from which we can learn how new recruits to a population merge with, or change, the structure reflected in a blockmodel. In the terminology introduced earlier, when will a new recruit become a “crystallizer,” and when a “floater”? When will his initial ties of various types relegate him to a block that is marginal to the structure? Is the *E* pattern, on specified tofts, the normal context for indoctrination or socialization into an open network population? Is the *V,F* array (on positive- and negative-affect ties, respectively) characteristic of clustering and deference within stable small, closed groups? Only with a broader empirical base can general regularities be sought.

<sup>89</sup> Observation of troops and packs of primates and other vertebrates is beginning to result in systematic reports of network ties among individual animals (e.g., Struhsaker 1967; Schaller 1972; see also the analysis of Struhsaker's data in Arabie and Boorman 1973). If we were to find blockmodels fitting such data, they would suggest to us that these ties are of strength (durability, intensity) comparable to strong ties among humans and that individuality is as pronounced among animals as among people (see Wilson 1971, p. 459). Since individuality seems much more muted or even nonexistent among insects and other invertebrates, we presume that the network models discussed in this paper are less appropriate for analyzing such populations.

## APPENDIX A

## Floaters and Crystallizers

In each case study reported in this paper, BLOCKER yields a unique assignment of men to blocks, subject to two limitations. First, an assignment is termed unique even though one or more men can each be in two or more blocks, for the given unique assignment of the rest of the population; however, these "floaters" must be specified. The extreme example is a man with no ties of any type, who can therefore be in any block simply because he is irrelevant; it would be misleading to create a different overall assignment for each of his possible locations. Second, the minimum number of men any block can contain must be specified; otherwise, for example, a vacuous solution with no men in some block would have to be counted. The acronym for this parameter, MIN, is used hereafter to designate the minimum block size thought appropriate for a given population and blockmodel. For convenience, the two specifications are reported together here for each case study:

Study Author	# Blocks	MIN	Floaters: Man $i$ between or among Blocks $J$
Griffith, Maier, and Miller	4	5	23 between 1 and 3 19 and 24 among 2, 3, or 4 Any man in 4 may also be put in 3
Sampson (1)	3	3	None
(2)	5	3	None
Roethlisberger et al. (1)	6	2	8 among blocks 1, 4, and 6 13 between 1 and 2
(2)	3	4	1, 9, 12, 13 between 1 and 2 8 between 1 and 3
Newcomb	3	4	2 between 1 and 2 5 between 1 and 3
White	3	3	8 and 16 among all three blocks

For a detailed analysis of assignments and floaters, see Heil and White (1974).

## APPENDIX B

## Attributes and Blocks in the Bank Wiring Group

In the dual manner elaborated by Breiger (1974*b*), reasons can also be regarded as "individuals," the cross-tabulation transposed, and CONCOR applied again. (First, though, in order to equalize the influence of different wiremen on the result, the column of counts for a wireman was converted

to percentages.) The partition of reasons into three sets was (1 4 5 10) (7 8 9 11) (2 3 6 12), with reasons numbered according to the order in which they are listed in the source (table XXIX there). The sum of counts in the original matrix between each of a set of reasons and each of the men on one side of the split above is

16	23
3	106
33	43

where the top row is the left set of reasons, etc. The wiremen in the back clique usually cited reasons from the second set; the wiremen from the front, almost never. These reasons, 7, 8, 9, 11, associated with the wiremen W6, W7, W8, W9, include "Waiting for Solderman" and "Waiting for Inspector," but not "Waiting for Trucker." Roethlisberger and Dickson (1939) noted that the "waiting" class of reasons is highly subjective and "includes reasons which placed the blame for delays upon people instead of upon things" (p. 431). This maximal taking advantage of discretion, coupled with resentment at equals and superiors (soldermen and inspectors, not truckers), fits in with the tendency to fight over windows within the back clique (see fig. 5) and conforms with R-D's finding that the back clique "resented any show of superiority more than the others did because they were in the most subordinate position" (p. 523). Reason 9 is somewhat a technicality, as wireman W9 was assigned to making repairs (p. 432). Reason 11 is not discussed in the source (see p. 432).

If the back clique's reasons tended to fall into an imputed "resentment" category, the front clique's reasons indicated "playing the game" or perhaps a "craftsman's ethic." In roughly one-fifth of all instances of time allowances for the front clique, its members gave no reason at all, while the comparable figure for the back clique was 8%. Mostly, the reasons of the front clique are objective, as R-D define that term (e.g., cable reversals were detected by inspectors' tests). However, they label "defective wire" and "defective solder" as *subjective* reasons. It may be argued that these reasons are different from those in the "waiting" class (reasons 6, 7, 8) in that they represent a "professional" judgment.

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# Development Towns in Israel: The Role of Community in Creating Ethnic Disparities in Labor Force Characteristics<sup>1</sup>

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This paper investigates the contribution of community to ethnic stratification in Israel. We show that "development towns," a category of new settlements established to achieve population dispersal and immigrant absorption, have influenced (a) the areal distributions of different ethnic (country-of-origin) groups, (b) the tendency for each group to be concentrated in certain industries, and (c) the occupational opportunities available to the members of an ethnic population. We also review the relevance of this analysis for understanding ethnic stratification in America.

## INTRODUCTION

Urban communities in Israel have not attracted particular attention as research sites. In part, this is because there are more novel settlement patterns, found only in that country, such as the kibbutz (agricultural collective) and the moshav (small-holders' cooperative). These forms of rural community have been studied intensively, in regard to work organization (Spiro 1970), productivity of the economy (Barkai 1974), decision making (Cohen 1968), family structure (Talmon 1972; Weintraub and Shapira 1971), and child-rearing practices (Bettelheim 1969). It is the case, however, that Israeli *urban* settlements provide convenient sites for investigating a number of topics that are of interest to urban specialists, especially issues which relate to planned community development. The country's new towns, which were established in large numbers beginning in the late 1940s, permit one to evaluate the various strategies that have

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been employed in attempting to put together a viable community *de novo*: which industries can be implanted successfully in outlying areas, what mixes of settler backgrounds will produce socially integrated communities, and what kinds of local political institutions tend to operate effectively in the early stages of development of a new settlement.

The pattern of Israel's urban growth has been a matter of much concern to governmental authorities. At the time of the country's founding, in 1948, 63% of the population was concentrated in three main cities—Tel Aviv, Haifa, and Jerusalem—and in their immediate environs. Exacerbating this population imbalance, a vast immigration in the early years of the state threatened to create massive congestion in the urban centers, taxing their physical plants, the absorption potentials of their labor markets, and their social service capabilities, unless many of the immigrants could be motivated to settle outside the metropolitan regions.

Israel is a society with a considerable tradition of social planning and centralized decision making, dating to the imperatives of existence in the prestate period. The government, consequently, was in a position to undertake far-reaching decisions concerning population redistribution, and did so within months of the establishment of the state, committing extensive resources to this task. The instrument to accomplish population redistribution was to be a network of small and medium-sized urban settlements, located away from the densely populated coastal plain; these settlements have since become known as "development towns."

The new towns have been a focus of much concern. They are populated by recent immigrants, particularly from less developed lands. Many are isolated, outside the main stream of Israeli society geographically as well as socially. For these reasons, the settlements constitute, in many respects, a second and inferior Israel. What we wish to accomplish here in regard to the towns is twofold: we want to describe their evolution and their problems, and in doing so we intend to view them as strategic sites for addressing an issue in ethnic stratification—the effect which community can have in producing ethnic disparities in labor market characteristics. Specifically, we consider how the creation of these settlements has influenced (a) the areal distributions of different ethnic (country-of-origin) groups, (b) the tendency for each group to be concentrated in certain industries, and (c) the occupational opportunities available to the members of an ethnic population. In the concluding section we review the relevance of these considerations for understanding the role of community in ethnic stratification in America.

#### DEVELOPMENT TOWNS IN ISRAEL

Settlement patterns in the Yishuv, the Jewish community in prestate Palestine, were influenced by an ideology which invested land reclamation and

Jewish manual labor with great significance. The erection of agricultural settlements was therefore consistent with the tenets of labor Zionism, as well as having a basis in strategic considerations, in that it established Jewish rights to land in outlying regions through purchase and cultivation. Yet, as Cohen (1970, pp. 2-7) has remarked, in modern times the Jewish population in Palestine was never more than 30% rural, a fact that was ignored in early Zionist writings, in which little attention was given to the role of cities in a modern economy, or to their potential importance in the state which was to be created.

This situation changed abruptly following the establishment of Israel. In the three years subsequent to the termination of the British mandate, an influx of hundreds of thousands of immigrants, fleeing from persecution in Europe and the Arab countries, more than doubled the population of the state. In prior occupations, these immigrants had been predominantly artisans and small shopkeepers; they came to Israel as refugees, not out of an ideological conviction which might sustain them while adapting to the harsh life of an agricultural laborer. It was soon recognized by governmental authorities<sup>2</sup> that residential quarters would have to be constructed in large numbers, in urban areas, to accommodate the immigrants. In order to relieve the congestion in Tel Aviv and Haifa resulting from very rapid population growth, and open the hinterland to settlement, industrialization, and mineral exploitation, the government adopted a policy of establishing new towns principally outside the periphery of Israel's metropolitan centers.<sup>3</sup>

<sup>2</sup> "The authorities did not turn their efforts to urban development out of their own free choice; this decision was forced upon them by the circumstances of immigration and settlement which emerged after the establishment of the state" (Cohen 1970, p. 33). Within two months of the creation of Israel, a National Planning Department was opened in the Ministry of Labor. Its principal objective was to initiate comprehensive settlement planning on a country-wide basis, as well as on a regional and local level (Brutzkus 1964, pp. 12-13).

The mechanics of immigrant settlement involves the cooperation of a private institution and several governmental ministries. The Jewish Agency, which fulfilled the tasks of refugee transportation and settlement during the mandate period, is still responsible for attracting new immigrants and for the initial stages of their absorption. The responsibility for planning and developing new towns is now divided mainly among three ministries—Labor, Housing, and Commerce and Industry—coordinated by an interministerial committee (Lichfield 1971, 1:3.2-3.10).

<sup>3</sup> The dependence of immigrants on public agencies offered a unique opportunity for altering the settlement pattern existing at the creation of the state. Between 1949 and 1967 seven schemes for population dispersal were drawn up and revised by the Planning Department, each scheme looking 10-20 years into the future. As summarized in Lichfield (1971, 1:3.7), the thrust of the government's policy has been to (a) reduce the urban concentration in the coastal belt between Tel Aviv and Haifa, (b) disperse settlements throughout the country in order to develop the land, (c) establish balanced regions through an integrated hierarchical structure of interdependent urban and rural settlements, and (d) aid in absorbing and assimilating large numbers of immigrants by providing housing and employment.

There is no consistent definition of a development town in the literature on Israeli settlements, though the intention is to signify urban settlements which lie outside metropolitan regions and which were established by design, since 1948, and with considerable governmental assistance.<sup>4</sup> The 1961 Israel Census of Population, the most recent census available, does not employ this term and distinguishes instead between veteran and new communities. "New" communities include those founded after 1948, whether in outlying areas or in the suburban periphery, as well as a number of older settlements which have experienced the major portion of their population growth since the creation of Israel. According to the census, 38 new settlements<sup>5</sup> existed in 1961, with a combined population of 398,000. Other governmental agencies use the term "development town" to refer to new communities distant from metropolitan areas; yet there are differences with respect to the settlements each includes in this category. A report written for the Ministry of Labor (Smith 1972, p. 17) lists 24 communities,<sup>6</sup> with 246,000 residents in 1961. A report prepared for the Ministry of Housing (Lichfield 1971, 2:1) records 25 settlements, with 289,000 inhabitants in 1961. Nongovernmental researchers also differ in terminology and settlement classification: Spiegel (1966, p. 34) cites 24 "new towns," with a combined population of 271,000 in 1961. Because suburban settlements are excluded, her specification corresponds to the notion of a development town. Amiran and Shachar (1969, table 4) list 28 development towns as of 1961, with a total population of 312,000.

The differences among these classifications result from decisions concerning which of the communities existing before 1948 should be consid-

<sup>4</sup> In many instances the founding of a new town was preceded by comprehensive planning of physical facilities, industrial composition, and population growth. During the years of mass immigration, the tendency was to provide refugees who lacked skills or capital with housing and employment in these settlements. Various governmental agencies were involved in building the towns, and attracting industrial enterprises through the provision of tax incentives and loans on favorable terms. Politically, local municipal responsibility has generally been slow to develop, all decisions initially resting with the central government. For many years afterward a division of power over municipal affairs existed between the central and local authorities (Lichfield 1971, 1:4.12-4.15; Matras 1973, pp. 5-9).

<sup>5</sup> This number refers to urban communities (31) and large villages (seven). An urban community is one containing more than 2,000 inhabitants and having at least two-thirds of its labor force not engaged in agriculture. A large village is a settlement with more than 2,000 inhabitants which does not satisfy the industrial requirement. Large villages are included in our tabulation because five of the seven settlements appear on some list of development towns. Settlement definitions are reported in Israel Central Bureau of Statistics (1966, pp. 21-23; 1965, pp. 61-63).

<sup>6</sup> The population figures which follow are adjusted to 1961 so that they will be comparable to the census values. For the same reason, settlements with fewer than 2,000 inhabitants in 1961 have been deleted from the tabulations.



ered "developmental," and which settlements that were initially in this category should be viewed as having outgrown the label, in that they no longer receive extensive financial assistance from the central government. Where we are in a position to calculate indices from census data, we will use the list of communities proposed by Amiran and Shachar. Their definition covers settlements that were established after 1948, whether founded on entirely new sites or brought into being as a result of an influx of new populations into towns that had been vacated by Arab residents (Amiran and Shachar 1969, p. 1). Suburbs are omitted from the list, although one town (Tirat HaKarmel), which now is in the Haifa metropolitan region, is included. Also, they retain settlements that have progressed successfully, a consideration that is important, since we wish to discuss ethnic composition and industrial structure in the full array of communities which were planned to achieve population redistribution and immigrant absorption. Where we refer to statistics from other studies, the development towns to which the figures pertain will be somewhat different from Amiran and Shachar's list, in accordance with the comments in the preceding paragraph.

The growth of development towns, and the impact which these settlements have had on the distribution of the urban population, is reported in table 1. The entries in the second row reveal that the considerable population expansion in the country since 1948 has been accompanied by an increase in proportion urban, from 73.3% to 84.4%. The decline in the early years of the time series, incidentally, is due to the establishment of many development towns during this interval; initially, they were too small to be categorized as urban in the census. For our purpose, a more important trend concerns the massive population increase in the development towns and the population redistribution resulting from this growth (last row), even during a period when all urban communities were expanding. Indeed, the last two rows of the table, in combination, suggest that the redistribution policies of the government were most successful, since the proportion of the urban population residing *outside* the three metropolitan areas increased from 14% in 1948 to 48% in 1972, with the bulk of this change deriving from the growth of development towns.

#### The Concentration of Immigrant Groups in Development Towns

The growth of new towns, and the population redistribution which was occasioned by this growth, did not arise principally from internal migration. Rather, new immigrants were encouraged to settle in development towns, with subsidized housing, low-interest loans, and the promise of employment serving as inducements. The outcome of this process was that,

TABLE 1  
DISTRIBUTION OF THE URBAN POPULATION IN ISRAEL, 1948-72

	1948	1951	1954	1957	1960	1963	1966	1969	1972
Total population in Israel (in thousands) ..	786	1,557	1,718	1,975	2,150	2,430	2,657	2,919	3,232
Percentage urban .....	73.3	67.7	70.6	75.6	76.7	79.7	81.8	82.5	84.4
Percentage of urban population in:									
Tel Aviv .....	43.1	32.4	29.5	24.8	23.0	20.4	17.9	15.9	13.3
Haifa .....	17.2	13.9	12.8	11.2	10.9	10.1	9.6	8.9	8.0
Jerusalem .....	14.6	13.2	11.9	10.2	9.9	9.3	9.0	11.8*	11.2*
Suburb† .....	11.1	12.2	15.1	16.0	18.0	18.1	19.1	19.4	19.4
Veteran settlements‡ .....	12.1	16.5	17.3	20.8	19.8	21.6	23.2	22.4	26.4
Development towns§ .....	1.9	11.8	13.4	17.0	18.4	20.5	21.2	21.6	21.7
Total urban .....	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0

Sources.—Israel Central Bureau of Statistics (1963a; 1973a, pp. 38-39); Amiran and Shachar (1969, Tables 1, 4); and unpublished tabulations, Central Bureau of Statistics.

\* Includes population of East Jerusalem, annexed in 1967.

† Includes 10 communities: Bnei Brak, Bat Yam, Givatayim, Holon, Ramat Gan, Kfar Ata, Nesher, Qiryat Bialik, Qiryat Yam, and Qiryat Motzkin. Tirat Hakarmel, which is classified in the Israel census as a suburb of Haifa, is listed by Amiran and Shachar (1969, table 4) as a development town and is categorized with those settlements.

‡ Includes all urban settlements except the three main cities, suburbs, and the development towns on Amiran and Shachar's list.

§ Includes the 34 communities (existing in 1967) on Amiran and Shachar's (1969, table 4) list of development towns.

in 1961, 67% of the population<sup>7</sup> in development towns consisted of recent immigrants to the country, arrivals since 1948, in comparison with 45% in the total Israeli population (36% in the main cities). Such great concentrations of recent immigrants, in communities that are also growing rapidly, could be expected to create massive problems of acculturation, and in the provision of housing, suitable jobs, and the many social services which a refugee population would require. Very great problems still exist in these towns; some will be discussed in the concluding section. Yet the settlement program was conducted within a framework of extensive assistance to the new communities by the central government. Also, the facts that Zionist ideology encouraged the ingathering of Jews, that many of the "veterans" were themselves immigrants in an earlier decade, and that Israeli culture was in the process of being molded during the initial years of the state, made for tolerance of the variety of life-styles brought by the immigrants, and a willingness to accept them as Israelis even while they were only marginally acculturated to the mores of the society.

We wish to focus on how development towns have patterned the industry affiliations of the immigrant groups and influenced their consequent occupational distributions. For this purpose it will be useful to delineate two aspects of residence location: representation of a population group in a settlement category, and variability of its concentration among settlements within the category. The latter factor speaks to the possibility that individual communities may "specialize" in particular industrial activities.

In regard to the first point, we note that not only are development towns places of concentration of recent immigrants, but that great differences exist between these settlements and other urban communities with respect to the origins of their foreign-born populations. Continent-of-origin figures are reported in columns 1 and 2 of table 2. From the entries in the two top panels it is evident that the population in development towns has been drawn, to a very considerable extent, from among Asian-African immigrants. They comprise 66% of the foreign born, versus 29% for Europeans. In reference to their percentages in Israel, Asian-Africans are overrepresented by a factor of 1.65, Europeans underrepresented by a factor of 0.54 (1.00 = representation at the same rate as in the total population). This suggests that the industrial structures of the towns should be especially pertinent to understanding the labor force characteristics of Asian-African immigrants.

An equally important matter concerns differences among settlement types in regard to the *variability* of representation of the immigrant streams in the individual communities. This point refers to the fact that the continent-of-origin groups are not dispersed evenly, but tend to be concentrated

<sup>7</sup> Since many of the Israeli born would be young children, the percentage foreign born in the adult population is much greater.

TABLE 2

FOREIGN-BORN POPULATION IN URBAN SETTLEMENTS,\* BY CONTINENT AND MAIN COUNTRIES OF ORIGIN, 1961

SETTLEMENT CATEGORY	CONTINENT OF ORIGIN			COUNTRY OF ORIGIN†									
	Asia- Africa	Europe	Turkey- Iran	Iraq	Yemen- Aden	Morocco- Algeria- Tunisia	Egypt- Libya	USSR- Poland	Germany- Austria	Czechoslovakia- Hungary	Romania	Bulgaria- Greece	Total
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	
	Percentage of the Foreign Born												
Total Israel .....	40.0	52.8	6.6	10.2	5.2	13.1	5.0	26.6	4.5	4.7	13.0	4.1	93.0
Main cities‡ .....	27.6	64.6	7.3	7.6	2.5	7.2	3.0	34.0	5.7	4.8	13.1	7.0	92.2
Suburbs§ .....	31.9	61.8	3.8	14.5	2.9	34.4	6.5	34.0	4.9	5.9	13.1	3.6	93.6
Veteran settlements¶ .....	40.2	53.4	6.2	12.5	9.5	6.7	5.3	26.3	4.3	5.3	16.0	1.6	93.7
Development towns** .....	66.0	28.7	9.2	12.4	5.7	32.5	6.4	10.3	1.1	2.6	12.0	2.5	94.7
	Percentage of the Foreign Born as a Fraction of Total Israel Value												
Main cities .....	0.69	1.23	1.11	0.75	0.48	0.55	0.60	1.28	1.27	1.02	1.01	1.71	...
Suburbs .....	0.80	1.17	0.58	1.42	0.56	0.31	1.30	1.29	1.09	1.26	1.01	0.88	...
Veteran settlements .....	1.00	1.01	0.94	1.23	1.83	0.51	1.06	0.99	0.96	1.13	1.23	0.39	...
Development towns .....	1.65	0.54	1.39	1.22	1.10	2.48	1.28	0.39	0.25	0.55	0.92	0.61	...
	Index of Variations††												
All urban settlements (N = 47) .....	.42††	.42††	.26	.37	.58	.50	.20	.20	.20	.16	.21	.23	
Suburbs (N = 10) .....	.27	.28	.14	.34	.14	.31	.21	.20	.19	.10	.16	.16	
Veteran settlements and main cities (N = 18)‡‡ .....	.19	.20	.15	.26	.17	.17	.17	.11	.10	.10	.19	.13	
Development towns (N = 19) .....	.33	.31	.31	.47	.86	.44	.22	.22	.10	.22	.24	.21	

Sources.—Israel Central Bureau of Statistics (1963*b*, tables 1 and 2; 1964, table 10); Amlran and Shachar (1969, table 4).

\* Computations pertain to communities with more than 5,000 residents in 1961.

The grouping of countries of origin is necessary because our raw data (Israel Central Bureau of Statistics 1963b, table 2) are presented in this fashion.

Difference between 100 and value in this column is accounted for by foreign born from other countries, and origin-unknown population.

Tel Aviv, Haifa, Jerusalem.

See n. † of table I.

### Communities of Hadar-Ramatayim, Herzliya

on, Rehovot, Ramat Hasharon, Ra'anana,  
The Committee of Ov-Yotz, Eilat, Ashdod

Communities of Or Yehuda, Eilat, Ashdod, H-Azra, Ramla. These are the settlements.

in some settlements and underrepresented in others. To measure variability we constructed an index that is analogous to the coefficient of variation (Stigler 1966, pp. 294–95). For an array of percentage values  $\{p\}$  we assess variability by the statistic  $IV(p) = SD(p)/\sqrt{\bar{p}(1 - \bar{p})}$ . This index compensates for the fact that the standard deviation of a percentage ( $SD[p]$ ) is constrained to be small when the average is very large or very small, and thereby enables comparisons to be made among sets of percentages with different means. Indices of variation in the proportion Asian-African are presented for the various settlement categories in the bottom panel of column 1.<sup>8</sup> The large value for development towns (0.33) means that there are greater differences in ethnic composition among settlements in this category than among either veteran communities or suburban settlements. The relevance of this point is that disparities in industrial structure which exist among development towns may correspond to the variation in location of the continent groups, thereby exposing Asian-African and European settlers to very different industries and occupational opportunities.

Continent of origin is hardly a sensitive measure of cultural homogeneity, although this distinction is a salient one in Israeli life. *Country* of origin is a more important consideration; it signifies language and life-style, and it is at this level that ethnic identity is commonly specified. Columns 3–12 report representation values by settlement type for several ethnic groups (top panel), and these figures standardized by the respective national percentages (middle panel). It is apparent from the latter that each Asian-African group is overrepresented in development towns (entry  $> 1$ ) and, with few exceptions, underrepresented in metropolitan centers and suburbs. The reverse situation characterizes the locations of most European populations. Superimposed upon this pattern are some sizable differences among the individual ethnic groups: the concentration of Moroccan-Algerian-Tunisian immigrants in development towns is especially high (2.48 times their representation in Israel); the presence of Yemenites (1.10) indicates near equality to their percentage in the country, while immigrants from Germany-Austria are grossly underrepresented in the towns (0.25).

The *variation* in ethnic group concentration also speaks to the matter of labor force characteristics of these populations. The index values reported in the lower panel of table 2 show, in every instance, greater variability among development towns than among settlements in the other categories. It also appears that the individual Asian-African ethnics are apt to concentrate in certain towns, while the European groups are more evenly dispersed (compare cols. 3–7 with 8–12, last row). (Indeed, the

<sup>8</sup> Since  $IV(p) = IV(1 - p)$ , the index of variation is identical for the two continent-of-origin groups, except for the effect of the origin-unknown population.

index values for the Asian-African populations tend to be larger in every settlement category.) As we noted for the continent-of-origin groups, these observations suggest that whatever differences in industrial structure are present among development towns, they may have considerable impact on the industry affiliations of a number of the ethnics, by exposing them disproportionately to particular labor market opportunities. Any such effect should be greater for the Asian-African populations, because these groups are overrepresented in the towns, and because they exhibit a larger variation than Europeans in concentration by settlement.

#### Time of Immigration and Concentration in Particular Development Towns

Although it is a digression from our main theme—to show the impact of development towns on the industrial and occupational opportunities of the various ethnics—it is of interest to understand how the concentration of individual immigrant groups in certain settlements came about. The relevant facts are: (a) the ethnic groups arrived in Israel in large numbers in different years, and (b) the development towns were established and experienced their periods of maximum population growth at different times.

The tendency for the country-of-origin groups to differ in year of arrival to Israel is documented in table 3. The first two columns report immigration to Israel by continent of origin; column 3 shows the ratio of European to Asia-African immigrants. There are several clusters of years during which new arrivals came disproportionately from one continent. The bulk of immigrants in the prestate period came from European countries, a trend which continued into the first two years of the state's existence, when the survivors of the Nazi extermination camps comprised the majority of newcomers. Following this period there was an eight-year interval when immigrants came from the Arab countries at approximately three times the rate from Europe. The ancient Jewish communities of Iraq, Yemen, and Aden moved to Israel, practically in their entirety, in these years. Several additional shifts followed in the dominance of a continent as a source of immigrants. These shifts frequently accompanied political upheavals in particular lands, and reflected the impact of those events on their Jewish populations.<sup>9</sup>

The dominant period of arrival of immigrants from the individual countries is more relevant to the issue of ethnic group concentration in different towns. In columns 4–7 we present immigration distributions for a few

<sup>9</sup> Immigration from Morocco intensified after it achieved independence in 1956. Following the Hungarian revolution in 1956, tens of thousands of Jews fled to Austria; many eventually came to Israel. Recent immigration from Poland is related to the introduction of an anti-Semitic campaign following the Six-Day War in 1967.

# Development Towns in Israel

TABLE 3

IMMIGRATION TO ISRAEL BY CONTINENT AND SELECTED COUNTRIES OF ORIGIN,  
1932-72 (THOUSANDS)

YEAR	CONTINENT			COUNTRY			
	Europe (1)	Asia- Africa (2)	(1)/(2) (3)	Romania (4)	Bulgaria (5)	Iraq (6)	Iran (7)
1932-38 .....	175.7	17.5	10.49	10.6	1.1	2.9	1.7
1939-45 .....	63.0	14.2	4.50	8.9	3.2	1.5	0.4
1946-48* .....	48.5	2.0	24.25	16.2	1.2	0.0	0.0
1948†-49 .....	200.3	123.7	1.61	31.3	35.1	1.7	1.8
1950-51 .....	136.0	207.0	0.66	86.6	2.1	121.6	20.0
1952-53 .....	10.3	25.1	0.40	3.8	0.8	1.6	5.3
1954-55 .....	5.9	49.8	0.12	0.4	0.4	0.6	0.6
1956-57 .....	48.8	77.9	0.63	1.6	0.2	0.4	1.8
1958-59 .....	30.7	20.0	1.55	17.6	0.2	0.2	7.2
1960-61 .....	42.3	29.4	1.45	28.8	0.3	0.3	1.4
1962-63 .....	34.1	91.2	0.37	20.0	0.2	0.3	5.1
1964-65 .....	48.2	36.9	1.33	23.4	0.1	0.1	5.6
1966-67 .....	14.4	15.2	0.95	‡	‡	‡	1.4
1968-69 .....	27.4	30.2	0.91	‡	‡	‡	3.1
1970-71 .....	54.2	23.7	2.28	‡	‡	‡	3.1
1972 .....	48.1	7.6	6.33	‡	‡	‡	0.9

Sources.—Sicron (1957, pp. 2, 6); Israel Central Bureau of Statistics (1973b, pp. 16-19).

\* Until end of the mandate period.

† From day of establishment of Israel (May 14, 1948).

‡ Data are not published.

countries of origin to illustrate the range of patterns that exist concerning time of arrival in Israel. These distributions show that for some ethnics (e.g., Bulgarians, Iraqis) immigration to Israel was compressed into a very brief interval, although the specific years may vary among the groups. Other ethnics (e.g., Iranians) show less concentration in time; their populations have arrived in sizable numbers over much of the lifetime of the state. Still other origin countries (e.g., Romania) exhibit arrival patterns which have several modes.

The tendency for period of immigration to have influenced the location of the individual ethnics in different development towns can be investigated by comparing the concentration of a group in the settlements that were growing rapidly during its years of maximum immigration, with the group's representation in all development towns. To pursue this matter we specified periods of substantial immigration to Israel for each ethnic group, and periods of rapid population growth for every town. A period of substantial immigration was defined as several adjacent years during which at least 50% of the group's population in 1961, the census year, arrived in the country. Alternatively, an ethnic group could have more than one period of substantial immigration if at least one-quarter of its population in 1961 arrived in each time period. The periods were specified so as to maximize

differences among the ethnic groups in terms of this classification. That is, for analytic purposes, we wanted to define several time periods, with the individual ethnics dispersed among them. Due to annual fluctuations in the size of the total immigration stream, our intervals vary in duration from two to five years. The four time periods that were specified, together with the ethnics which experienced substantial immigration in each, are reported in table 4.

TABLE 4  
ETHNIC GROUP CONCENTRATION IN DEVELOPMENT TOWNS,\* BY PERIOD  
OF ARRIVAL TO ISRAEL, 1961

Ethnic Group with Large Population Proportion Immigrating to Israel in Indicated Period†	Proportion of Ethnic Group's Development Town Population That Is in Settlements Which Grew Rapidly in Time Period‡ (1)	Expected Proportion of Ethnic Group in Rapidly Growing Development Towns§ (2)	Overrepresentation of Ethnic Group in Rapidly Growing Development Towns: (1) - (2) (3)
1948-49			
Lybia* . . . . .	.215	.288	-.073
Turkey* . . . . .	.350	.288	.062
Yemen . . . . .	.717	.288	.429
Bulgaria . . . . .	.736	.288	.448
Poland . . . . .	.391	.288	.103
1950-51**			
Egypt (Lybia)†† . . . . .	.834	.570	.264
Iran* . . . . .	.438	.570	-.132
Iraq . . . . .	.799	.570	.299
Romania . . . . .	.701	.570	.131
1954-56‡‡			
Morocco* . . . . .	.501	.413	.088
1957-61§§			
Egypt* . . . . .	.260	.246	.014
Poland . . . . .	.270	.246	.024
Romania . . . . .	.291	.246	.045

SOURCES.—Israel Central Bureau of Statistics (1973b, table 3; 1964, table 10; 1963b, table 2); Amiran and Shachar (1969, table 4).

\* Computations pertain to the 19 development towns with populations exceeding 5,000 in 1961.

† "Large population proportion" is defined as more than one-half of the group's population in Israel in 1961 arriving in indicated period (if ethnic group appears in one time interval), or more than one-quarter of group's population arriving in the period (if ethnic group appears in two time intervals).

‡ A "rapidly growing town" is one with more than one-third of its 1961 population accounted for by expansion in the indicated period (if settlement appears in one time interval), or more than one-quarter of its population accounted for by expansion in the period (if settlement appears in more than one time interval).

§ Entry is the proportion of the total population in development towns in 1961 that is in settlements which grew rapidly in the time period.

|| Rapidly growing development towns in this period: Akko, Lod, Ramla, Rosh HaAyin, Yehud.

\* In some census data used in our calculations (Israel Central Bureau of Statistics 1964, table 10) this ethnic group is paired with another which did not have a high immigration rate in the period.

\*\* Rapidly growing development towns in this period: Akko, Afula, Ashkelon, Beer Sheva, Or Yehuda, Tiberias, Tirat Hakarmel, Zefat.

†† Egypt and Lybia, which are grouped in the census data, each satisfied the criterion for inclusion in this time period.

‡‡ Rapidly growing development towns in this period: Ashkelon, Beer Sheva, Bet Shaan, Bet Shemesh, Dimona, Qiryat Shemona, Yavne.

§§ Rapidly growing development towns in this period: Beer Sheva, Dimona, Elat, Qiryat Gat.



Rapidly growing development towns were defined as places which obtained at least one-third of their 1961 populations in the indicated time period. Alternatively, a town was considered to have multiple periods of rapid growth if it obtained at least 25% of its 1961 population in each of several intervals. Using these definitions we calculated the proportion of an ethnic group's development town population in 1961 which resided in settlements that were growing rapidly when its members had a high rate of immigration to Israel. This value is reported in column 1 of table 4. In column 2 we present the proportion which would reside in these settlements if the group were represented equally in all development towns. Column 3 shows the difference between the entries in the preceding columns, and measures the extent of overrepresentation in the towns that were expanding rapidly.

These calculations support the argument that the ethnics tend to be concentrated in settlements which have growth histories that parallel their immigration distributions. Eleven of the 13 entries in column 3 are positive, indicating overrepresentation in rapidly expanding towns. Also, the two instances in which a column 3 entry is negative refer to situations where the ethnic group under consideration is paired in the census data with a second group, one lacking a high immigration rate in the referenced time period. In these cases our calculations cannot provide a sensitive test of the thesis.<sup>10</sup>

#### THE INDUSTRY DISTRIBUTIONS AND OCCUPATIONAL STANDINGS OF THE ETHNIC GROUPS

To this point our argument has been to the effect that as a consequence of a variety of arrival times in Israel by the individual ethnic groups, in combination with different periods of rapid population growth by the development towns, each immigrant population tends to be concentrated in particular settlements. The next consideration that we address concerns the tendency for a development town to "specialize" in certain industries, in the sense that its firms are grossly overrepresented in the labor force of the settlement, and the effect which this situation has on the industry affiliations of the individual ethnics.

There are a number of reasons for a lack of industrial diversity within development towns. First, most of the settlements are quite small; only six of the 28 on Amiran and Shachar's (1969, table 4) list had more than 15,000 inhabitants in 1961. This fact, alone, limits the number of industries

<sup>10</sup> In contrast, three of the four largest entries in column 3 pertain to groups (Yemen, Bulgaria, Iraq) whose arrival patterns are characterized by "spikes"; that is, practically their entire populations emigrated during a very brief interval. This situation permits the cleanest test of the correspondence.

which a town can support. Second, the national government has followed a policy of extending incentives for certain kinds of industries to locate in development towns. Third, the preferred industries tend to have large plants;<sup>11</sup> this also serves to reduce the variety of firms in a settlement.

Governmental encouragement of industry is carried out through a plan of financial incentives administered by the Ministry of Commerce and Industry. For locating in a development town, firms are granted tax reductions and low-interest loans for site acquisition, site development, on-the-job training of workers, and working capital (Lichfield 1971, 1:3.13). The kinds of industries that have been given preference are ones which either exploit the resources of a region—food-processing plants in agricultural locales, mining and chemical manufacturing in towns near the Dead Sea—or ones which, while neutral to location, are labor intensive and provide many jobs at a low initial capital cost. Textile manufacturing has been the most favored industry; the short training period for spinning and weaving jobs makes these tasks especially suitable for accommodating low-skill immigrants.

The impact of the factors which make for industrial concentration can be illustrated by reference to the economies of a few development towns. In Qiryat Shemona, 71% of industrial employment is in textile manufacturing; this figure represents one-fourth of the total labor force in the city<sup>12</sup> (Zarchi and Shiskin 1972, pp. 61, 84). In Dimona, textile plants also dominate the manufacturing sector: 96% of industrial workers, representing 50% of the labor force, are in these enterprises. Ashqelon specializes in food processing (46% of industrial employment); Bet Shemesh manufactures transportation equipment (40% of the industrial labor force); Afula weaves textiles (57% of the industrial work force); and the economy of Yeruham, a new development town, is based principally on chemical and mineral processing (92% of industrial employment). While it is true that the preceding examples depict extreme instances of unbalanced economic structures, they only exaggerate what is an evident tendency.

<sup>11</sup> There is an evident tendency for plant size in development towns to exceed plant size in other settlements. In 1967, 18.2% of plants in development towns employed 100 or more persons; the comparable figure for the country was 4.4%. In regard to the labor force, 69% of industrial employment in the towns was in these large plants, versus 43% for the country (Berler 1970, p. 115). To a considerable extent, the concentration of large plants in development towns is due to the kinds of industries which have located there: food processing and packing, textile manufacturing, potash and chemical works, and cement products. Spiegel (1966, p. 51) adds that the government has preferred to negotiate with a few big enterprises rather than with many small ones.

<sup>12</sup> This statistic is for 1971. Other figures in this paragraph pertain to 1968, and are from Lichfield (1971, 3:202, 160, 122, 41, 216).

As a result of community differences in industrial composition, the individual ethnic groups tend to be overrepresented in certain activities. Yemenites are concentrated in textile manufacturing (three times their representation in the Israeli population), Moroccans are in mining (four times their representation), Lybians manufacture cement products (four times their representation), and immigrants from Algeria-Tunisia are employed disproportionately (by a factor of three) in wood product industries.<sup>13</sup> The overrepresentation of those immigrant groups can be attributed to the particular development towns where they reside. Veteran settlements play much the same sort of role, but tend to expose *European* ethnics to certain industrial sectors. Germans are overrepresented (by a factor of two) in chemical and petroleum processing, an industry which has a major center in the Haifa Bay region, where this group is concentrated. Bulgarians, who reside principally in the Tel Aviv metropolitan area, are overrepresented (by a factor of two) in machinery and metal products manufacturing, an economic sector with a strong representation in this region.

To inquire in a more systematic fashion into the impact of community on the industry affiliations of the ethnic groups, indices of dissimilarity (Taeuber and Taeuber 1965, p. 236) were computed from the 1961 census tape to compare the industry distribution of each immigrant group with that of the urban population. For ethnic group  $j$ , this measure is defined by  $ID_j = .50\sum |P_{ij} - P_i|$ , where  $P_{ij}$  = the proportion of group  $j$ 's urban population in industry  $i$ ,  $P_i$  = the proportion of the total urban population in industry  $i$ , and the subscript  $i$  ranges over the two-digit census industry categories. The index  $ID_j$  varies from 0 to 1, and has an interpretation as the proportion of persons in ethnic group  $j$  who would have to change their industry affiliations in order for the two distributions to be in agreement.

Index values for the individual ethnic groups are reported in column 1 of table 5. These figures document a rather consistent tendency for Asian-African ethnics to show greater discrepancies from the urban population in their industry distributions than is the norm for European groups. When continent of origin, as a summary measure, is considered, Asian-African immigrants exhibit almost twice the disparity of Europeans<sup>14</sup> (.13 versus

<sup>13</sup> Calculations are from the 1961 census tape, and refer to three-digit industry codes.

<sup>14</sup> A problem exists in comparing the dissimilarities of subpopulations with a parent population. In the present application, the urban population consists of Asian-Africans (28%), Europeans (35%), and Israeli born plus continent unknown (37%). Since Europeans constitute a larger percentage of the urban population than Asian-Africans, they should have a smaller index value. However, because the deleted group is the largest numerically, and because the two foreign-born groups are similar in size, the index values are not simply artifacts of the composition of the urban population. With respect to the individual ethnic groups, this consideration is of negligible importance, since each comprises a very small percentage of the urban population.

TABLE 5

DEGREE OF DISPARITY BETWEEN INDUSTRY DISTRIBUTIONS OF THE ETHNIC GROUPS  
IN URBAN AREAS AND TOTAL URBAN POPULATION, 1961

ETHNIC GROUP	ALL URBAN SETTLEMENTS		DEVELOPMENT TOWNS		
	$ID^*$ (1)	Percentage Explained by Community† (2)	$ID^*$ (3)	Percentage Explained by Community† (4)	Ethnic Group Proportion in Development Towns (5)
Turkey .....	.17	.06	.26	.38	.23
Iran .....	.28	.12	.50	.45	.24
Iraq .....	.13	.15	.26	.56	.19
Yemen-Aden .....	.28	.18	.39	.59	.20
Morocco .....	.23	.26	.31	.58	.46
Algeria-Tunisia .....	.25	.29	.32	.46	.63
Egypt .....	.21	.04	.30	.12	.21
Lybia .....	.28	.21	.40	.37	.26
Russia .....	.11	.07	.25	-.07	.05
Poland .....	.10	.07	.23	.31	.06
Germany-Austria .....	.21	.10	.40	.07	.04
Czechoslovakia .....	.15	.05	.33	.02	.07
Hungary .....	.14	.00	.27	-.03	.14
Romania .....	.10	.04	.22	.29	.15
Bulgaria .....	.18	.13	.31	.18	.12
Asian-African .....	.13	.25	.26	.70	.27
European .....	.07	.12	.18	.29	.08

SOURCE.—Computations are from the 1961 census tape, for males aged 14 and older, and pertain to communities with more than 5,000 inhabitants in 1961.

\* For ethnic group  $j$ ,  $ID_j = .50 \sum |P_{ij} - P_{i.}|$ , where  $P_{ij}$  = proportion of group  $j$ 's urban population (development town population) in industry  $i$ , and  $P_{i.}$  = proportion of the total urban population in industry  $i$ .

† Formula is  $(ID_j - \hat{ID}_j)/ID_j$ . See text for details of this computation. Negative values should be interpreted as indicating zero importance of community.

.07). We will indicate, momentarily, that this difference is due to the greater representation of the former ethnics in development towns.

To ascertain the extent to which community industrial structure is responsible for the index values, we estimated an expected industry distribution  $\{\hat{P}_{ij}\}$  for each ethnic group  $j$ , and calculated the degree to which the *observed* distribution for the group is accounted for by this set of estimates. The expected distribution was computed as a weighted average of the industry distribution in every community, the weights being the proportions of ethnic group  $j$ 's urban population in the various settlements. These figures therefore report the representation which a group would have in different industries if its members were employed by them, in every community, at the same rates as the settlement's total labor force. The dissimilarity between the observed and expected industry distributions for group  $j$  is specified by  $\hat{ID}_j = .50 \sum |P_{ij} - \hat{P}_{ij}|$ , which measures the residual discrepancy, the amount *not* explained by community. The percentage re-

duction in the index which can be attributed to community industrial structure is  $(ID_j - \hat{ID}_j)/ID_j$ .

The index proportions which are explained by community, in the sense of the preceding discussion, are presented in column 2 of table 5. There is an evident tendency for residence location to be more important for Asian-African groups than for the European ethnics. When continent of origin alone is considered, 25% of the discrepancy between the industry distributions of Asian-Africans and the total urban population can be attributed to the concentration of those immigrants in certain communities; for Europeans, the corresponding figure is 12%. The percentages for the individual ethnic groups should also be compared with the indices of variation in settlement concentration (table 2, top row of lower panel). Despite the lack of full comparability between the ethnic categories in our two data sources, there is evidence that the effect of community is greater for groups having a large variation in settlement concentration. For instance, immigrants from Yemen-Aden and Morocco-Algeria-Tunisia have the highest index values of settlement concentration, and the largest percentages of their industry distributions accounted for by community. This correspondence is hardly surprising; it simply illustrates the mechanism we have been describing whereby community may create ethnic disparities in labor force characteristics.

What is the impact of *development towns* on the industry affiliations of the ethnic groups? To pursue this matter, the preceding calculations were repeated for the 19 development towns identified on the 1961 census tape. The index  $ID_j$  now reports the difference between the industry distribution of group  $j$  in the development towns, and the industry distribution of the total urban population. The results, which are presented in column 3 of table 5, reveal slightly larger index values for Asian-African ethnics than for Europeans; at the level of continent of origin, the respective figures are .26 and .18. A more pronounced effect concerns the disparity between the index value of an ethnic group in the development towns, and its value in all urban settlements (cols. 1 and 3). The figures for development towns are consistently larger, suggesting that these settlements expose their residents to industrial structures that differ considerably from the ones existing in other communities.<sup>15</sup> This observation, together with the greater representation of Asian-African populations in development towns (col. 5), explains the manner by which the towns contribute to ethnic differences in industry affiliation (col.1).

<sup>15</sup> Another way to convey this point is by noting the ID value of each settlement type. For all residents in a settlement category, as compared with the total urban population, the values are .20, .13, .09, and .09, for development towns, veteran communities, suburbs, and main cities, respectively. Thus, the divergence from the urban population in industry structure is greatest for development towns.

The degree to which the index values in column 3 can be attributed to the industrial composition of development towns is strikingly different for Asian-African and European ethnics. The proportions explained by community in the case of the former greatly exceed the proportions for the latter groups (col 4).<sup>16</sup> At the level of continent of origin, the respective figures are .70 and .29. It is also noteworthy that differences among the ethnic groups in the effect of community correspond to differences among them in degree of concentration in individual development towns (table 2, lower panel, last row). For instance, with regard to Asian-Africans, Yemen-Aden immigrants are highest on both concentration by settlement and importance of community for explaining their industry distribution, while immigrants from Egypt-Lybia are lowest on both factors. The European groups exhibit little difference in degree of settlement concentration, although Romanians do have somewhat higher values on the two indices.

We conclude that the overrepresentation of Asian-African ethnics in development towns (which contain industries different from those common elsewhere in the country [n. 15]), and their further concentration in certain of these settlements, has served to expose them, to a disproportionate extent, to particular industrial opportunities. This situation is responsible for the larger disparities between their industry distributions and that of the total urban population, in comparison with European groups (table 5, col. 1), and for the greater importance of community in explaining their industry affiliations (col. 2).

#### Impact of Residence Location on the Occupational Standings of the Ethnic Groups

The industrial configuration of a settlement is salient to labor force opportunity for a variety of reasons. Industry determines work satisfaction (Blauner 1964), seasonality of employment, and rate of promotion, as well as occupational composition. For the narrow purpose of understanding settlement differences in the latter factor, however, it is the case that the various industrial structures translate into much the same sort of occupational distribution for communities within each settlement category. In particular, despite the tendency for individual development towns to "specialize" in a given industrial activity—textiles, food processing, and mineral extraction being the most common—the different towns tend to have similar occupational distributions.

The reasons for this are not difficult to comprehend. We have already described the inducements proffered by the central government to firms

<sup>16</sup> The negative values in this column indicate greater disparity from the industry distribution of the total urban population when the community distribution is considered. The most reasonable interpretation for these figures is as zeros.

in certain industries, to motivate them to locate in development towns. The preference has tended to be for industries that are labor intensive and utilize low-skill work forces. Labor-intensive technologies create many jobs for a fixed initial capital investment, a matter of importance to a country with limited resources in the process of accommodating a large refugee population. Low-skill occupations permit immigrants from diverse cultural backgrounds to be assimilated into the labor force with a minimum of job retraining and language acquisition. With respect to the main urban centers, there is evidence from other studies (e.g., Galle 1963, p. 263) that major metropolitan places typically have many commercial and administrative functions, which entail sizable white-collar work forces.

In table 6, columns 1-4 report the average occupational distribution in each settlement type.<sup>17</sup> The results for development towns and the main cities confirm our a priori notions. Development towns have few white-collar workers (22% of the labor force) while the main cities employ a great many (49%), in comparison with the urban population (41%). It

TABLE 6  
OCCUPATIONAL COMPOSITION\* BY COMMUNITY CATEGORY, FOR  
URBAN SETTLEMENTS, 1961

COMMUNITY CATEGORY	OCCUPATIONAL DISTRIBUTION (%)†					OCCUPATIONAL STATUS SCORE‡	
	Upper White Collar	Lower White Collar	Upper Manual	Lower Manual	Total	Observed	Predicted
	(1)	(2)	(3)	(4)	Total	(5)	(6)
Development towns . . . .	.09	.13	.39	.39	1.00	97.5	101.3
Veteran settlements . . . .	.14	.18	.40	.28	1.00	104.0	106.4
Suburbs . . . . .	.15	.23	.42	.20	1.00	111.7	110.0
Main cities . . . . .	.23	.26	.33	.18	1.00	114.8	113.0
All urban settlements . . . . .	.18	.23	.37	.22	1.00	109.7	109.7

SOURCE.—Computations are from the 1961 census tape, for males aged 14 and older, and pertain to communities with more than 5,000 inhabitants in 1961.

\* Occupational categories are defined as follows: upper white collar—scientific and academic workers, other professional workers, administrators and managers; lower white collar—clerical and sales workers; upper manual—skilled workers in industry, mining, building, and skilled workers in agriculture; lower manual—unskilled workers in industry, transport, building, other unskilled workers, service workers. The components refer to the 1972 occupational classification, to which the 1961 census codes were transformed (Israel Central Bureau of Statistics 1974, appendix B).

† Entries are unweighted averages of settlement values, with the exception of the last row, which reports the occupational distribution of the urban population.

‡ Each of nine major occupational categories was indexed by its median earnings in 1972 (Israel, Central Bureau of Statistics 1973a, p. 344). "Observed" status scores are a weighted average of the earnings values, the weights being the proportions of the labor force from a settlement type in the occupations. "Predicted" status scores were calculated in the same manner except that the occupational distribution of a settlement was first estimated from its industry distribution, the estimation being made from an industry-by-occupation matrix for the entire urban population. Status scores are in units of Israeli currency (IL × 100) and reflect annual occupational earnings in 1972.

<sup>17</sup> Settlement occupational distributions were estimated from the occupational affiliations of residents in the respective communities. Our estimates, therefore, are least accurate for suburbs.

is also the case that development towns have few skilled occupations, when this occupational category is viewed relative to the size of the lower manual work force. That is, while there are equal numbers of upper and lower manual positions, in other settlements the ratio of skilled to unskilled occupations is much higher, enhancing the mobility prospects there for lower-class workers. Some implications of the restricted occupational opportunity in development towns are outlined in the next section.

We wish to make clear the structural underpinnings of the occupational differences among settlement types. The differences derive principally from the kinds of industries located in the communities, and relate only indirectly to the skill levels of the inhabitants. The final two columns of table 6 provide evidence for this contention. In column 5, "observed" occupational status scores are presented for each settlement type. These scores were calculated by classifying the occupations into nine major categories for which national earnings data are available,<sup>18</sup> and computing a weighted average of the earnings figures, the weights being the labor force proportions in the categories. The resulting scores index occupational standing; the value for a community reflects only its occupational composition, not the quality of work by practitioners of an occupation or community differences in rate of pay for the same work. The entries indicate that occupational status varies considerably by settlement type, from a low of 97.5 for development towns to a high of 114.8 for the main cities.<sup>19</sup> These figures, then, express the preceding distributional information in more summary form. In column 6 estimates of the occupational status scores are reported, the calculations being based on the occupational distributions characteristic of the various industries,<sup>20</sup> and the industry composition of each settlement. These scores therefore report the status discrepancy that *should* exist among community types, given their industrial compositions. The estimated scores parallel the observed values, and reveal that 11.7 points from the 17.3-point range in occupational status is expected from industry differences among the settlements.

The fact that the occupational distributions of the communities can be

<sup>18</sup> Earnings data are for 1972 (Israel Central Bureau of Statistics 1973a, p. 344). Information on income was not collected in the 1961 census. While earnings figures are available for years previous to 1972, they pertain to the one-digit categories of an old occupational classification, which is largely an industry classification, and show less dispersion than do the figures for the present occupational categories. The two-digit 1961 occupations were transformed to the 1972 codes using the occupational translation key in Israel Central Bureau of Statistics (1974, appendix B).

<sup>19</sup> These calculations, and others in this section, were repeated with occupational status being indexed by average education in an occupation. The results were very similar to the ones reported in the paper.

<sup>20</sup> An occupational distribution was constructed for each industry using data from the entire urban population.



understood from a consideration of the kinds of industries they contain does not mean that the personal characteristics of residents are irrelevant to the determination of occupational composition. Indeed, we have argued that the industrial structures of development towns were deliberately planned with the needs of their prospective populations in mind. What a consideration of columns 5 and 6 reveals is that while the skill levels of the inhabitants may have been influential *as initial causes* in attracting certain kinds of enterprises, once an industrial base has been established the occupational structure of a settlement is an immediate resultant of its mix of industries. Also, since few industries have technologies which can accommodate much variety in occupational composition, a settlement cannot respond easily to changes in the skill and education levels of its residents.

The impact of community on the magnitude of ethnic differences in occupational status is reported in table 7 for the continent-of-origin groups.<sup>21</sup> Columns 1 and 4 present occupational status scores for each ethnic group, by settlement category. These "observed" values were calculated in the same fashion as the entries in column 5 of table 6, the sole difference being that the weights used in computing the occupational averages now pertain to proportions of the relevant ethnic group. The figures reveal sizable disparities for each group, over the community categories, with development towns showing the lowest status scores, and suburbs and

TABLE 7  
OCCUPATIONAL STATUS\* OF THE CONTINENT-OF-ORIGIN GROUPS,  
BY COMMUNITY CATEGORY, 1961

COMMUNITY CATEGORY	ASIAN-AFRICAN			EUROPEAN		
	Observed Status (1)	Expected Status (2)	Proportion of Group's Urban Population (3)	Observed Status (4)	Expected Status (5)	Population of Group's Urban Population (6)
Development towns ....	91.0	96.6	.27	106.4	98.1	.08
Veteran settlements ....	91.1	103.4	.21	110.5	104.2	.19
Suburbs .....	102.7	110.6	.18	115.3	112.1	.22
Main cities .....	99.8	114.9	.34	119.7	114.7	.51
All urban settlements .....	96.2	106.6	1.00	115.9	110.8	1.00

SOURCE.—Computations are from the 1961 census tape, for males aged 14 and older, and pertain to communities with more than 5,000 inhabitants in 1961.

\* Status scores were computed in the manner described in n. ‡ of table 6, except that the weights now refer to an ethnic group's proportions in the occupational categories. Scores are in units of Israeli currency (IL × 100), and reflect annual occupational earnings in 1972.

<sup>21</sup> The findings for the individual ethnic groups show only small differences from their continent means, and therefore are not presented separately.

main cities the highest. Within each settlement type there also are substantial status differences between the immigrant groups: Europeans consistently have higher occupational standing than Asian-Africans. When all urban settlements are considered, a 19.7-point differential is obtained between the origin groups. What we wish to ascertain is the extent to which this gap is a consequence of the settlement patterns of the two ethnic groups.

Expected status scores were computed for each origin group by assuming that, in every community, its members had the same occupational distribution as the settlement's total population. This computation therefore indexes the degree to which an immigrant population is advantaged occupationally by its community locations. The expected scores are reported in columns 2 and 5, and reveal comparable occupational exposure patterns for the two ethnic groups *within each settlement category*. However, because Asian-Africans are overrepresented in development towns, while Europeans are concentrated in the main cities and suburbs (cols. 3 and 6), at the level of the total urban population a significant gap emerges in expected occupational status; the score for Asian-Africans is 106.6, and for Europeans it is 110.8. This 4.2-point gap, which is attributable to different settlement locations by the two ethnics, accounts for 21% of the observed discrepancy in occupational status between the groups.

In actuality, this 4.2-point gap is an upper bound to the impact of community. Our computations have ignored resident characteristics, and thereby presume that any settlement differences in years of schooling or other job-related skills are, properly, the effects of community; for instance, lower educational attainment in development towns might be due to poor school facilities in these settlements. However, there is evidence (next section) that capable individuals tend to migrate from development towns. This means that individual attributes are, in part, a determinant of settlement location, rather than the reverse. If we adopt this formulation, the effect of community should be examined *net* of individual characteristics. While we cannot hold these factors constant using the preceding methodology, by means of a regression procedure an individual's occupational status was examined against terms for (a) ethnic origin, (b) education, age, and length of residence in Israel, and (c) 46 dummy terms for the settlements. The result was that the initial 19.7-point gap<sup>22</sup> between Asian-African and European immigrants was reduced to 6.9 points by addition of the individual characteristics, and to 4.7 points by the introduction of the settlement terms. This last 2.2-point reduction, representing

<sup>22</sup> In the regression formulation, the ethnic gap in occupational status appears as the difference between the *b* coefficients for the two continent-of-origin groups (entered as dummy variables). The Israeli-born population is the deleted category.

11% of the initial status difference, is a lower bound to the effect of community.<sup>23</sup>

Settlement location by no means accounts for a major portion of the ethnic gap in occupational standing. Sizable disparities exist between the groups in education and occupationally relevant skills (Ben Porath 1973), and they are responsible for the larger part of the occupational differential. Nonetheless, the effect of settlement is not negligible. At the level of the individual ethnics, its importance is even greater in certain instances. For example, in terms of the more conservative calculation, a 3.8-point difference in occupational status is expected between immigrants from Russia and Algeria-Tunisia, in favor of the former, on the basis of the occupational opportunities available in the communities where each group resides. That this settlement effect is due largely to the different representations of these two ethnics in development towns may be seen from column 5 of table 5.

#### CONSEQUENCES OF INDUSTRIAL STRUCTURE FOR THE DEVELOPMENT TOWNS

We have characterized development towns as locales in which few moderate-status positions are available, this limitation deriving from a concentration of low-skill industries in the settlements. Relating the occupational composition of the towns to their demographic features, we have suggested that the restricted occupational opportunity in these communities is responsible, in part, for the disparities which exist in Israel in occupational standing between immigrants from European and those from non-European countries; that is, the latter ethnics, being overrepresented in the towns, are exposed disproportionately to disadvantageous industrial contexts. Yet the impact of development towns on national-level indices of the ethnic gap is muted by the fact that even among Asian-Africans, only 27% of the population resides in this class of settlements. When we turn to a consideration of the social character of the towns themselves, however, the full impact of the initial decisions concerning the structure of their economies becomes evident.

There is merit to the contention that the government's policy to establish new towns in outlying areas, direct large numbers of immigrants to the settlements, and introduce in them principally labor-intensive, low-skill industries, constituted an efficient strategy by which a small country with

<sup>23</sup> The persistence of a community effect, which we note here, means that, despite a high migration rate from development towns to communities with better work opportunities, many individuals, when confronted with a choice between career and ties to family and neighbors, do not choose to maximize occupational prospects.

modest resources could cope with several pressing and related problems. First, immigrant absorption was a matter of urgency, not something that could be deferred or solved gradually, and the introduction of low-skill technologies facilitated their rapid incorporation into the labor force. A second objective furthered by the new towns was promotion of population redistribution and the opening of the hinterland to development. This was easier to achieve through encouraging new immigrants, who had few ties with communities in Israel, to settle in the towns, than through stimulating the migration of veteran Israelis from the country's metropolitan centers. Nevertheless, substantial disutilities can be associated with the new-town policy once a longer-range time perspective is adopted. Pertaining to this matter, we wish to discuss several immediate consequences of the labor force composition of development towns, as well as a number of derivative, second-order problems.

The direct consequences of limited opportunity are that immigrants who come to the towns with training which qualifies them for skilled manual or lower white-collar positions have difficulty in locating suitable work, and residents who begin their careers in low-ranked jobs have limited prospects for upward mobility. An indication of the seriousness of the blockage in occupational mobility, even for poorly educated persons, can be seen by noting that in the two medium-status categories underrepresented in the towns (skilled manual and lower white collar),<sup>24</sup> 58% of the country's labor force in the former category, and 29% of the labor force in the latter, have eight or fewer years of education (Israel Central Bureau of Statistics 1973a, pp. 336-37). This means that it is not uncommon for capable individuals with little education to enter these occupations. To summarize, while the industries established in development towns may be appropriate to the skill level of the *average* immigrant, they severely constrain the occupational prospects of the better-educated or more motivated settlers.

Partly as a consequence of limited occupational opportunity, there has been considerable migration from development towns. A report on 21 towns (Israel Manpower Planning Authority 1964, p. 6) notes that 40% of their population in 1961 moved out of this settlement category in the succeeding two years; this figure was four times the national rate of inter-urban movement. There is also evidence that the migrants come from the more talented segment of the community:<sup>25</sup> they are better educated than

<sup>24</sup> "Skilled manual" refers to the census occupational category—skilled workers in industry, mining, building, and transport; and to part of the census category—agricultural workers. "Lower white collar" refers to two occupational categories—clerical and related workers, and sales workers (Israel Central Bureau of Statistics 1974).

<sup>25</sup> Data are from tabulations prepared by the authors from the 1961 census tape. Occupational status is indexed by 1972 earnings figures for the one-digit census occupation categories. Facility with Hebrew is based on a census tape code which refers simultaneously to reading and writing competence.

nonmigrants (7.6 years of schooling vs. 6.2), have higher current occupational status (103 vs. 96), are more facile with Hebrew (74% can read and write vs. 65%), and have been in Israel for a longer period (11.4 years vs. 11.0).<sup>26</sup> There is little migration to development towns from veteran settlements, which might offset this loss of capable residents.<sup>27</sup> Rather, these towns have functioned as ports of entry for new immigrants to the country, providing them with temporary places of abode until they acquire the means to establish themselves in other communities. Those who remain tend to have modest occupational aspirations or require the housing subsidies and other kinds of assistance that are more readily available to residents of development towns. To put matters succinctly, the towns serve as "sinks" for less resourceful immigrants.

An additional process appears to operate in development towns within commuting distance of veteran settlements. Residents who are better qualified occupationally frequently obtain employment outside the towns and travel to work, much as if they lived in suburbs. From the 1961 census tape we calculated that, for the industrial labor force<sup>28</sup> residing in development towns, mean education of males employed outside their settlements is 7.4 years, compared with 6.9 years for those residing and working in the towns. It is unclear from our data whether the existence of suitable employment in nearby communities permits development towns to retain these individuals, or whether it facilitates their eventual departure by enabling the withdrawal to be accomplished in stages. To some extent the latter process probably operates, since many of the settlements are not very attractive, and their inhabitants often have only a weak identification with community (Lichfield 1971, 1:6.3, 6.10). In this regard we point out that, alongside the departure of skilled manual and lower white-collar workers, development towns tend to have nonresident professional and administrative work forces (Smith 1972, p. 34). Teachers, social workers, and industrial managers choose to commute to work rather than reside in the towns. Our calculations from the 1961 census tape provide support-

<sup>26</sup> We point out that the higher scores of migrants are not just a consequence of geographic mobility being more common among better-situated persons. Individuals who moved to another development town tend to have lower scores than migrants out of this settlement category. Averages for the former class of movers are: 7.1 years of schooling, 100.0 in current occupational status, 73% competent in Hebrew, and 10.9 years in Israel.

<sup>27</sup> Amiran and Shachar (1969, p. 21) cite a net out-migration figure of 10,000, during the interval 1956-61, from the census category "new urban settlements" (which includes approximately one-half the population in development towns). Lichfield (1971, 1:6.2) reports a net out-migration of 43,800, between the years 1961 and 1967, from his list of development towns.

<sup>28</sup> Several development towns, in outlying areas, also supply agricultural workers. Since we are discussing the impact of the labor market in neighboring settlements, agricultural employment is deleted from this comparison.

ing evidence for this residence style among white-collar workers: educational attainment by nonresidents employed in development towns is 10.0 years of schooling, versus 6.6 years for inhabitants of the towns.

These processes are responsible for the indirect effects of industrial structure that we wish to document. The inability of the settlements to retain their more resourceful and better-aculturated inhabitants, or attract comparable persons from other Israeli communities in substantial numbers, has meant that the settlers who remain tend to have high rates of dependency and related social problems. In 1963, 11.6% of families in development towns received continuous economic help, in comparison with 4.4% in the total population. For all forms of assistance, the respective figures were 34.5% and 17.4% (Israel Manpower Planning Authority 1964, p. 26). With regard to participation in education by youth, in the 14-17-year age group, 41% in development towns study full-time in some educational institution, compared with 68% in the entire country (Israel Manpower Planning Authority 1964, p. 11). Delinquency statistics are consistent with this picture. Using data from the Israel Ministry of Welfare's (1972) handbook of community characteristics, we calculated an incidence rate of 26.6 per 1,000 in the age group 9-16 for the 19 development towns used in the previous computations. This figure compares with 14.3 for the country, and 16.4 for the three main cities.

Even if the rates of incurring social problems in development towns by the ethnic and class groupings residing there were not greater than the rates these same populations exhibit in other communities, the mere fact of concentration of less resourceful families creates undesirable consequences. The low representation of moderate-status individuals in the towns and their high population turnover mean that local leadership tends to be weak<sup>29</sup> (Lichfield 1971, 1:63), that the schools are not centers of excellence, and that the clientele capable of supporting cultural activities is small. Despite grants-in-aid from the central government, these settlements are hard pressed to collect adequate tax monies to provide for the manifold needs of their lower-class populations (Lichfield 1971, 1:5.16), not to mention the amenities which can make a community an attractive place of residence. Finally, because development towns are contexts in which dependency is common, there is a serious risk that it will become an approved life-style.<sup>30</sup>

<sup>29</sup> At the same time, according to Aronoff (1973, pp. 42-44), the concentration of Asian-African populations in development towns has facilitated the emergence of political leadership in these ethnic groups. Since their populations dominate the electorates, local ethnic leaders have a more secure political base in the towns than in veteran settlements.

<sup>30</sup> In a recent study of factors which influence requests for a certain welfare allowance, Bar Yosef, Schild, and Varshar (1974) report that, after controls for personal need,

To clarify the preceding comments, and place them in perspective in relation to long-range prospects for the towns, we must stress two points. First, we have discussed development towns as if they formed a homogeneous settlement category with regard to occupational composition and the presence of derivative problems. This is a reasonable first approximation to the reality, and an efficient analytic strategy considering that our interest is to elucidate the relation between community and ethnic stratification, and document the particular role played by industrial structure. However, it means ignoring the many differentiating mechanisms which have resulted in a few development towns constituting cases of successful progress, in that they attract settlers from diverse population groups via internal migration, retain their more talented residents, and have either acquired a diversified industrial base or were founded originally around industries which utilize a greater array of occupational skills than is the norm in the majority of the towns.

Beersheva, Ashdod, and Arad are examples of successful development towns. Beersheva is now a regional center for southern Israel; it has a university, provides medical and commercial services for the surrounding region, and is able to support a varied white-collar labor force. Ashdod is a major seaport. Workers in shipping are well paid; also, the town has attracted ancillary manufacturing, transportation, and commercial firms that find it advantageous to be located in a maritime center. Arad is an isolated community near the Dead Sea which has been built around mineral extraction, chemical processing, and metal fabrication industries. These activities require engineers, technicians, and skilled craftsmen. Because of the settlement's isolation, these personnel, together with teachers, social workers, and others who provide professional services in the town, must live there, and consequently have a vested interest in its progress. Arad, in addition, is advantaged by social composition; a majority of its residents are Israeli born, and lack the many adjustment problems which confront new immigrants. Differentiating processes of considerable import therefore operate, and some settlements are following satisfactory development trajectories. However, a detailed consideration of these matters is outside the scope of this report.

A second qualification concerns the fact that we are examining the towns at a very early point in their histories. The bulk of our data are from the 1961 Census of Population; thus no development town is older than 13 years, at least in its modern phase. Most are very small, having less than 10,000 inhabitants, and their unbalanced industrial character stems partly from this size factor. However, because governmental policy is to encourage further growth, and because the immigrants now arriving in

knowledge about the allowance, and various individual characteristics, residence in a development town has a strong, positive effect on application.

Israel come predominately from European countries and are better educated than the earlier refugees, conditions exist for altering the industrial composition of the settlements. The amount of high-technology industry that would have to be introduced to provide reasonably balanced occupational structures is not at present very great, and could be organized around the skills of the new immigrants in combination with a modest number of Israelis who might be attracted from veteran settlements.

#### RELEVANCE OF THIS ANALYSIS FOR ETHNIC STRATIFICATION IN THE UNITED STATES

The processes we have described are very visible in Israel because of the huge immigration inflow this country has experienced in a brief time interval—far exceeding the rate of growth of the American population from immigration in any similar period—and because the government's espoused intention has been to settle the immigrants in new towns. Nonetheless, the importance of community for explaining ethnic stratification in the United States is also considerable, although the mechanisms that have brought immigrant groups to certain sections of the country, and to particular communities, are not identical with the ones which have operated in Israel.

The sorts of mechanisms that have generated ethnic concentration by region and community in America concern time of arrival, route of travel, and degree of affinity of a group for its own kind. The first factor is associated with the processes we have described in connection with ethnic concentration in Israel. Immigrants from various lands came to America in different decades; and time of arrival correlates with location of the western frontier and, consequently, with period of settlement of a geographic region. Route of travel is a consideration in explaining the concentration of certain groups—French-Canadians, Mexicans, and Chinese—who came via routes that were not followed by the majority of immigrants. These ethnics tend to be overrepresented in the states that border their ports of entry.

The preference of individuals from the same country of origin to live in proximity to their own kind is also responsible for generating ethnic concentration, although this factor does not explain where in America a group will choose to reside. Breton (1964) suggested that immigrants who are different from the receiving population on a number of dimensions—language, religion, cultural traditions—are likely to settle together in a community in order to constitute a clientele of sufficient size to support ethnic churches, schools, restaurants, newspapers, and *Landsmanshaften*. Once the initial migrants have established themselves in particular cities,



chosen for whatever reason, subsequent immigrants from the country of origin tend to travel to the same settlements.

As a consequence of these processes, the correspondence of ethnicity with geographic region and community is quite strong. Scandinavians and Germans are concentrated in the Midwest; French-Canadians are located in northern New England; Italians are in southern New England and in the Mid-Atlantic states; and Jews, a heavily urban group, reside principally in New York City and Chicago. To cite instances of extreme ethnic concentration in states, 1920 census data on the foreign born reveal that Norwegians were represented in North Dakota at 15 times their national rate; French-Canadians were concentrated in New Hampshire at 27 times their rate in the country; and Mexicans were overrepresented in Arizona by a factor of 41 (Hutchinson 1956, pp. 34-48). It is also the case that first-generation American born continue to reside in the geographic regions of their parents; 1950 census data on natives of foreign or mixed parentage show representation rates of 19, 27, and 11 for the preceding three groups, in their respective states (Hutchinson 1956, pp. 39-43).

The persistence of ethnic concentration means that regional and community differences in industry location will have a stable impact on ethnic opportunity. There are striking differences among the immigrant groups in industry affiliation. This is principally a consequence of the industrial composition in the settlements where a group is concentrated, although the ethnic populations are themselves responsible for introducing several industries into America—ready-made apparel manufacturing, for instance, is associated with Jewish immigrants (Hapgood 1966, p. 10). To illustrate the tendency toward ethnic concentration by industry, according to 1950 census data on the foreign born, French-Canadians are employed in textile mills (a New England industry) at seven times their representation in the population. Mexicans work in farming at 11 times their expected rate, and in food processing at three times the expected rate (both industries have extensive operations in the Southwest and far west). Czechoslovakian and Yugoslavian immigrants are employed in primary metal processing at four times their representations in the country; these groups have large populations in Pennsylvania, Ohio, and Illinois, which are centers of ferrous metalworks (Hutchinson 1956, pp. 224-31).<sup>31</sup>

Industry affiliation is an important consideration in the analysis of occupational standing and mobility for several reasons. Industries differ in technology and, as a consequence, in mix of occupations. Textile manufacturing and food processing, for instance, contain few skilled manual positions, while the majority of the blue-collar work force in printing and

<sup>31</sup> Industry concentration figures are based on data for operatives, except in the case of Mexicans in farming, for whom farm laborer figures were used.

in aircraft manufacturing is classified as skilled (U.S. Bureau of the Census 1963, pp. 506-12). The occupational distribution in an industry is one determinant of the mobility prospects facing an individual, and constrains his advancement to a greater or lesser extent, depending on the shape of the distribution, as long as he remains employed in that sector of the economy.<sup>32</sup> Where communities are centers of a particular industrial activity, the occupational composition of the dominant industry has an even more pervasive effect on labor market opportunity; indeed, this is the situation that exists in many development towns in Israel.

Industries differ in other respects which can influence an individual's occupational mobility prospects. In some, the firms characteristically fill their upper-manual positions by promotion from below; in other industries a craft model is the norm, which permits little possibility for upward movement. Demographic features of an industry are also relevant to understanding mobility opportunity: some industries are expanding, and create new upper-manual and white-collar positions which might be filled through promotion; others are stable or declining in employment, and present limited promotion prospects. Finally, industries differ in the age composition of their employees, a consideration relevant to mobility, as it specifies the amount of promotion and replacement hiring that will take place in the short-term future.

It is unfortunate that industry and community characteristics have been neglected in the main thrust of research on status attainment, which has focused exclusively on individual-level variables. This omission is especially serious in the study of ethnic stratification, since, as we have documented, ethnic groups tend to be concentrated by region and settlement, and are therefore exposed to different industrial structures. Analyses of occupational standing have commonly attributed the ethnic effects which remain after controls for individual characteristics (father's SES, respondent's education and status of first job, etc.) to cultural background or motivational differences among the groups (Duncan and Featherman 1972). While we have no reason to doubt the salience of such factors for understanding ethnic stratification, we do suggest that they are confounded in the residual ethnic terms with substantial industry and community effects.

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<sup>32</sup> Change of industry, indeed change of firm, is hardly a decision that can be made easily, except by the young, in response to the availability of better occupational prospects with a different employer. Workers accumulate pension rights, seniority toward job security, and other vested interests which are not transferable.

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# Simmel's Influence on American Sociology. I<sup>1</sup>

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Three phases in the diffusion of Simmel's thought within the American sociological community are identified. His influence is then traced, first, in the area of general theoretical orientations, and second, with respect to research traditions on the stranger and social distance. Part II, to be published in the next issue of this *Journal*, will discuss Simmel's influence on other substantive areas.

Georg Simmel stands in the unusual position of being the only European scholar who has had a palpable influence on sociology in the United States throughout the course of the 20th century. This is particularly noteworthy in view of the fact that, contrary to current impressions about the history of the discipline, when sociology was becoming established within the American academic system during the first few decades of this century, it was truly a homegrown product.

Social research in this country was initially stimulated by such indigenous currents in American intellectual life as pragmatism and the social gospel movement, and by conspicuous domestic problems such as crime, ethnic relations, immigration, industrial conflict, suffragism, and rapid changes in rural and urban communities. Its leading proponents were predominantly self-made American social analysts—Charles Horton Cooley, Franklin H. Giddings, Edward A. Ross, William Graham Sumner, and Lester F. Ward. These five were the men named most frequently as important sources of intellectual stimulation in the autobiographical statements of 258 American sociologists collected by Luther L. Bernard in 1927. Only 20% of those respondents mentioned any European author as having exerted a significant influence on their intellectual outlook.<sup>2</sup>

Among the European authors who were mentioned as influential by professional American sociologists in the late 1920s, the three most often cited were, in order of frequency, Herbert Spencer, Georg Simmel, and Gabriel Tarde. A similar picture emerges from a look at the general treatises on sociology and social psychology of that period: the authors

<sup>1</sup> An earlier version of this paper appeared as an appendix to *Ästhetik und Soziologie um 1900: Georg Simmel*, edited by Hannes Böhringer and Karlfried Gründer (Frankfurt: Klostermann, 1975). The authors gratefully acknowledge suggestions at various stages in the preparation of this paper from Paul J. Baker, Robert Bogart, Lewis A. Coser, Peter Langer, Robert K. Merton, and Kurt H. Wolff.

<sup>2</sup> Appendix A presents some of the data from this survey.

most frequently cited are the pioneer American sociologists listed above and their epigoni, while the scarcer citations of Europeans are invariably to Spencer and Simmel, frequently to Tarde, Comte, Hobhouse, and Durkheim, and rarely if ever to Marx, Tönnies, Pareto, or Weber.<sup>3</sup>

Not long after Bernard's survey, however, interest in Spencer, Tarde, Comte, and Hobhouse dropped precipitously and irreversibly; in their place as sources of stimulation from abroad came Vilfredo Pareto—himself later eclipsed by an upsurge of attention to the works of Karl Marx—and, slowly but more durably, Émile Durkheim and Max Weber. Interest in Simmel, by contrast, remained alive though subdued during the 1930s and 1940s and thereafter revived to become more extensive than ever.

Simmel's influence on American sociology is unusual in another respect. Although literate American sociologists today could be expected to produce a coherent statement of the theoretical frameworks and principal themes of Marx, Durkheim, and Weber, few would be able to do the same for Simmel. In good measure this condition reflects the character of Simmel's intellectual productions themselves: the bewildering variety of topics he treated and the disorganized manner in which he presented his general principles. After all, Simmel himself advised the reader of *Soziologie* (1908) that the best way to get a complete picture of what his volume contained would be to consult the list of topics presented (in alphabetical order) in the index! However, the fragmentary picture of Simmel's sociology held by American sociologists must also be connected with the disjointed manner in which his ideas entered the mainstream of American sociology over the past seven decades, not to mention the transmutations they underwent in the process. The two parts of this paper attempt to outline the main events in the long and erratic career of Simmel's thought in the history of sociology in the United States.

#### AGENTS, MEDIA, AND CENTERS OF DIFFUSION

Before discussing the fructifying impact of Simmel's ideas on social research in the United States, we analyze the actual processes by which they were transmitted. Their transmission cannot be viewed as an automatic function of the inexorable progress of scientific knowledge. Simmel received far less attention from sociologists in France than in the United States, and the preoccupation of earlier American sociologists with indigenous authors, issues, and outlooks suggests that the insertion of a European author's ideas into the center of sociological development must not be taken for granted.

The transmission of Simmel's ideas into American sociology actually

<sup>3</sup> Appendix B presents data on these points.

took place at different times, in different places, in different ways, and for different reasons. The process is best understood as occurring in three phases: 1895–1930, an initial wave of translations associated with the diffusion of Simmel's thought from a single center; 1930–55, the dispersal of centers of instruction concerning Simmel, followed by a new wave of translations; and 1955–75, a period of more critical exegesis and of efforts to integrate Simmel more systematically into the sociological tradition.

### 1895–1930: Defining the Sociological Domain

The diffusion of Simmel's sociological writings to the United States depended initially on the fact that many of those who sought to establish the social sciences in American universities looked to the universities of Germany for inspiration and legitimation and, in particular, that a small number of graduate students (from Johns Hopkins, Harvard, and the University of Chicago) who subsequently occupied influential positions in American sociology studied at the University of Berlin when Simmel was active there.<sup>3</sup> Simmel stood out during those years as one who took the question of establishing a clearly demarcated analytic domain for sociology with especial seriousness, and his forthright delimitation of the field helped to provide a sense of professional identity for some of those who learned from him.

Albion W. Small was the earliest American sociologist to establish a link with Simmel. Small studied at the University of Berlin in 1880, when Simmel himself was a student there. At some point the two developed a close collegial relationship. Small visited Simmel during later trips to Germany and corresponded regularly with him. He thus kept abreast of Simmel's sociological productions from the outset and became the first to bring the good news of Simmel's work to the United States.

Among those who actually attended Simmel's lectures in Berlin were no fewer than six men who became sociologists as the discipline was starting to gain footing in American universities: Frederick A. Bushee, Charles A. Ellwood, Edward C. Hayes, Robert E. Park, Nicholas J. Spykman, and Howard J. Woolston. Ellwood, sometimes called the "founder of scientific psychological sociology," heard Simmel lecture in the fall of 1897. Although he considered Simmel's approach to social

<sup>4</sup> Many American scholars and intellectuals other than sociologists were drawn to Simmel's lectures at Berlin. For example, George Santayana wrote to William James in the late 1880s that he had "discovered a Privat Dozent, Dr. Simmel, whose lectures interest me very much," and later called Simmel "the brightest man in Europe." (James himself subsequently referred to Simmel as "a humanist of the most radical sort.") The distinguished educator Abraham Flexner attended Simmel's lectures just after the turn of the century and found his mind "critical, imaginative, and . . . subtle" (Corey 1955, p. 27; James 1904, p. 462; Flexner 1940, p. 140).

psychology excessively philosophical, Ellwood recalled that he had found some of Simmel's ideas very stimulating, and named him as one who had a significant impact on his intellectual career.<sup>5</sup> Robert Park, who was to become the most influential of Simmel's American students, arrived in Berlin two years later. Acquiring from Simmel the only formal instruction in sociology he ever had, Park later recalled that "it was from Simmel that I finally gained a fundamental point of view for the study of the newspaper and society" (Baker 1973, p. 256).

If the first condition favoring the transmission of Simmel's ideas to the United States was the creation of agents of diffusion through direct contact with Simmel in Germany, the second was the creation of a center of diffusion at a single prominent institution. Of the 11 sociologists in Bernard's survey who indicated that they had been strongly influenced by Simmel, six were directly and one indirectly associated with the University of Chicago.<sup>6</sup>

Albion Small established the first department of sociology in the United States at the University of Chicago in 1892, and founded the *American Journal of Sociology* there in 1895. Through the department and the *AJS* Small was well situated to promote the dissemination of Simmel's ideas. Around the turn of the century Small sent three of his students to Berlin to study with Simmel. He undertook an active program of publishing papers by Simmel in the *AJS*—15 such entries appeared between volumes 2 and 16, most translated by Small himself—and also published an appreciative article by S. P. Altmann, "Simmel's Philosophy of Money," in 1903.

Small was particularly attracted by Simmel's seriousness about the need to define a proper domain for sociology; after all, as Small wrote to one of his colleagues in 1908, "the main business of my life is to show that there is a definitely definable field for the division of social science to which we are applying the name Sociology." Although Small hesitated

<sup>5</sup> Personal communication from Paul J. Baker, based on documents of the Pennsylvania Historical Collections at Pennsylvania State University. From other sources (Small MSS., n.d.), Professor Baker further informs us that W. W. Elwang, who translated Simmel's essay on the sociology of religion for the *AJS*, was a student of Ellwood in Missouri.

<sup>6</sup> Those seven were Charles A. Ellwood, Edward C. Hayes, and Howard J. Woolston (who later taught briefly at Chicago), all students of Albion Small sent by him to study with Simmel in Berlin; Robert E. Park; Samuel Ratcliffe and Carl Taylor, both students of Park; and Melvin Vincent, a student of Park's student Emory Bogardus. The other four were Frederick A. Bushee, a Harvard student who attended Simmel's lectures in 1900; Charles Gardner, a professor of "Homiletics and Sociology" at Southern Baptist Theological Seminary; Elsa Kimball, a student of Giddings at Columbia who went to Germany in the 1920s; and Pitirim A. Sorokin. This information is based on material obtained by Paul J. Baker from documents of the Pennsylvania Historical Collections at Pennsylvania State University.



to embrace Simmel's definition of the domain, he remained steadfast in his efforts to secure a proper hearing for Simmel's formulations. In the year before he died, he expressed one last "devout hope" that American sociologists would be wise enough to familiarize themselves with Simmel's social theory (Small 1925, p. 87).

Activity on Simmel's behalf at Chicago continued under Robert Park, who succeeded Small as the dominant figure in the department. Apart from the interest in Simmel which Park kindled in his own students, he gave a major boost to the diffusion of Simmel's writings through the list of readings assembled with his junior colleague, Ernest W. Burgess, and published in 1921 by the University of Chicago Press as their *Introduction to the Science of Sociology*. The Park and Burgess volume included 10 selections by Simmel, some of them new translations made by Park—many more selections than were drawn from any other author. "Park and Burgess" became the most influential introduction to sociology in the United States in the 1920s and 1930s, playing a major role in the exposure of generations of sociology students to Simmel's writings.

Although almost no new translations of Simmel's work were published in the United States for nearly three decades after the Park and Burgess volume, another contribution to the diffusion of Simmel's sociology was issued by the University of Chicago Press in 1925. This was Nicholas J. Spykman's *The Social Theory of Georg Simmel*. Spykman summarized much of Simmel's sociological material in a systematic, albeit uninspired, fashion, thereby enabling American students to become acquainted with some ideas contained in Simmel's untranslated works.

By 1930, then, three decades of students at the preeminent department of sociology in the United States had been exposed to men who had had personal contact with and enthusiasm for Simmel, and through the publications of the University of Chicago Press the fledgling sociological community in the country had been given readier access to a small but provocative sampling of Simmel's sociological thought.

By the end of this period, the process of drawing attention to Simmel was no longer confined to Chicago—nor was all of it favorable. In *Contemporary Sociological Theories* Pitirim A. Sorokin devoted considerable space to Simmel, rejecting his sociological enterprise as misguided in aim and flawed in execution, and warning that "to call the sociologists back to Simmel, as Drs. Park and Spykman do, means to call them back to a pure speculation, metaphysics, and a lack of scientific method" (1928, p. 502, n. 26).<sup>7</sup> Theodore Abel, in an energetically argued critique of Simmel's "formal sociology" in *Systematic Sociology in Germany*, faulted Simmel

<sup>7</sup> In addition, Sorokin included an abridged translation of Simmel's essay on the metropolis and mental life, made by an anonymous graduate student, in the compendium coedited with Carle C. Zimmerman and Charles J. Galpin (1930).

for propounding an approach which was at variance with his actual substantive analyses, a discrepancy which "has deprived him of [making] valuable contributions to sociology" (1929, p. 48).

It must be said that all three secondary writings grossly misrepresented the scope of Simmel's achievement. Spykman's exposition was a dry skeleton that gave no hint of the luminosity of Simmel's mind or the trenchancy of his sociological perceptions. Sorokin and Abel misunderstood Simmel's distinction between form and content; both failed to represent any of his substantive materials adequately. Even so, the intensity of their protests indicated that Simmel was a sociologist with whom one had to reckon (Abel even noted that the works of the four authors discussed in his monograph [Simmel, Vierkandt, von Wiese, and Max Weber] were, *excepting Simmel's*, comparatively unknown in the United States at that time). The existence of so much commentary, both positive and negative, doubtless furthered the process of acquainting American sociologists with Simmel.

A somewhat different gloss was presented in the preface to the 1930 edition of *Sociology* by Edward C. Hayes: In Hayes's opinion, the chief theoretical development in recent years had been the continuation of Simmel's effort to circumscribe the field of sociology within a limited domain.

### 1930-55: Recovering the European Classics

From the early 1930s to the late 1950s, American sociology appears to have been animated by two rather different kinds of value orientations. One, representing what has been called the dominant American ethic of instrumental activism, motivated the development of new observational technologies, substantive specializations, and responsiveness to societal demands for the services of social research. The other orientation, that of a small minority, was concerned chiefly with the quality of thought in intellectual work. Expressed by those who sought to introduce a finer philosophical tone into the discipline (and were perhaps sensitive to the threat to European civilization posed by the rise of Nazism), it actuated the efforts of Europe-oriented American scholars and European immigrants who sought to acquaint American colleagues with rich works from the European literature which had been neglected.

It was chiefly the latter impulse which sustained the limited interest in the works of Simmel throughout the second period. At the University of Chicago, a tradition of interest in Simmel was symbolically institutionalized with the publication in 1931 of Park's 1899 notes on Simmel's Berlin lectures, issued by the Society for Social Research, a newly formed organization of faculty and graduate students from the Chicago department. The tradition of teaching Simmel at the University of Chicago was con-

tinued by Park's students, Louis Wirth and Everett C. Hughes, and by their younger colleague, Edward A. Shils. In 1936, Shils translated Simmel's essay on the metropolis and mental life for the benefit of the students at Chicago. Hughes published his translation of "The Sociology of Sociability" in the *AJS* in 1949.

Meanwhile, instruction concerning Simmel was spreading to other universities. Talcott Parsons, who studied in Germany in the mid-1920s and became familiar with Simmel's work as well as that of Max Weber and other German scholars, offered an influential course on social theory at Harvard in the early 1930s which considered the works of Simmel, Durkheim, Hobhouse, and Weber. Robert K. Merton has attributed the origins of his interest in Simmel to that course, as well as to the Simmel selections in the Park and Burgess volume and the numerous references to Simmel in Sorokin's *Contemporary Sociological Theories*.<sup>8</sup>

Merton himself went on to make Columbia University an important center for the diffusion of Simmel's sociology. Having acquired a personal copy of *Soziologie* during a trip to Europe in 1937, Merton soon after began to affect generations of students through concentrated attention to Simmel in his basic courses on the history of sociological theory and analysis of social structure. Merton's interest in Simmel came to be shared by several of his students at Columbia, including a number who figure prominently later in this paper: Peter M. Blau, James S. Coleman, Lewis A. Coser, and Charles Kadushin.

Another center for the teaching of Simmel appeared in the 1930s with the establishment of the University in Exile at the New School for Social Research in New York City. Created to provide a haven for European scholars who had escaped the menace of Nazism, the University in Exile (shortly renamed the Graduate Faculty of Political and Social Science) symbolizes the major impetus of the second period much as the University of Chicago symbolizes that of the first. A new wave of interest in Simmel in the 1940s and 1950s was engendered by the teachings and translations of a distinguished group of immigrants: Alfred Schutz and Albert Salomon at the New School itself and, elsewhere in the country, Reinhard Bendix, Lewis A. Coser, Hans Gerth, Gustav Ichheiser, Kurt Lewin, and Kurt H. Wolff. Wolff's translation of a substantial amount of new material from the Simmelian corpus, published by the Free Press in 1950 as *The Sociology of Georg Simmel*, was more than any other single factor responsible for the revival of American students' interest in Simmel in the postwar years. It was followed, in 1955, by the translation of two more chapters of *Soziologie* by Wolff and Bendix (Simmel 1955).

By the end of this period, a baseline of "missionary" work on behalf of

<sup>8</sup> Personal communication.

Simmel had been established. It is worth noting that nearly every translation of Simmel into English and nearly every major presentation of Simmel through university instruction had been the work of Americans who had been in Germany, immigrants from central Europe, or students of either or both of these two small groups.

#### 1955-75: Codifying Traditions of Inquiry

During the early 1950s, Simmel was still regarded by many American sociologists as a talented but archaic figure who could readily be dispensed with in any properly scientific view of the discipline. This view was buttressed by the fact that the most prestigious synthesis of major developments in early 20th-century social theory, Parsons's *The Structure of Social Action*, concentrated on the works of Pareto, Durkheim, and Weber, and did not include a section (written, but never published) on Simmel. The impression given by this authoritative volume was that Simmel was not a figure with whom serious students had to reckon.

That impression was shaken by a profusion of statements to the contrary in the latter half of the decade. At that time American sociology was starting to become more self-conscious: to codify its achievements, reassess its past, and reconsider its theoretical orientations. These impulses were reflected in a number of efforts to integrate Simmel into a more carefully wrought reconstruction of the sociological tradition and to mine his works for what appeared to contain a considerable untapped theoretical resource for contemporary sociology.

In two year-long graduate seminars on the theory of organization Merton and his students systematically combed Simmel's sociology for ideas germane to a codification of group properties. In personal communications about those seminars, Merton has written that they "focussed on Simmel's work as an indispensable point of departure for developing a problematics of social structure and organization. . . . I had become convinced . . . that there was much in Simmel that would lend itself to a style of theorizing he himself could never bring himself to do." Some of the results of that inquiry were published in Merton's lengthy chapter, "Continuities in the Theory of Reference Groups and Social Structure," in the 1957 edition of his *Social Theory and Social Structure*.

In the same year Donald N. Levine presented a dissertation entitled "Simmel and Parsons: Two Approaches to the Study of Society" at the University of Chicago. He maintained that Simmel's approach remained rich and fruitful, providing a sociology different from but complementary to the orientation which Parsons had developed on foundations laid by Pareto, Durkheim, and Weber.

Reprints of several of the Simmel translations were included in two

quickly accepted anthologies of sociological theory produced in 1956 and 1957.<sup>9</sup> Parsons himself contributed to the movement toward a revised appreciation of Simmel by including five selections from Simmel in the masterful anthology he edited with Shils, Naeglele, and Pitts, *Theories of Society* (1961).

Two publications designed to commemorate the centenary of Simmel's birth gave further notice that his sociology was still a challenge for critical scholarship and a fertile source of stimuli for the sociological imagination. The May 1958 special issue of the *AJS*, edited by Peter H. Rossi, included a number of articles which sought to elaborate some of Simmel's key ideas. A commemorative volume, *Georg Simmel, 1858-1918*, edited by Kurt H. Wolff and published by Ohio State University Press the following year, contained exegetical articles as well as new translations of Simmel's essays. These were followed by a small but steady stream of additional translations: the little monograph on religion (Simmel 1959), further chapters from *Soziologie* and *Philosophie des Geldes* (Simmel 1971), and several of the philosophical essays (Simmel 1968, 1971).

A similar message was broadcast at the Durkheim-Simmel Centenary Session of the American Sociological Association in 1958. Reversing the harsh negative assessment of Simmel's work he had expressed three decades earlier, Theodore Abel (1959, p. 474) proclaimed that Simmel "could justifiably be regarded as the founder of modern sociology" and suggested that it had taken the many years of development in sociology since Simmel's death to enable the profession to appreciate his importance. Robert A. Nisbet fully concurred with this reappraisal and added that "of all the pioneers, Simmel is the most relevant at the present time" (1959, p. 480).

By the 1960s Simmel's work was being taught in numerous universities, and one can no longer speak of one or a few special centers of diffusion. Even so, the presence of both Coser and Wolff at Brandeis University during that decade made its department a particularly favorable ambience for the serious study of Simmel. One of the products of that milieu, Murray A. Davis, has gone on to become a spirited advocate of what he considers Simmel's distinctive approach to sociology (Davis 1973).

Throughout this third period of diffusion there appeared efforts to reinterpret the classical tradition of sociological theory in which Simmel was accorded a more prominent place than ever before. Important syntheses by Don Martindale (1960), Robert A. Nisbet (1966), and Lewis A. Coser (1971) devoted considerable attention to Simmel's place in the history of social theory. Coser also produced a volume of choice selections from the secondary literature on Simmel for the Prentice-Hall series, *Makers of*

<sup>9</sup> Borgatta and Meyer (1966) reprinted four of the Small translations. Coser and Rosenberg (1957) reprinted three of the Wolff translations.

*Modern Social Science*, in 1965. In other works emphasizing the emergence of contemporary theory from its classical background, most notably Alvin Boskoff's *Theory in American Sociology* (1969), Simmel was held to have contributed a number of key components to the prevailing orientations of American sociologists.

If Simmel's work has been diffused to the point where its status as a significant component of American sociology now seems assured, it should nevertheless be noted that parts of *Soziologie*, many articles, and nearly all of *Philosophie des Geldes* remain untranslated; that a thorough, critical study of Simmel's sociology has yet to be undertaken; and that even when American sociologists have drawn on Simmel's ideas in their own research they have often patently misrepresented what Simmel was saying, as we show below. The task of integrating Simmel into American sociology remains a challenge.

#### MODES OF INCORPORATION

It is one thing to admit an author's work into a select list of classical readings and another to utilize that work as a point of departure for scientific inquiry. Received as a classic, the work becomes an intrinsically valuable object: an exemplary specimen of intellectual creativity and a statement about the human condition so rich and so deep that it rewards the attention of each new generation. It seems fair to state that many of Simmel's sociological writings that have been translated into English have attained the status of classics in the curriculum of higher learning in America.

To assess the status of Simmel's work as a source of stimuli for further scientific inquiry is more problematic. There are many variations with respect to both what has been incorporated and how it has been taken. One must distinguish, in speaking of Simmel's influence, among borrowings that range from particular observations and specific hypotheses to general theoretical orientations and the definition of significant problem areas. In addition, one must take care to determine whether the borrowings are real or only apparent; if real, whether direct or retrospective; if retrospective, whether they represent prediscoversies, anticipations, or merely adumbrations.<sup>10</sup>

In discussing the extent of Simmel's influence we try to be mindful of the latter sets of distinctions. As for the content to be considered, we deal, in the next section, with general theoretical orientations and, in the remaining sections, with those problem areas whose treatment by Simmel appears to have had the greatest impact on American sociology: the

<sup>10</sup> For clarification of the meanings and relationships of these concepts, and the mandate to scholarship implicit in their use, see Merton (1968, chap. 1).

stranger and social distance (Part I), and urbanism, interaction in small groups, secrecy and social knowledge, conflict, and exchange (Part II).

#### GENERAL THEORETICAL ORIENTATIONS

Even though Simmel failed to articulate all his sociological principles in a systematic manner, his general position can be represented by the following major assumptions:<sup>11</sup>

1. *Sociology* should restrict its inquiries to a precisely demarcated terrain identified on the basis of a technically specific definition of its core concept: society.

2. *Society* is to be viewed neither as a corporate entity distinct from and exerting constraints upon individuals nor as an aggregation of corporate entities such as classes and elites nor as an epiphenomenon reducible to the motives and acts of individuals, but rather as the modality of interaction among individuals—the general process and particular processes of *Vergesellschaftung* (which has been translated variously as societalization, sociation, and association).

3. *Individuals* enter into interaction with one another (i.e., form “society”) in order to satisfy such basic needs as those for companionship and the expression of aggression and to pursue such goals as income, territory, salvation, education, and the like. Individuals enter into interaction with only parts of themselves: an individual always stands both within and outside social interaction.

4. The defining characteristic of interaction is *reciprocity* of effect: A acts on B and in turn responds to B's reaction to him. Interactions differ with respect to the degree of symmetry of such reciprocity, but it is always there to some extent. All human interactions should be viewed as kinds of *exchange*.

5. Interactions take place in discrete identifiable *forms*, such as competition, conflict, super- and subordination, and sociability, having determinate properties that to some extent remain constant despite variations in content, that is, in the purposes served by the interactions.

6. Interactions relate individuals who stand at varying distances from one another. A major variable in forms of interaction thus concerns the amounts and combinations of horizontal and vertical *distance* they entail. This is distinct from what may be called the valence of interactions—whether the affect in question is positive or negative.

7. Every tendency in interaction is to some extent balanced by an opposing tendency. The fundamental *dualism* of social life stems both from man's ambivalent instinctual dispositions and from the need for

<sup>11</sup> Earlier versions of these formulations of the basic principles of Simmel's sociology appear in Levine (1959; 1971, pp. xxxi–xliii).

some ratio of discordant to harmonious tendencies in order that society may have a determinate shape. The principal sociological dualisms are conformity and individuation, solidarity and antagonism, publicity and privacy, compliance and rebelliousness, and constraint and freedom.

8: *Cultural* forms emerge in social interaction and become fixed. As such, they stand in a relationship of perpetual tension with the ongoing life processes, which bring about recurrent efforts to modify those forms or create new ones.

Several efforts by American sociologists to construct a general theoretical framework reflect Simmel's influence. They differ from one another largely in the kinds of selections made from the eight principles just presented. This selectivity has been based, for the most part, not on a careful analysis of which of Simmel's ideas appear viable and which not, but on a relatively haphazard reading of his work. Owing to this carelessness, most of them either ignore other ideas of Simmel which are germane to and should have been incorporated into their statements or deviate from Simmel's position without noting, let alone seeking to justify, the divergence.

Although *Introduction to the Science of Sociology* was highly eclectic in the best sense of the term, its central chapters articulate a coherent theoretical orientation which must be quite what Park had in mind when he wrote that Simmel was the man from whom he "finally gained a fundamental point of view for the study of . . . society." These chapters, which contain the 10 selections by Simmel, fall into two parts. Chapters 4-7 treat the ways by which individuals move from isolation into contact and then ongoing interaction and conceptualize the phenomenon of social distance along lateral and vertical dimensions. Chapters 8-11 discuss the four major interaction processes identified by Park: competition, conflict, accommodation, and assimilation. The heart of the volume thus outlines a program for sociology grounded on principles 5 and 6: a concern with distance as the key dimension of society and with specific forms of interaction as they appear in varying circumstances.<sup>12</sup>

Regarding Simmel's basic conception of society, however, Park's position is notably ambiguous. At some points he seems to follow Simmel in equating society with human interaction, pure and simple—particularly when he suggests that the "four great types of interaction" underlie the four main types of "social order": economic equilibrium (competition), political order (conflict), social organization (accommodation), and

<sup>12</sup> Guided by this theoretical orientation, Melvin Vincent, a student of Park's student Emory Bogardus, produced a dissertation (1928) entitled "A Study of Accommodation as a Conscious Social Process Reflected in Employer and Employee Relationships." In it he states that "the sociology of this dissertation is based largely upon that of Georg Simmel. The forms (processes) of interaction must be abstracted from the complex phenomena (content) of social life."



personality and the cultural heritage (assimilation) (Park and Burgess 1921, pp. 506–10). More characteristically, and especially in his later work, however, Park leaned toward the more conventional identification of the social with the *moral* order, which he contrasted with the ecological order derived from the mere pursuit of individual self-interest. "Society," Park asserts, "may be said to exist only so far as this independent activity is *controlled* in the interest of the group as a whole" (Park and Burgess 1921, p. 508). Social control, then, became for Park and Burgess "the central fact and the central problem of society"—not the normatively indifferent phenomena of human association, as with Simmel—and sociology "a point of view and a method for investigating the processes by which individuals are inducted into and induced to cooperate in some sort of permanent corporate existence which we call society" (ibid., p. 42).

A more explicit attempt to build a general sociology on Simmelian principles was made by Leopold von Wiese in his *System der allgemeinen Soziologie* (1924–28). Howard Becker, who was an exchange fellow at von Wiese's institute in Cologne in the 1920s, undertook on returning to the United States to work out an adapted and amplified version of that monograph. It was published in 1932 as *Systematic Sociology*. In new chapters written for the volume, Becker paid great tribute to Simmel for having enunciated the first principle—the need to demarcate a scientific terrain peculiar to sociology—and followed Simmel and von Wiese by making that terrain the abstracted properties of "specifically interhuman" phenomena.

Like Park and Burgess, von Wiese and Becker proceed from a discussion of isolation and contact to a classification of interaction patterns. Three sets of distinctions undergird Becker's elaborate taxonomy. One is the broad division of interhuman phenomena into action patterns of *association* and *dissociation*, both categories to be subsumed under the inclusive concept of *sociation*. The second distinction is between relationships and processes, that is, the static aspect of *sociation* represented in terms of distance and the dynamic aspect represented in terms of motion. A third distinction separates elementary types of process or relationship called "action patterns," like adjustment, amalgamation, competition, and conflict, from structured clusters of relationships and processes called (unhappily) "plurality patterns" (Simmel's and von Wiese's *Gebilde*). In addition, differentiation, integration, and "constructive and destructive processes" are identified as special forms of association and dissociation: the former differ from "common-human" patterns in that they take place only within and between plurality patterns.

Becker's adaptation of von Wiese was one of the most determined attempts in American sociology to construct a general theory along lines staked out by Simmel. Widely regarded as an uninspired work of taxon-

omy, the book had relatively little impact on sociology itself. Nevertheless, the basic idea of classifying social phenomena into basic processes of association and dissociation subsequently became popular in several branches of American social psychology.<sup>18</sup>

If Becker is to be viewed as a follower of Simmel, however, it should be noted that he, like Park, failed to represent some of the most distinctive features of Simmel's conception of social conflict. By equating societal order with the moral order produced by accommodation and assimilation, Park came to see conflict as a source of disruption and necessary change, thus neglecting Simmel's emphasis on the ways in which conflict, as routinized antagonism, serves also to constitute and stabilize social order. Like Simmel, however, Park did see conflict as a process which brought people into closer contact, whereas Becker classified conflict and "contravention" as forms of *dissociation*—directed, that is, toward avoidance and away from social contact.

Since World War II a number of scattered efforts to crystallize a general orientation in sociology have been made by authors seeking to revive and extend Simmel's approach. One such effort has been animated chiefly by devotion to the second principle: the conception of society as interaction process as the fundamentally appropriate image for sociological inquiry. Proponents of this line of thought have found it worthwhile to return to Simmel's own point of departure in developing the notion, that is, to an explicit repudiation both of organismic models of society and of models which reduce society to purely individual phenomena and their mechanical interactions.

Werner Stark argues that these models repudiated by Simmel have been the dominant metaphors for society in Western intellectual history and suggests why they have continuously reappeared despite the patent flaws of each. Although he faults Simmel for lack of consistency on the question, Stark writes that Simmel was "the thinker who, perhaps more ably than any other, drew the necessary conclusions from the fact . . . that both philosophical realism [organicism] and philosophical nominalism [mechanism] are inappropriate in sociological theory." Stark concludes that "what we must above all retain out of Simmel's far-flung investigations is, of course, his definition of society. This, as we have seen already, is simple enough. 'Society exists where several individuals enter into interaction.' . . . Nothing could be more elementary and nothing more true" (1963, pp. 213, 216).

A similar argument was set forth by Hugh Duncan, urging that society be viewed as a process of communication: "Following Simmel, we argue that the study of society is the study of the forms of sociation. But we

<sup>18</sup> See, for example, Bales 1950.

argue further—and here our clue is supplied by Mead and Burke—that the data of sociation exist in the various kinds of symbolic expressions men use to enact their social roles in communication with one another” (1962, p. xvii).

Independently of both Stark and Duncan, Walter Buckley has used a more elaborate technical vocabulary to make essentially the same point. Urging American sociologists to abandon “outmoded” conceptions of society predicated on mechanical equilibrium models and organismic homeostatic models, Buckley advocates a model incorporating general systems research, cybernetics, and information and communication theories. Its main feature would be an emphasis on process—“a move down from structure to social interrelations and from social relations to social actions and interaction processes” (1968, p. 499)—and on the continuous activity by which complex adaptive systems create new forms in response to changing conditions.

Buckley gives Simmel credit for anticipating the basic thrust of a “process” model. After criticizing modern sociological theory for being dominated by mechanical-organismic models with their overemphasis on fixed structures, he notes that “mention should be made of the ‘process’ model, which was a predominant point of view in American sociology of the early twentieth century under the leadership especially of the ‘Chicago school,’ including, in particular, Albion W. Small, G. H. Mead, R. E. Park and W. E. Burgess, who in turn were stimulated by such German sociologists as G. Simmel and L. von Wiese” (1967, p. 17). Buckley observes that “though Simmel and others focused on ‘forms’ of interaction, their emphasis was always on the interaction as process rather than on the ‘forms’ ” (ibid., p. 19). In Buckley’s view, this earlier emphasis on process can be considered a genuine anticipation of the basic principles of cybernetics. What he fails to appreciate is that the other phenomenon he emphasizes, the “morphogenic” properties of sociocultural systems, was stressed no less heavily by Simmel. Presented as the eighth principle in our outline, the significance of continual morphogenesis was a major theme in Simmel’s later writings.

Recent work on a general theory of social exchange has been in a similar vein. Stressing Simmel’s fourth principle more than the abstract conception of society as process, its productions have been more substantively focused. Contributors to it represent diverse research traditions, some linked to Simmel, others not. A major force behind this development came from the efforts of various scholars in the 1930s and later to study the properties of directly observed transactions in small groups. That movement was to some extent given cohesion and greater visibility by George C. Homans in *The Human Group*. Although Homans did not acknowledge or utilize Simmel’s work in that volume, an intellectual

affinity is suggested in the introduction by Merton, who wrote: "Not since Simmel's pioneering analyses of almost half a century ago has any single work contributed so much to a sociological theory of the structure, processes, and functioning of small groups as George Homans' *The Human Group*" (Homans 1950, p. xxiii).

In work published a decade later, Homans modified his approach in two ways: he shifted from the study of small groups to the more abstract phenomenon of elementary social behavior and from a classification of descriptive accounts of interaction systems to a generalized explanatory theory of human exchange. In doing so, he found it necessary to derive the theoretical grounding for *Social Behavior: Its Elementary Forms* (1961) from disciplines outside sociology: behavioral psychology and elementary economics.

Stimulated by Homans's work, Peter M. Blau sought to advance the general theory of social exchange in ways that took account of emergent social structural properties and rooted the theory more firmly within sociology. Blau began by establishing a link with "the theoretical tradition of Simmel." In a chapter entitled "The Structure of Social Associations," he wrote: "To speak of social life is to speak of the associations between people. . . . Simmel's fundamental postulate, and also that of this book, is that the analysis of social associations, of the processes governing them, and of the forms they assume is the central task of sociology. The title of this first chapter can be considered a free translation of Simmel's basic concept, 'Die Formen der Vergesellschaftung'" (1964, p. 12).

Thanks to the creative work of Homans and Blau, "exchange theory" quickly assumed a prominent position in American sociology. This was much like what happened with "conflict theory," largely in response to Lewis Coser's (1956) critical codification of Simmel's essay on conflict. Indeed, conflict and exchange theories soon came to be presented as alternative if not competitive perspectives in standard courses and textbooks on sociological theory.

For Simmel, however, conflict and exchange were simply two intimately related general forms of interaction—a point which Walter L. Wallace is one of very few recent theorists to have grasped. Wallace complements Blau's perception that exchange processes generate power differentials which in turn stimulate forces promoting opposition and cleavage with Simmel's perception that peaceful exchange is often a substitute for conflict. For such reasons, Wallace argues that "conflict theory" is logically a variant of "exchange theory," concerned with the exchange of injuries rather than of benefits (1969, pp. 31–34).

Other efforts to shape "new" orientations in American sociology have been stimulated by somewhat different aspects of Simmel's thought. Simmel is frequently invoked to legitimate and provide insights for a

"sociology of everyday life" carried on by proponents of what have been called phenomenological and ethnomethodological approaches to sociology and social psychology. In this endeavor, the third principle mentioned above is often stressed: the idea that individuals actively construct their social relationships and are never wholly absorbed by them. In a number of brilliant studies in this genre, Erving Goffman has drawn fruitfully on Simmel's work at many points. Alfred Schutz served to mediate between Simmel (as well as Husserl) and a recent group of sociologists of everyday life which includes Harold Garfinkel, Aaron Cicourel, and Jack Douglas. In this vein, Stanford Lyman and Marvin Scott have propounded *A Sociology of the Absurd* (1970), for which Simmel is called second only to Machiavelli as a precursor.

Of all the basic ideas informing Simmel's social thought, that which has received the least attention in American sociology is the seventh principle: the conception of the fundamentally dualistic character of social life. We know of only one instance in which Simmel's dualistic mode of analysis was cited as exemplary, the brief invitation for sociologists to think explicitly in terms of pairs of opposed concepts which Reinhard Bendix and Bennett Berger issued in 1959. It was little heeded.

#### THE STRANGER

The segment of Simmel's work which had the earliest and perhaps most publicized impact on American social research appears in the only chapter of *Soziologie* not yet translated into English: chapter 9, "Der Raum und die räumlichen Ordnungen der Gesellschaft." Although the bulk of that chapter, on how the properties of physical space provide both conditions for and symbolic representations of different kinds of social interaction, had no perceptible influence, its short excursus "The Stranger," first translated by Park for inclusion in *Introduction to the Science of Sociology*,<sup>14</sup> has been widely acknowledged as a stimulus to two prominent research traditions: the social psychology of the stranger and the measurement of social distance.

What Simmel did in that famous excursus was to make vividly clear just how problematic is the whole question of being a member of a group. Numerous inquiries on this general topic have consequently taken Simmel's formulation as a point of departure, even though they were headed in several different directions.

In a seminal essay, "Human Migration and the Marginal Man" (1928), Park cited Simmel's definition of the stranger and proceeded to delineate a concept of the marginal man as its equivalent—an equivalence illustrated

<sup>14</sup> Two later translations of this essay appear in Simmel (1950, 1971).



by his remark that "the emancipated Jew was, and is, historically and typically the marginal man . . . he is, par excellence, the 'stranger,' whom Simmel, himself a Jew, has described with such profound insight and understanding" (Park 1928, p. 892; reprinted in Park 1950). Commenting on this adaptation, Boskoff recently observed that "Park borrowed the concept of the stranger [from Simmel] and applied it to the phenomena of migration and culture contact in complex society. Briefly, Park suggested that various kinds of deviant behavior (crime, delinquency, illegitimacy) reflected the experience of persons who, by migrating, had given up old values but had not adequately acquired the norms and skills of their new setting" (1969, pp. 282-83).

It should be clear, however, that in the borrowing Park altered the shape of the concept: his "marginal man" represents a configuration notably different from Simmel's "stranger." Thinking of the experience of ethnic minorities in zones of culture contact in American cities, Park conceived the marginal man as a racial or cultural hybrid—"one who lives in two worlds, in both of which he is more or less of a stranger"—one who aspires to but is excluded from full membership in a new group. Simmel's stranger, by contrast, does not aspire to be assimilated; he is a potential wanderer, one who "has not quite got over the freedom of coming and going." Whereas Park's excluded marginal man was depicted as suffering from spiritual instability, intensified self-consciousness, restlessness, and malaise, Simmel's stranger, occupying a determinate position in relation to the group, was depicted as a successful trader, a judge, a confidant, and a personally attractive human being.

In an extended study of *The Marginal Man* (1937), Park's student, Everett V. Stonequist, indicated his awareness that Park's marginal man was not identical with Simmel's stranger. He observed, first, that marginality need not be produced by migration but could also come about through internal changes like education and marriage. More explicitly, he stated: "The stranger, Simmel writes, first appears as a trader, one who is not fixed in space, yet settles for a time in the community—a 'potential wanderer.' He unites in his person the qualities of 'nearness and remoteness, concern and indifference.' . . . This conception of the stranger pictures him as one who is not intimately and personally concerned with the social life about him. *His relative detachment frees him from the self-consciousness, the concern for status, and the divided loyalties of the marginal man*" (1937, pp. 177-78; italics ours). Stonequist went on to note that the distinctive properties of the stranger identified by Simmel are lost if an individual moves into the position of being a marginal person.

In spite of Stonequist's clarity, a tendency to confuse the marginal man with Simmel's stranger has persisted. Thus, more than a decade later,

Hughes (1949) uncritically repeated Park's view that Simmel's passages on the stranger referred to the same phenomenon as the marginal man. Boskoff, with comparable carelessness, glossed Simmel's stranger as "vulnerable to internal uncertainties" (1969, p. 282). Seeking to "re-examine the ubiquitous concept of 'marginal man,'" Peter I. Rose did so by asking "how the 'stranger' in the midst of alien territory adapts to community life." After interviewing exurban Jews in several small towns of upstate New York, Rose concluded that their position could be described more aptly as one of duality than as one of marginality; for they felt "we have the best of both." Rose considered his findings to provide evidence against the applicability of the concept of marginality and to refute the view of "Stonequist [*sic*], Park and others who have characterized the Jew as a disturbed marginal man, an eternal stranger [here Rose footnotes Simmel!] unable to reconcile the traditions of his people with the counterforces of the majority world" (1967, p. 472). In making this point, Rose, like Hughes and Boskoff, was misreading Simmel through Park's distorting lens. What he in fact found was that the Jews in question were not adequately characterized by Park's concept of marginality but that they might indeed be characterized in terms of Simmel's concept of the stranger.

If Stonequist's distinction between marginality and strangerhood was made only to be lost, it was inadvertently recovered by Paul C. P. Siu. In his investigation of Chinese laundrymen in Chicago, originally carried out as a study of "marginality," Siu was dismayed to find that "none of the Chinese laundrymen I studied could be considered a marginal man." Siu, however, did not use his findings to invalidate Simmel's concept of the stranger. Instead, he returned to Simmel to raise the question whether the marginal man might not more aptly be viewed as one of a number of variant types of stranger. Siu then proposed a new type, the sojourner—who, in contrast to the bicultural complex of the marginal man, clings to the culture of his own ethnic group—and added a few notes on still another type of stranger, the settler (Siu 1952). The way was thus opened for a more differentiated view of phenomena previously lumped together under the diffuse categories of strangerhood or marginality.

A related step, albeit in a different direction, had been taken around the time of Stonequist's study. Margaret Mary Wood's *The Stranger: A Study in Social Relationships* (1934) drew freely on Simmel but adopted a definition of the stranger that was clearly differentiated from Simmel's: "We shall describe the stranger as one who has come into face-to-face contact with the group for the first time. This concept is broader than that of Simmel, who defines the stranger as 'the man who comes today and stays tomorrow, the potential wanderer, who although he has gone no further, has not quite got over the freedom of coming and going.' For us

the stranger may be, as with Simmel, a potential wanderer, but he may also be a wanderer who comes today and goes tomorrow, or he may come today and remain with us permanently" (pp. 43-44).

In other words, Wood's topic was not the sojourner but the newly arrived outsider, and her concern was with those internal changes through which different types of groups adapt to his arrival in their midst. Her work might have laid the groundwork for an extensive sociology of the stranger in which Simmel's formulations might properly have been understood as referring to a specialized social type; but, as McLemore (1970) stresses in a spirited review of some of the voluminous literature related to Simmel's essay, subsequent sociologists of the stranger tended to cite Simmel as a primary point of reference for the general topic and, even when citing Wood, tended to miss the distinction between Wood's newly arrived outsider and Simmel's stranger. Thus Julian Greifer, in a suggestive reconstruction of the evolution of attitudes toward the stranger in ancient Judaism, defines the stranger as one who "has come into face to face contact with the group for the first time," and in the next sentence refers to this stranger "*as described by Georg Simmel*" (1945, p. 739; italics ours). Similarly, Oscar Grusky (1960) confuses the newly arrived with Simmel's stranger by using the latter concept in describing the position of a newcomer in a line of administrative succession.

In this context it is instructive to examine an experimental study which claims to draw inspiration from Simmel, "The Stranger in Laboratory Culture" by Nash and Wolfe (1957). In a series of experiments in which a Rorschach card stimulus was presented to small groups of subjects over a number of "epochs," Nash and Wolfe sought to create a role which "would seem to approximate in the laboratory Simmel's description of the stranger." The hypothesis they tested, however, sprang from the ideas of Park, Stonequist, and others concerning the peculiar creativity of the marginal man. What they found was that the "strangers" proved to be less innovative than other participants in the experiment.

The weakness of this study is that it rests on a double conceptual confusion. It seeks to verify Simmel's formulations about the stranger by using hypotheses devised, not by Simmel, but by others concerned with a different social type, the marginal man; and to do so by constructing an experimental role modeled, not on Simmel's stranger, but on the still different type of the newly arrived. Nash and Wolfe were led by their unexpected findings to draw a distinction between persons socialized in a marginal situation and persons introduced into such a situation briefly as adults. The distinction seems useful and broadly parallels the distinctions noted above between the marginal man and the newly arrived—neither of which, it should be clear, replicates Simmel's own concept of the stranger.

In a study more faithful to Simmel's formulations, "Aggressive Attitudes



of the 'Stranger' as a Function of Conformity Pressures," Robert Zajonc (1952) has in effect recovered the distinction between the stranger and the newly arrived. Linking Simmel's ideas about the stranger's relative independence from local customs with frustration-aggression theory, Zajonc hypothesizes, first, that insofar as strangers are expected to conform to host culture norms and find that expectation disturbing because of conflicts with values brought from their home culture, they will tend to express aggression against those norms; and that such criticism is facilitated by their unique position as strangers in the host society and further reduces the need to conform by devaluing the norms in question. "This relationship," Zajonc notes, "hinges upon the unique role of the stranger, and it consequently cannot be expected to hold for the newly arrived." His second hypothesis, then, is that "attitudinal aggression as a result of frustration in conformity will be greater for strangers with long residence [Simmel's 'stranger'] than for those with short residence [the 'newly arrived']". That hypothesis is supported by his findings.

The studies thus far cited indicate the wide range of responses to Simmel's essay on strangerhood and the fruitfulness of moving toward a more differentiated conception of the modalities of group affiliation. Whether imagined as discrete types or as reflecting variations in such dimensions as length of residence and desire for assimilation, concepts like those of the marginal man, the sojourner, and the newly arrived suggest the richness of the vein struck by Simmel's "Der Fremde."

A number of other studies, however, have stayed much closer to the terrain marked out by Simmel himself. Defining the stranger much as Simmel did, they have used his essay to raise questions about a number of homologous phenomena.

Broadly speaking, these studies are of two kinds. Some are aimed at determining the properties of stranger *communities*. Extending Simmel's remarks on the stranger type to such similarly situated immigrant communities as the Chinese in Southeast Asia, Asians in East Africa, Armenians in Turkey, Syrians in West Africa, Parsis in India, and Japanese in the United States, as well as Jews in Europe, they seek to articulate both the internal organization and orientations of the stranger minority groups and their relations with the host societies. In *Immigrants and Associations*, for example, Lloyd A. Fallers assembled a collection of papers on Chinese, Lebanese, and Ibo immigrant communities. Introducing them with a lengthy excerpt from Simmel's "excellent little essay on 'The Stranger,'" Fallers goes on to note that "Simmel here draws our attention to a fact often ignored by social scientists: human societies are very often culturally heterogeneous, not for 'historically accidental' or 'exceptional' reasons, but because they 'need' to be. . . . The essays in this volume are all concerned with one particular source of cultural diversity—the source Simmel had in

mind particularly in the above passage: that is, the culturally alien community resulting from migration in pursuit of economic opportunity." From the several papers in the volume Fallers observes that the stranger communities exhibit a typical pattern: "a socially segregated and hostilely-regarded community of kinship units, knit together and defended by associational ties" (1967, pp. 8, 12-13).

Edna Bonacich (1973), in "A Theory of Middleman Minorities," seeks to account for the development and persistence of this relational form. The process consists of two stages. First, the immigrants settle in a new territory as sojourners, with the expectation of eventually returning to their homeland. This orientation favors their economic liquidity and social solidarity, traits which elicit pronounced hostility from the host society. This hostility in turn perpetuates their identification with their homeland, leading, for those who cannot or do not return, to a second mode of adjustment, marked by a persisting ambivalence toward the new place of residence that makes them equivalent to the stranger type described by Simmel.

In a second kind of study, Simmel's propositions have been extended to other individual types bearing a formal resemblance to the stranger. One trait which Simmel asserted to be characteristic of the stranger, enhanced objectivity, has figured in a number of discussions of the nature of social knowledge. Park and Burgess were clearly mindful of this issue; following Simmel's essay, they posed for discussion the question "In what sense is the attitude of the academic man that of 'the stranger' as compared with the attitude of the practical man?" (1921, p. 338). Arlene Daniels has dealt with one aspect of this question by discussing the costs and benefits of carrying out social research in a relationship that maximizes the stranger-like traits of the sociologist. If one enters a group as a visibly alien observer, she writes, "the stranger in this instance encounters a situation fraught with all the opportunities for confidence and intimacies which Simmel describes" (1967, p. 267). Dennison Nash, in "The Ethnologist as Stranger" (1963), points out that the stranger's position is not necessarily conducive to cognitive objectivity. His role is beset by ambiguities and inconsistencies which can produce a degree of anxiety that will constrict or even disorganize his perception. Whether or not constrictive perceptual reactions occur depends on such factors as the compatibility of home and host cultures, the stranger's treatment by the hosts, his relative power, the presence or absence of an enclave, and his own potential for adaptation. Considering this last parameter, Nash reviews a small set of data on American anthropologists and concludes that they tend to be "autonomous" rather than "authoritarian" personalities, a finding which implies that they are likely to be adaptive to the role of the stranger and therefore able to cope with field conditions without serious perceptual distortion.

Variations on Simmel's theme that strangers lack the social attachments and moral obligations common to members of the host society appear in a series of imaginatively conceived papers in historical sociology by Lewis A. Coser. Noting that "what Georg Simmel said about the stranger applies with peculiar force to the eunuch: 'He is not radically committed to the unique ingredients and peculiar tendencies of the group,'" Coser (1964) argues that the detachment of the eunuch-stranger from all group involvements makes him an ideal instrument for carrying out the ruler's subjective desires, in contrast with bureaucratic officials who exercise considerable constraint upon the ruler's power. In a subsequent paper Coser (1972) extended the point to deal with uncastrated but politically impotent aliens, as illustrated by the court Jews of Baroque Germany and by Christian renegades who served Ottoman sultans. In addition, Coser (1958, 1965) has applied the insight about the stranger's unconventionality to the biography of Simmel himself, attributing the increased production of unconventional publications and lectures during the latter part of Simmel's career to the fact that he was indeed a "stranger" in the German academy of his time.

In view of the foregoing, by no means exhaustive, survey, we find it easy to assent to the statement with which McLemore (1970) concludes his critique of the literature on Simmel's stranger: "Sociologists would be fortunate indeed if more of their concepts were as productive as the 'stranger.'"

#### SOCIAL DISTANCE

In his note on the concept of social distance, however, we find McLemore misleading. He writes: "The concept of social distance also may be traced to Simmel's 'stranger'; however, social distance was quickly differentiated from the parent concept and since 1924 has guided an independent and cumulative research tradition" (1970, p. 87).

This statement must be qualified in three respects. (1) Simmel's may well have been the first consequential use of the concept of social distance in sociology, but it had previously been used by Gabriel Tarde and, under the guise of moral density, by Émile Durkheim. (2) Simmel's utilization of the metaphor of distance was by no means restricted to his pages on the stranger; it constitutes a pervasive and distinctive feature of his sociology as a whole and of his philosophical thought as well (Levine 1971, pp. xxxiv-xxxv). (3) The research tradition on social distance is to some extent cumulative, but it is also marked by considerable discontinuity and disjointedness. Some of the latter quality reflects the fact that different writers drew on quite different segments of Simmel's sociology in working out their ideas about social distance.

"Social distance" became a prominent concept in American sociology in the 1920s owing to the convergence of a felt need to understand the relations among racial groups in the United States, made especially acute by the influx of large numbers of Orientals and by the hunger of a young scientific discipline for concepts that would lend themselves to quantitative measurement. In the papers by Park (1924) and Bogardus (1925) which launched this research tradition, Simmel was not mentioned. However, Park and Burgess had called attention previously (1921, p. 286) to the fact that, in his essay on the stranger, "Simmel himself employs the conception of social distance in his [identification of the form] of the stranger as the combination of the near and the far." And Bogardus, ruminating on three decades of research on the subject in *Social Distance* (1959, p. 13), cites Simmel's use of "the terms nearness and remoteness in his discussion of 'the stranger'" in the book's first footnote. In any case, it is widely believed that, as Charles Kadushin has put it, Bogardus's social distance scale "made Simmel's notions of social distance more concrete" (1962, p. 519)—in other words, converted the concept into a variable that could be measured.

The Bogardus scale was designed to measure attitudes reflecting the degrees of "sympathetic understanding" which respondents had to members of other groups. The scale was extensively utilized and was largely responsible for the widespread diffusion of the concept. If, however, as Bogardus observed in 1959, "within the last thirty years, the concept of social distance [became] widely accepted in sociology," there is no warrant for the eulogy he appends to that observation: "[Social distance] is a crisp term which conveys its own meaning. It says exactly what it means. It is rarely if ever misunderstood" (p. 7). On the contrary—the literature from those three decades and later ones reveals a continuous unfolding, confusing, disentangling, and crosscutting of several disparate meanings of social distance.

The complexity of this development would not have surprised Simmel, whose own expressed view was that "very manifold meanings [are] encompassed by the symbol of 'distance'" (1908, p. 321; 1955, p. 105). The point is illustrated by the Bogardus scale itself, if it is to be seen as a means of making Simmel's notions of social distance more concrete: it captures *none* of the meanings Simmel had in mind when alluding to social space in "The Stranger," which dealt rather with (1) ecological attachment and mobility, (2) emotional involvement and detachment, and (3) the extent to which persons share similar qualities and sentiments. Proceeding in yet another direction, Sorokin, who cites Simmel as the first modern sociologist to attend to the problem of social space and related questions, defines social distance in terms of the constellation of a person's group memberships and his positions in those groups. He contrasts this "objective and socio-

logical" approach with that of Park and Bogardus, which he calls "purely psychological and subjective (as far as it measures the social distance by the subjective feelings of liking and disliking)" (1927, chap. 1).

A number of other authors have sought to specify the concept of social distance anew by returning to parts of Simmel's work other than "The Stranger." Willard Poole (1927), erroneously thanking Park and Burgess for the transmission, drew on a (then untranslated) passage in Simmel's chapter "The Poor" to ground a distinction between social distance (hierarchical position) and personal distance (emotional involvement), then evolved a fourfold classification by identifying "subjective" and "objective" forms of both social and personal distance. Erving Goffman (1959, p. 69) drew on a passage from Simmel's discussion of secrecy to develop another meaning: the radius of a sphere around a person which marks the distance at which others must stand back. Charles Kadushin cited a passage from Simmel's chapter "Quantitative Aspects of the Group" to produce still another meaning of the concept, averring that "when Simmel first introduced the notion of social distance" he used it to distinguish emotional involvement from cool detachment (1962, p. 517).

Much of the recent work on social distance has been devoted to sorting out some of the "very manifold meanings" of the concept and to devising correspondingly diverse instruments of measurement. Westie (1952, 1953) developed four different social distance scales (residential distance, position distance, interpersonal physical distance, and interpersonal interaction distance). Banton (1960) distinguished four forms of social distance (attitudinal, positional, qualitative, and ecological [labels ours]). Kadushin, in his "effort to re-specify and apply Simmel's ideas," distinguishes still another set of four "dimensions of social distance" (and the relevant indicators needed to tap them): "*Normative distance* refers to the manner and degree of interaction which *ought* to hold between two or more persons or statuses. *Interactive distance* is the degree of *actual* interaction and need not match the normative prescription. . . . *Cultural or valuational distance* is the degree of value homophily that exists between two persons or statuses. . . . *Personal distance* or empathy is the degree of understanding and unspoken communication that takes place between two persons or two statuses—what Simmel had in mind when he wrote of 'personal' relationships" (1962, pp. 519–20).

Perhaps the most rigorous and ambitious work in this research tradition is that of Edward O. Laumann. Concerned that "although Simmel at the turn of the century suggested the promise of research on interaction, remarkably little systematic research in urban settings . . . has been undertaken," Laumann (1966, p. 147) has undertaken major efforts to carry out such research in a systematic manner. He has created instruments to measure both subjective social distance ("an attitude of ego toward a

person . . . that broadly defines the character of the interaction ego would be willing to undertake with the attitude object"), the variable tapped by the Bogardus scale, and objective social distance ("the actually observed differential association of persons of different status attributes in various social relationships") (Laumann 1966, pp. 29-30). He has used these instruments to provide a more empirically grounded understanding of the interaction patterns of social classes in urban communities.

In an appendix to Laumann's most recent work in this vein, David McFarland and Daniel Brown (1973) return to Simmel as a point of departure for developing a rigorous conception of social distance as a "metric," a term used by mathematicians to denote a type of quantity that bears a resemblance to physical distance. Their remarks are essentially negative:

Simmel's discussion of social distance concerned "The Stranger," whom he described as having elements of both nearness and distance. The nearness comes from features held in common with the observer, and the distance comes from the observer's awareness that the features held in common are common to all men or at least to large groups of men. Simmel's use of the concept does not lend itself either to quantification or to a clear analogy with physical distance since in his usage two people can simultaneously be "near" and "distant." His concept of social distance actually seems to be a mixture of two different concepts: features held in common, and the degree of specificity or generality of these common features. . . . Simmel made no attempt to quantify his idea of social distance, or to represent it geometrically as the distance between physical points. [P. 215]

Taking up the challenge implicit in their critique of Simmel, McFarland and Brown propose, through the use of such constructs as "proximity measures" and "relative distance," a set of operations capable of yielding both a social distance function that is a true metric and a dimensional representation of the objects in question that discloses the configuration formed by the set of objects.

The great promise which McFarland and Brown sensed in the metaphor of social distance—that social relations could somehow be represented in mathematical terms in a way that bears some resemblance to the measurement of physical space—had been responded to in other ways within a different research tradition, that of sociometry. Workers in this field developed such constructs as choice and rejection status indices to represent social distances within small groups in mathematical form. Commenting on this earlier development, J. L. Moreno wrote: "Although certain aspects of sociometry and microsociology had already been conceptualized by Simmel, von Wiese, Gurvitch, and myself in Europe, it is still a genuine American movement because it would have died there, whereas here it flowered to great productivity. More than any other living

variety of the human species, the American man loves to express status in figures, he is the '*homo metrum*'" (1960, p. vii).

At several points, then, the work of Simmel provided some stimulation for American sociologists seeking to find ways and means of applying metric techniques to social relations. In the process, however, much of Simmel's own message was lost. How much was lost may be surmised from a close consideration of the assertions by McFarland and Brown quoted above.

Theirs is a distorted reading of Simmel's discussion of social distance because it is based on a biased sample of his statements. While Simmel did to some extent consider "nearness" in terms of features held in common—what McFarland and Brown call a "similarity" notion of social distance, comparable to what Lazarsfeld and Merton (1954) have referred to as "value homophily"—he also employed what they call an "interaction" notion of social distance. Simmel's description of the nearness of the stranger clearly refers in part to the fact that the stranger is someone close by with whom interactions are readily experienced, and his allusions to social distance in other contexts make even greater use of the interactional (and still other) meanings of the concept.

Much more misleading, however, is the statement that Simmel made no attempt to represent social distance geometrically. Here *homo metrum* has denatured the European legacy. Sociology was explicitly conceived by Simmel as a kind of geometry of social relations. Distance, however, was a central analytic concept for him, not because social phenomena might therewith be measured univocally,<sup>15</sup> but because the properties of physical space figure significantly in the symbolic structures through which human actors define, interpret, and organize their social relations.<sup>16</sup> He spent no time looking for a literal social counterpart to physical space, since social distance *was* a metaphor only:

At the moment two persons begin to interact, the space between them appears to be filled and inhabited. This appearance of course rests only on the ambiguity of the concept "between": that a relation between two elements which actually consists only of a certain movement or modification within the one and the other takes place between them in the sense

<sup>15</sup> Indeed, Simmel's formulation that the stranger represents a combination of nearness and distance, far from being a logical flaw, aptly illustrates his more general assumption that all social phenomena reflect a combination of opposed tendencies. This has the interesting methodological implication that many phenomena would be more accurately measured if scales tapping two opposed dimensions were employed. For one example of this possibility, see the conjoint independent measurement of "happiness" and "unhappiness" in Bradburn and Caplovitz (1965).

<sup>16</sup> Levine (1975) argues that the various ways in which Simmel employs the symbolism of simultaneous closeness and remoteness in "The Stranger" in fact provide a basic insight about strangerhood which is of great use in developing a more rigorous and systematic sociology of the stranger.

of a spatial interposition. Although the ambiguity in question may give rise to errors, it represents a matter of quite profound sociological significance. The "Inbetween" as a purely functional reciprocity, whose contents stay within each of the parties to a transaction, also takes the form of a claim on the space which exists between these parties, actually manifesting itself in all cases *between* two spatial locations at which each party has a specially designated place, one to be occupied by himself alone. [Simmel 1908, p. 616, as translated by D. Levine]

It remains for other research traditions within American sociology to appreciate that the principal relevance of the essay on the stranger for the study of social distance may lie in its second sentence: "[The example of the stranger] is another indication that spatial relations not only are determining conditions of relationships among men, but are also symbolic of those relationships."

#### APPENDIX A

Luther L. Bernard collected autobiographical statements from 258 American sociologists in 1927, and Baker, Long, and Quesnel (1973) analyzed some of the data thus obtained. (See tables A1 and A2 for the data on

TABLE A1  
AMERICAN SOCIOLOGISTS MOST FREQUENTLY MENTIONED AS INFLUENTIAL

SOCIOLOGIST	RESPONDENT'S RELATIONSHIP TO SOCIOLOGIST		TOTAL RESPONDENTS
	Student	Reader	
C. Cooley .....	3	53	56
F. Giddings .....	18	31	49
L. Ward .....	2	45	47
E. Ross .....	14	32	46
A. Small .....	23	16	39
R. Park .....	24	7	31
W. I. Thomas .....	14	16	30
C. Ellwood .....	4	23	27
W. G. Sumner .....	2	23	25

influential American and European sociologists.) Our identification of Cooley, Giddings, Ross, Sumner, and Ward as the five most influential American sociologists was based on the reader/author relationship in order to make the findings comparable to the responses concerning European sociologists. Sumner was chosen over Ellwood because of our impression that he was indeed the far more influential of the two—an impression confirmed by the citation data presented in Appendix B. It should also be noted, with reference to our discussion of the first period of diffusion of



## Simmel's Influence on American Sociology. I

Simmel's thought in the United States, that the four other American sociologists included among the nine most influential were associated with the University of Chicago and that, of those, all but Thomas had direct contact with Simmel.

TABLE A2  
FOREIGN SOCIOLOGISTS MOST FREQUENTLY MENTIONED AS INFLUENTIAL

Author	Respondents
H. Spencer .....	23
G. SIMMEL .....	12
G. Tarde .....	11
É. Durkheim .....	7
L. Hobhouse .....	7
A. Comte .....	6

### APPENDIX B

The differential citation of American and European authors (table B1) was recorded for selected sociology textbooks published between 1924 and 1933. Texts where no sociologist was cited 20 times or more were excluded.

TABLE B1  
DIFFERENTIAL CITATIONS OF AUTHORS

AMERICAN AUTHORS		EUROPEAN AUTHORS	
Author	Citations	Author	Citations
Case 1924			
Ross .....	30	Spencer .....	20
Thomas .....	22	Comte .....	10
Ward .....	21	Hobhouse .....	6
Cooley .....	14	Durkheim .....	3
Giddings .....	14	Marx .....	3
Small .....	14	SIMMEL .....	3
Park .....	13	(No citations at all for Pareto, Tarde, Tönnies, or Weber)	
Sumner .....	13		
Ellwood 1925			
Giddings .....	22	Hobhouse .....	18
Park and Burgess .....	18	Comte .....	8
Ross .....	15	Marx .....	2
Cooley .....	13	SIMMEL .....	2
Ward .....	10	Spencer .....	2
Small .....	6	Tarde .....	2
Sumner .....	4	(One citation for Durkheim, none for Pareto, Tönnies, Weber)	
Lumley 1928			
Sumner .....	31	Spencer .....	9
Keller .....	23	Hobhouse .....	3
Park .....	18	SIMMEL .....	3
Ross .....	14	Tarde .....	2
Cooley .....	7	(No citations for Comte, Durkheim, Marx, Pareto, Tönnies, Weber)	
Giddings .....	6		
Dawson and Gettys 1929			
Park .....	49	SIMMEL .....	14
Cooley .....	31	Durkheim .....	10
Burgess .....	28	Westermarck .....	7
Thomas .....	25	Spencer .....	5
Sumner .....	16	Tarde .....	3
Ross .....	15	Comte .....	2
Small .....	14	(One citation for Hobhouse, none for Marx, Pareto, Tönnies, Weber)	
Znaniecki .....	13		
Hiller 1933			
Park .....	56	Hobhouse .....	34
Thomas .....	50	Westermarck .....	33
Cooley .....	49	SIMMEL .....	14
Sumner .....	47	Tarde .....	7
Burgess .....	44	Spencer .....	3
Sorokin .....	37	Durkheim .....	2
Znaniecki .....	37	(No citations for Comte, Marx, Tönnies, Pareto, Weber)	
Ross .....	33		

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# **Continuities in the Study of Ecological Succession: Changes in the Race Composition of Neighborhoods and Their Businesses<sup>1</sup>**

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This paper concerns the problem of how residential succession affects the character of one set of local organizations: small businesses in racially changing neighborhoods. A key issue examined is the extent to which the causes and consequences of residential succession account also for the succession of small-business organizations. Only one type of succession is examined in this paper: the movement of blacks into areas previously occupied by whites. The impact of the transition in residential population on the abandonment of business sites and/or the turnover of business from white to black or Puerto Rican ownership is the particular focus of inquiry. Results of the analysis indicate that the residential succession model fits our data on business succession quite well.

The nature and consequences of residential succession of populations are well documented, but its impact on the character of local organizations is less clearly understood. As the Duncans observed nearly two decades ago, such a concomitant can be established only by empirical investigation: "Accompanying residential succession may be changes in the density of population, the composition of households, the way in which residential structures are used, the character of local institutions, and the like. Such changes, however, are not assumed to be invariable concomitants of succession; their occurrence and intensity must be established by research" (1957, p. 108).

<sup>1</sup> We gratefully acknowledge the extremely helpful suggestions and critical comments of Charles Barresi, Mary Beth Beres, Gerald Gordon, Ivan Light, Harvey Molotch, Robert Stern, Gerald Suttles, Charles Tilly, and Eleanor Wolf. Because of space limitations, this paper does not include a complete review of the previous literature on ecological succession. For a comprehensive review, see Aldrich (1975). Support for data collection and analysis of our four-wave panel study was made available by the President's Commission on Law Enforcement and the Administration of Justice, the Small Business Administration, the National Science Foundation, Yale University, and the New York State School of Industrial Relations. Howard Schwartz and Carl Kolchak provided valuable research assistance, and Carol Cashman patiently typed her way through seemingly unending drafts.

## Race Composition of Neighborhood Businesses

This paper concerns the effects of residential succession on one set of local organizations: small businesses. We examine the extent to which the causes and consequences of residential succession account also for succession among small-business organizations. Only one type of residential succession is considered in this paper: the movement of blacks into areas previously occupied by whites. The impact of this transition on the abandonment of business sites and/or the turnover of business from white to black or Puerto Rican ownership is the focus of inquiry.<sup>2</sup>

### HYPOTHESES ON SUCCESSION AND SMALL BUSINESS

A review of the literature on residential succession yields several empirical generalizations to be investigated in this study of business succession, and recent research on minority entrepreneurship suggests additional hypotheses. Most of the supporting literature will not be cited here; it is extensively reviewed in Aldrich (1975).

Once the process of succession is begun it is rarely, if ever, reversed; succession follows a continuous line as the established group recedes and the expanding group replaces it. Rates of housing turnover by the originally dominant group are fairly stable, as the normally high rate of residential mobility in urban populations provides ample opportunity for substantial numbers of vacancies to occur in a relatively short time (Molotch 1972). The proportion of minority occupancy rises because the whites who leave are not replaced by other whites rather than because whites begin leaving at a faster rate.

The pent-up demand for housing among minority populations that prevents housing vacancy rates from falling is not matched by a proportionately high demand for business opportunities. First, like white entrepreneurs, blacks and Puerto Ricans face a pattern of metropolitan-wide economic concentration and changes that are detrimental to local businesses (Birch 1970). Second, would-be minority entrepreneurs face increasing competition from businesses outside the area, particularly from shopping centers made accessible by improved intraurban mobility. Third, lack of business experience and discriminatory credit practices make minority businesses extremely vulnerable to short-range misfortune (Cross 1969).

Several historical studies of blacks in business have found that the

<sup>2</sup> Although Puerto Rican small businessmen are identified separately from blacks in this paper, we have not identified succession stages in terms of "percentage Puerto Rican." The proportion of Puerto Rican residents is extremely small in our sample areas and could not be estimated in the demographic information available through the census. Thus when we refer to changes in residential composition, we speak only of blacks, whereas when we refer to changes in business ownership, we speak of blacks and Puerto Ricans. Also, in the later stages of succession there were no Puerto Rican businessmen; in those cases we refer only to blacks.

removal of white-owned competition allows black businessmen to occupy types of businesses previously closed to them (see Drake and Cayton 1945; Light 1972, p. 14). As white businessmen leave the more desirable niches in the business structure, we would expect black and Puerto Rican entrepreneurs to move quickly to fill them.

White-owned small businesses are less free to move than the white residential population because of the lower liquidity of their locational investments. Businesses making a profit are unlikely to move owing to the uncertainties associated with doing business at a new location (Zimmer 1964). Considering the tenuous viability of small businesses in aging urban neighborhoods, we would expect economic factors to weigh more heavily than racial change in small business survival, although we recognize the interdependence of economic and racial change. This expectation is reinforced by evidence indicating that only a small proportion of white residential withdrawal can be accounted for by racial attitudes such as hostility to blacks or fear of them ("white panic") (see Wolf and Lebeaux 1969).

This paper tests five hypotheses based on the review of the literature mentioned above (Aldrich 1975) and on research concerning small businesses:

H<sub>1</sub>: An increase in the proportion of minority-owned small businesses occurs because whites cease buying into business opportunities in racially changing areas rather than because white businessmen accelerate their rate of withdrawal.

H<sub>2</sub>: Whereas the high pent-up demand for housing among minorities produces a low housing vacancy rate during succession, the low pent-up demand for business opportunities among minorities and the declining market they face lead to an increase in the business vacancy rate.

H<sub>3</sub>: The withdrawal of majority-group competition opens up opportunities for minority entrepreneurs and minority businesses eventually occupy the abandoned niches, with the industry distribution for all businesses remaining fairly stable as a result.

H<sub>4</sub>: The changing economic status of a neighborhood and the profit status of a business have a greater impact on small business survival than does racial change.

H<sub>5</sub>: Attitudes of white small businessmen toward race or crime have no impact on the probability of their leaving an area in racial transition from white to black.

#### DESIGN OF STUDY

The study reported below differs from previous studies of ecological succession in at least two significant respects. First, we studied a population of



businesses (or, strictly speaking, a set of business sites) in areas undergoing race residential succession, not a population of area residents. Second, we have four waves of observations on the same set of business sites, spaced at a fairly close interval of two years between observations and thus providing a detailed picture of the *process* of succession (Duncan and Duncan 1957, p. 111).

The data for this study were obtained from interviews and observation of a four-wave panel of business sites. In 1966 a probability sample of businesses was surveyed in eight high crime rate police precincts of Boston, Chicago, and Washington, D.C. Follow-up surveys were made in 1968, 1970, and 1972 in seven of these areas, a predominantly black and a predominantly white precinct in both Boston and Chicago and two predominantly black precincts and a primarily white one in Washington, D.C. The areas in our sample include virtually all of the South Side of Boston, from just below the central business district to the city border, all of Washington, D.C., from S Street to the Maryland border, and much of the North and West Sides of Chicago. Given the large territory covered in each city, we feel our conclusions have high external validity. For a more complete description of the sample, see Aldrich and Reiss (1971) and Aldrich (1973).

The seven precincts retained from the original sample included 648 profit-oriented businesses, varying from 86 to 102 for each precinct. Interviews of the current owner or manager at each 1966 site were sought in the follow-up studies of 1968 and 1970, yielding 432 interviews in 1968 and 431 in 1970. Excluding vacant sites, completion rates were, respectively, 80% and 84%. Information about survival and business type was recorded for every business site in 1968 and 1970, whether or not anyone was interviewed. The procedure was modified slightly in 1972 when, because of cost limitations, a short-form mailed questionnaire was used, followed by on-site interviews for businesses not returning a questionnaire or reporting a change in ownership. Since almost no new business sites were developed in these areas between 1966 and 1972, the original sample should provide reliable estimates of the experience of the local business populations over this period.

To examine the effect of changes in race composition of residential areas on the race composition of small businesses, each sampling area was divided into its component census tracts. To portray aggregate changes concomitant with residential and business succession, we classified each census tract as representing one of six stages of residential succession as of 1960. The definition of stages follows Duncan and Duncan (1957), being based on the proportion of black residents in 1960 and 1970. A brief definition of each stage is given in table 1.

The mean percentage of blacks increases and the standard deviation

TABLE 1  
SELECTED DEMOGRAPHIC CHARACTERISTICS FOR 1960 AND 1970 OF CENSUS TRACTS FOR SIX STAGES OF SUCCESSION AS OF 1960

SUCCESSION STAGE	DEFINITION OF STAGE	1970 % BLACK		DISTANCE OF BUSINESS SITE FROM LINE OF RACE MOVEMENT (FEET)	SITES (N)	MEDIAN INCOME OF FAMILIES AND UNRELATED INDIVIDUALS IN CONSTANT 1959 \$				TOTAL POPULATION	
		$\bar{M}$	SD			1960	1970	1960	1970		
Influx .....	1960 and 1970 population less than 2% black	0.5	0.5	5,103	148*	5,630	6,337	4,890	4,539		
Low minority .....	1960 population less than 2% black, but 1970 population greater than 2% black	34.7	39.9	3,236	48	5,483	5,560	6,178	6,420		
High minority .....	1960 population between 2% and 50% black	60.0	29.1	2,149	186	4,668	4,763	5,323	4,485		
Majority .....	1960 population between 50% and 80% black	89.2	9.7	2,104	109	4,887	5,257	8,897	8,227		
High majority .....	1960 population between 80% and 97.5% black	95.3	5.2	1,505	88	3,758	4,139	4,752	3,783		
Succession .....	1960 population 97.5% or more black	98.2	1.0	895	69	2,825	3,418	4,243	3,869		

\* Of the 148 sites, 21 had no blacks resident in 1960 but are included because they fall within the line of race movement. Note that business sites are classified on the basis of the composition of their census tract, not on that of city or sampling area.

decreases across the stages of succession, except for the influx stage (table 1). This pattern reflects the initial scattering and uneven distribution of blacks in the influx stage with more settlement concentration as residential succession proceeds. Following Pryor's (1971) suggestion, we classified 1966 business sites according to their distance from the "line of race movement" by 1970.<sup>3</sup> The validity of this classification is shown in the systematic decrease in the "distance" measure from influx to succession, even though there is considerable variation within stages.

Income in constant dollars varies by stage of succession, with the median income of families and unrelated individuals decreasing as succession proceeds. By comparing the rate of change of median family income in the sample areas with that in the relevant SMSAs, we can derive a crude estimate of the extent to which the census tracts in our study were affected by the decentralizing forces described by Berry and Cohen (1973). For all three SMSAs, the median family income between 1959 and 1969, in constant dollars, rose 32% as compared with an average rate of increase of only 8% for the census tracts in our sample. Although median family income increased in all stages of succession, only in the final stage, termed "succession," is the increase more than 12%. When this relative loss in median family income is coupled with the declining population of nearly all tracts, the result is lessened economic attractiveness *relative to* other business sites in the metropolitan area.

### BUSINESS POPULATION CHANGES: RACIAL COMPOSITION AND VACANCY RATE

If the process of succession in the business population is similar to the process of race residential succession and if hypotheses 1 and 2 are correct, we should observe three trends in the population of businesses. First, the aggregate rate at which white businesses leave changing areas should be fairly stable over time, with no evidence of "panic selling." Second, the proportion of white-owned businesses in these areas should decrease over time because of a lack of white buyers to replace white businessmen who have left. Third, because of the lack of new white businessmen and the presence of economic barriers to black or Puerto Rican entry into business, many vacated sites will not be occupied by new owners; thus the proportion of vacant sites or of ones occupied by organizations other than businesses should increase over time.

<sup>3</sup> The line of expansion of the black residential population was determined by plotting the distribution of blacks in 1960 and 1970 on street maps of the areas. In Washington, D.C., expansion has been east and west from Georgia Avenue, with some movement northward. In Boston, expansion has been northwest and southeast from Blue Hill Avenue. On Chicago's West Side, expansion has been north and south from Madison Street and Roosevelt Road, and on Chicago's North Side, it has been east and west from Halsted Street and north from Fullerton Avenue.

The turnover rate for white-owned businesses is fairly stable over the four waves of our panel, as shown in table 2. At the aggregate level, the proportion of businesses with the same white owner as in the previous panel is remarkably similar, except for areas of high majority succession between 1970 and 1972. Only in the late stages of race residential succession does the white rate of leaving increase significantly. The pattern of white population withdrawal during residential succession thus holds true of the business population as well, and hypothesis 1 is upheld.

Major change occurs, however, in the market for available business sites in the changing areas. Just as white families do not seek housing in racially changing areas, so apparently most white businessmen no longer view changing neighborhoods as viable business locations. The percentage of new business owners belonging to either the black or the Puerto Rican minority increased between 1968 and 1972 through all stages of succession. Within each wave there is a substantial hiatus between the influx stage, during which blacks are only a very small proportion of the 1970 population, and the other five stages. Of 143 minority-owned or operated firms in 1972, 132 were black- and 11 Puerto Rican-owned or managed, but the Puerto Ricans were located only in the influx and low minority areas. Thus by 1972 the market of new buyers in the high majority through succession stages is composed entirely of blacks.

The fairly stable rate of white owners leaving and the increasing rate at which blacks or Puerto Ricans replace them as buyers of available businesses interact to produce a steady decline in the proportion of white-owned or operated businesses in all areas. Note that although our panel began in 1966, the process of succession had begun earlier in all but the influx areas, having started most recently in the low minority succession areas but before 1950 in the high majority and succession areas. The 1966 cross section reflects these historical differences in succession, showing considerable decrease in the percentage of white-owned businesses through stages of succession.

Even though blacks and, in the early stages, Puerto Ricans begin buying businesses in areas undergoing residential and business succession, their buying is limited and does not absorb the large number of sites vacated. Therefore, as shown in table 3, only a minority of sites vacated by businesses (both white and black or Puerto Rican owned) are reoccupied by new profit-oriented businesses. There was a temporary increase in the proportion of sites reoccupied between 1968 and 1970, but in the 1970-72 period the reoccupation rate fell again to about one-quarter of all available sites in the high minority through succession stages. Only during the influx stage is the reoccupation rate sufficient to absorb half or more of the available sites.

As a consequence of the low reoccupation rate, vacancy rates rise in all

TABLE 2  
CHANGES IN THE RACIAL COMPOSITION OF BUSINESS OWNERS BY STAGE OF SUCCESSION

Succession Stage	WHITES ONLY: BUSINESSES WITH SAME OWNER AS AT PREVIOUS WAVE (%)			NEW OWNERS WHO ARE BLACK OR PUERTO RICAN (%)			OPERATING BUSINESSES WHITE OWNED (%)			
	1968	1970	1972	1968	1970	1972	1966	1968	1970	1972
Influx .....	74	82	82	10 (21)	17 (24)	19 (21)	100	98	96	94
Low minority .....	77	70	76	[1] (4)	57 (7)	71 (7)	92	88	78	75
High minority .....	80	78	79	33 (18)	50 (26)	35 (17)	82	82	76	74
Majority .....	78	66	80	50 (8)	80 (20)	83 (12)	79	80	61	56
High majority .....	69	72	94	71 (7)	85 (13)	100 (7)	62	61	52	57
Succession .....	62	59	62	80 (5)	79 (14)	100 (8)	49	41	31	25
All stages .....	76	75	80	36 (63)	57 (104)	56 (72)	80	80	72	71

NOTE.—Number of cases the percentage is based on is given in parentheses because of the small N.

TABLE 3  
CHANGES IN DEMAND FOR SMALL BUSINESS OWNERSHIP AND IN BUSINESS-SITE VACANCY RATE BY STAGE OF SUCCESSION

SUCCESSION STAGE	WHITES ONLY: BUSINESSES WITH SAME OWNER AS AT PREVIOUS WAVE (%)			ALL OWNERS: AVAILABLE SITES BOUGHT BY PROFIT-ORIENTED BUSINESSES (%)				VACANT OR NONBUSINESS SITES (%)	
	1968	1970	1972	1966-68	1968-70	1970-72		1968	1970
Influx .....	74	82	82	54	60	49		12	12
Low minority .....	77	70	76	40	39	35		12	23
High minority .....	80	78	79	45	44	26		12	18
Majority .....	78	66	80	29	44	27		18	23
High majority .....	69	72	94	23	31	16		26	33
Succession .....	62	59	62	25	45	24		22	25
All stages .....	76	75	80	38	44	29		16	20

NOTE.—Available sites include sites vacant at the previous wave as well as those left between waves.

areas. Hypothesis 2 is thus confirmed. Some former business sites are converted to nonprofit uses, especially by government-sponsored organizations and black community-oriented organizations. By 1972, the proportion of vacant or nonbusiness use sites was two out of five in the high majority and succession stages. The lowest vacancy rate for 1972, 15%, existed in areas experiencing an influx of black residents.

The process of business succession thus resembles quite closely the process of residential succession, as hypothesized. Not "white flight" *per se* but the absence of new white entrepreneurs interested in doing business in racially changing areas accounts for the transition of business ownership in such areas from white to black or, in the early stages of succession, to a minority of Puerto Ricans. Concomitantly, the lack of white buyers and the small number of black and Puerto Rican buyers account for a steady increase in the vacancy or nonbusiness-use rate. The business population appears to adapt to the changed socioeconomic status and composition of the residential population and to a decreased functional importance of central city location by decreasing in absolute size until it is substantially smaller than when race residential succession began.

### BUSINESS POPULATION CHANGES: INDUSTRY COMPOSITION

A business population responds to race residential succession not only by a decrease in absolute size and by changes in the race composition of ownership but also by changing its industry composition. With changes in population composition come changes in consumer and labor markets that have more impact on white-owned retail and service businesses than on such white-owned industries as construction, manufacturing, or wholesale trade (Aldrich 1973). Consequently, white-owned retail and service firms decrease in number, not only absolutely but also relative to other kinds of businesses.

Changes in the population of black- and Puerto Rican-owned businesses during race residential succession are somewhat different from those for white-owned businesses, as the withdrawal of white-owned businesses opens up opportunities heretofore closed to minorities. In the early stages of succession, the population of black- and Puerto Rican-owned businesses increases in absolute size; in the later stages it decreases more slowly than that of white-owned businesses, even though the business failure rate is higher for minority-owned firms than for white-owned ones. Tables 4 and 5 provide information on the changing industry composition of white- and minority-owned businesses that is needed for testing hypothesis 3.

Businesses are classified into seven industry categories<sup>4</sup> in table 4, and

<sup>4</sup> The categories are retail businesses, comprising (1) consumer goods, (2) food sales, (3) restaurants and bars, (4) other retail; service businesses, comprising (5) personal services and (6) business services; (7) other businesses, including construction, manufacturing and wholesale trade.

TABLE 4

CHANGING INDUSTRY DISTRIBUTION OF BUSINESS POPULATION: PERCENTAGE OF BUSINESSES IN SEVEN INDUSTRIES IN 1966 AND 1972  
BY RACE OF OWNER AND STAGE OF SUCCESSION

SUCCESSION STAGE AND YEAR	WHITES OWNED										BLACK OR PUERTO RICAN OWNED									
	Retail					Total					Retail					Service				
	Camr. Goods	Food	Bar	Other		Camr. Goods	Food	Bar	Other	% N	Camr. Goods	Food	Bar	Other		Paul. Bus.	Bus.	Other		% N
Influx:																				
1966 .....	19	14	20	6		13	7		22	101 148	...	...	...	...		...	...	...	...	...
1972 .....	22	12	20	6		13	6		22	101 119	...	(1)	(2)	...		(2)	...	(2)	...	*
Minority:																				
1966 .....	27	14	7	2		18	9		23	100 44	...	(2)	...	...		(1)	...	(1)	...	*
1972 .....	21	8	8	4		21	8		29	99 24	(2)	(4)	...	...		(2)	...	...	...	*
High minority:																				
1966 .....	14	12	13	3		18	6		34	100 153	8	16	16	8		36	4	12		100 25
1972 .....	17	12	18	3		14	6		31	101 102	9	21	9	3		30	21	6		99 33
Majority:																				
1966 .....	20	21	9	5		16	6		23	100 86	13	13	13	9		39	4	9		102 23
1972 .....	16	19	9	0		16	5		35	100 43	26	20	6	12		26	6	3		99 34
High majority:																				
1966 .....	27	20	9	4		14	2		24	100 55	12	12	19	6		41	3	6		99 32
1972 .....	14	14	10	3		14	3		41	99 29	5	36	9	5		41	5	0		101 22
Succession:																				
1966 .....	29	18	6	6		9	12		20	100 34	3	9	14	9		49	6	11		101 35
1972 .....	10	10	30	0		0	10		40	100 10	7	17	10	3		47	10	7		101 30

NOTE.—Actual number of businesses shown in parentheses when the base was too small to give percentage.

\* No percentage given because of small number.



TABLE 5  
CHANGING INDUSTRY COMPOSITION OF BUSINESS POPULATION: INDEX OF DESIMILARITY BETWEEN INDUSTRY DISTRIBUTIONS  
OF ADJACENT WAVES BY RACE OF OWNER AND STAGE OF SUCCESSION

SUCCESSION STAGE	WHITE OWNED:				BLACK OR PUERTO RICAN OWNED:				COMPARISON BETWEEN WHITE- AND BLACK- OR PUERTO RICAN- OWNED INDUSTRY				ALL BUSINESSES:			
	COMPARISON BETWEEN WAVES OF:				COMPARISON BETWEEN WAVES OF:				COMPARISON BETWEEN WAVES OF:				COMPARISON BETWEEN WAVES OF:			
	1966-68 (1)	1968-70 (2)	1970-72 (3)	1966-72 (4)	1966-68 (5)	1968-70 (6)	1970-72 (7)	1966-72 (8)	1966 (9)	1968 (10)	1970 (11)	1972 (12)	1966-68 (13)	1968-70 (14)	1970-72 (15)	1966-72 (16)
Influx .....	3.5	3.5	4.0	3.0	*	*	*	*	...	*	*	*	3.5	5.0	4.5	3.0
Minority .....	6.0	7.5	7.0	12.5	*	*	*	*	*	*	*	*	4.5	6.5	4.0	6.0
High minority .....	6.0	3.0	3.5	7.5	16.5	17.5	9.5	23.5	30.0	39.5	40.0	41.0	5.0	5.5	1.0	9.5
Majority .....	5.5	9.0	6.5	12.0	17.5	33.0	11.0	25.5	31.0	40.0	27.0	34.5	3.5	3.0	5.5	4.0
High majority .....	13.0	12.0	3.5	19.5	11.5	7.5	19.0	25.0	40.5	39.0	49.5	52.0	9.5	4.5	8.5	16.5
Succession .....	21.0	31.0	15.0	44.0	11.5	12.0	9.5	16.0	50.5	50.0	63.0	56.5	12.0	11.0	7.0	14.0

\* Fewer than 10 cases per wave.

indices of dissimilarity for these categories are presented in table 5. The industry composition of white-owned businesses in the influx stage is highly stable across the six years of our panel: the index of dissimilarity is never larger than 4.0 for comparisons between adjacent waves, indicating that all types of firms decline at about the same rate. Industry composition is less stable for all other stages, since the percentage of retail and service businesses decreases more rapidly than that of other kinds. Most of the dissimilarity between waves is due to a decline in the number of two kinds of retail businesses owned by whites: consumer goods and food sales. The industry distribution for white-owned businesses changes, sharply, however, only in the final succession stage, in which the number of consumer-goods businesses drops from 10 in 1966 to one in 1972 and the number of food-sales businesses drops from six to one.

For the black- and Puerto Rican-owned businesses, changes in industry composition between 1966 and 1972 in the first four stages occur because businesses are being *added* in the subpopulations from which whites are withdrawing, consumer goods and retail food sales, so that this business population increases. In the last two stages of succession, however, the absolute number of black-owned businesses also decreases, although the number and relative proportion of consumer-goods firms remain fairly stable and food sales firms increase in absolute numbers. In particular, in 1968 there were no black-owned liquor stores, whereas in 1970 there were six, and in 1972 five were still in business. The more volatile pattern of change in the black- and Puerto Rican-owned business population is reflected in the larger indexes of dissimilarity between waves for their industry distribution, relative to those for whites.<sup>5</sup>

One implication of these changes is that the industry distributions for white- and for black- or Puerto Rican-owned businesses are complementary. Blacks and Puerto Ricans move into the types of businesses vacated by whites and consequently meet with less competition from local white-owned businesses (Light 1972; see also Tilly, Jackson, and Kay 1965, on the process of occupational succession). This process can be shown in two ways by means of the information in columns 9–16 of table 5. First, as shown in columns 9–12, the industry composition of the two business populations diverges between 1966 and 1972 in the four later stages of succession (there

<sup>5</sup> Change in the industry composition of black- and Puerto Rican-owned businesses is due mainly to purchases from whites and to survival of existing businesses, rather than to conversion of newly purchased or existing businesses. Of the four businesses bought by blacks in 1968 and converted to other business usages, there was a net movement of one from the retail to the service category. In 1970 there was a net gain of two retail businesses out of 16 conversions; in 1972 there was a net gain of three out of 14. In terms of the two retail categories in which black and Puerto Rican businesses showed relative growth, there was a net increase of three consumer-goods or food-sales firms in both 1970 and 1972, but none in 1968.

## Race Composition of Neighborhood Businesses

are not enough cases of minority ownership in the first two stages to make this comparison). Second, the indexes of dissimilarity between waves for the *total* industry composition within stages (cols. 13–16), disregarding race of owner, are smaller for 19 of 24 comparisons with indexes for white-owned businesses and for all of the 16 possible comparisons with indexes for black- or Puerto Rican-owned businesses. Thus, changes within the population of white-owned businesses are at least partially compensated for by changes in the population of black- or Puerto Rican-owned businesses, so that the total industry distribution changes more slowly than either of the two separate distributions.

Hypothesis 3, concerning the impact of white businessmen's withdrawal from racially changing areas, is thus supported: The total business population declines with lower incomes and changing demand in the new population, and industry composition changes accordingly. The nonretail and non-service subpopulation of white-owned businesses changes more slowly and declines less rapidly than that of other types. The black- or Puerto Rican-owned business population increases and then declines more slowly than the white, as minority businessmen take advantage of the niches opened to them by white businessmen leaving the area.

### INDIVIDUAL BUSINESS SURVIVAL

Hypotheses 4 and 5 concern the prediction of individual business survival, rather than changes at the population level. Close to 10% of the original 1966 cohort of white-owned businesses left each year, with a total loss of 51% of the white-owned business cohort of 1966 by 1972. In this section we examine the extent to which local economic, racial, and attitude changes account for business failure. The dependent variable is whether white-owned businesses at the sample sites in 1966 were still at those sites in 1972,<sup>6</sup> a 0,1 variable, with 1 indicating that a business is no longer at its site ( $X_1$  in the correlation matrix of table 6).

The literature on race residential succession and organization-environment interaction suggests five sets of variables that might predict the survival of small businesses in areas undergoing race residential succession. First, given the location of such areas in a central city, national economic and demographic trends reduce their chances of survival. The decentralization of economic activity to the suburbs works against central city survival, and the growing scale of business enterprise places further constraints on

<sup>6</sup> Note that the issue, strictly speaking, is survival at the site, as businesses no longer at the 1966 site could have moved out of the area to another site. The difficulties of tracking down businesses that had left their sites prevent us from focusing on the move versus fail issue, but an analysis of the data from the set of businesses whose ultimate fate we were able to determine indicates there is no discernible bias in our use of the survival-at-the-site criterion.

TABLE 6  
CORRELATIONS AMONG FATE OF BUSINESS AND CENSUS-TRACT (CT) AND ORGANIZATIONAL CHARACTERISTICS

	X <sub>1</sub>	X <sub>2</sub>	X <sub>3</sub>	X <sub>4</sub>	X <sub>5</sub>	X <sub>6</sub>	X <sub>7</sub>	X <sub>8</sub>	X <sub>9</sub>	X <sub>10</sub>	X <sub>11</sub>	X <sub>12</sub>	X <sub>13</sub>	X <sub>14</sub>	X <sub>15</sub>	X <sub>16</sub>	$\bar{M}$	SD
X <sub>1</sub> : 1966 business gone by 1972 (1 = yes)	...	.10*	.06	-.11*	-.09*	-.13*	.15*	.10*	.02	.07	.10*	-.07	.07	-.04	-.21*	.06	.52	0.50
X <sub>2</sub> : 1960 percent black (CT)	...	...	.00	-.44*	.05	-.35*	.09*	.05	-.01	.03	.01	.09*	-.07*	-.06	.08*	.44	.31	0.36
X <sub>3</sub> : 1970 residualized percent black (CT)*	...	...	...	.06	-.31*	-.39*	.04	-.08*	.02	-.04	-.03	.00	-.14*	.15*	.02	.15*	.00	0.25
X <sub>4</sub> : 1960 median income (CT)	...	...	...	...	.00	.13*	-.12*	-.03	.10*	-.14*	.05	-.05	.03	.01	.00	-.29*	5.952	1.312
X <sub>5</sub> : 1970 residualized median income (CT)*	...	...	...	...	...	.29*	-.08*	.09*	-.05	.03	-.09*	.04	.04	-.10*	.09	-.14*	.00	1.340
X <sub>6</sub> : distance from line of movement	...	...	...	...	...	...	-.13*	.02	-.02	-.08*	-.03	-.06	.09*	-.05	.00	-.23*	3.028	2.870
X <sub>7</sub> : 1966 desire to move (1 = yes)	...	...	...	...	...	...	...	-.10*	.01	.02	-.13	.04	-.01	.00	-.02	.03	.32	0.47
X <sub>8</sub> : retail business (1 = yes)	...	...	...	...	...	...	...	...	-.55	.11*	.39*	-.13*	.15*	.04	-.01	.14*	.52	0.50
X <sub>9</sub> : service business (1 = yes)	...	...	...	...	...	...	...	...	...	.00	.03	-.06	.00	-.06	-.05	-.05	.22	0.41
X <sub>10</sub> : 1966 age of business	...	...	...	...	...	...	...	...	...	...	.12*	.05	.07	-.03	.05	.03	18.6	15.7
X <sub>11</sub> : 1966 percent customers within mile	...	...	...	...	...	...	...	...	...	...	...	-.14*	.12*	.08*	-.03	.07	.46	0.38
X <sub>12</sub> : 1966 number of employees	...	...	...	...	...	...	...	...	...	...	...	...	-.12*	.09*	.10*	.02	12.8	40.5
X <sub>13</sub> : 1966 percent relatives employed	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...
X <sub>14</sub> : 1966 chain owned (1 = yes)	...	...	...	...	...	...	...	...	...	...	...	...	...	-.13*	.02	.03	.18	0.33
X <sub>15</sub> : 1966 profit (1 = yes)	...	...	...	...	...	...	...	...	...	...	...	...	...	...	.04	.04	.11	0.31
X <sub>16</sub> : 1968 riot target (1 = yes)	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	.03	.86	0.35
X <sub>16</sub> : 1968 riot target (1 = yes)	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	...	.16	0.37

NOTE.—Number of cases varies from 476 to 520 because of missing data.  
\* Measured by residualizing 1970 figure on the corresponding figure for 1960.  
\*  $P < .05$ .

the survival of small business entrepreneurship. Since we do not measure these changes in this study, we can only speculate about their effect on the basis of our failure to account for all of the variance through local forces.

Second, the variable we hypothesize to have a major impact on business survival is the economic status of the incoming population compared with that of the previous residents (hypothesis 4). Our measures of economic status ( $X_4$  and  $X_5$ ) are the median income of all families and unrelated individuals in a census tract in 1960 and changes in economic status between 1960 and 1970, the latter measured by residualizing 1970 income on a tract's value for 1960 (Bohrnstedt 1969, p. 118).

A counterhypothesis to hypothesis 4, the possibility that the changing racial composition of an area explains white businessmen's withdrawal, suggests a third set of variables. Findings from previous investigations are inconsistent concerning the impact of the race of the incoming population. If race per se is an important factor inducing businesses to leave a neighborhood in transition from white to black residents, then both the 1960 percentage of blacks in a census tract ( $X_2$ ) and the change between 1960 and 1970 ( $X_3$ ; again, residualized) should have a positive impact on a business leaving its 1966 site. A business's distance from the line of movement in settlement ( $X_0$ ) should also predict 1972 survival, if race considerations are so paramount in the minds of white businessmen that they leave in anticipation of race change. As we show below in discussing the discriminant analysis (table 7), none of these three race variables is a significant predictor of 1966-72 survival, once economic status and change are taken into account.

Hypothesis 5 suggests that a fourth set of variables may be labeled the "panic syndrome," since they measure the impact of white businessmen's fear of crime and of black in-movement on the probability of their leaving the area. Findings from previous studies of race residential succession predict a low correlation between such fear or panic and business's survival in race residential succession. Two questions from the 1966 interviews were used as indicators of a white businessman's fear of and hostility to blacks: first, whether the businessman believed crime in the area was decreasing, stable, or increasing, and second, his opinion of the civil rights movement and its impact on police work. These two variables are not shown in table 6 because their correlation with business survival ( $X_1$ ) is essentially zero ( $-.01$  and  $.01$ ). The test of the panic hypothesis (hypothesis 5) proved negative, lending further weight to arguments that residential succession is a "normal process" in the central city, a point returned to in our concluding section.

Variables  $X_7$ - $X_{16}$ , constituting a fifth set, measure elements of a business organization's structure and management. Businessmen were asked in 1966 whether they "had thought of moving" ( $X_7$ ); the low correlation between

$X_7$  and  $X_1$  indicates that some carry out their intent. Also,  $X_7$  is positively correlated with the 1960 percentage of blacks and negatively correlated with 1960 median income, income change, and distance from the line of race movement. Earlier it was found that whites left the retail trades in greater proportions than other types of business, and the .10 correlation between  $X_8$  (retail business) and  $X_1$  reinforces this conclusion to some extent. Service businesses ( $X_9$ ) were no more likely to leave than other types, casting doubt on any inference that businesses requiring close contact with customers are more sensitive to race residential succession than other types of business. A more direct indicator of dependence on local customers, the percentage of customers residing within a mile of a business site ( $X_{11}$ ), also has a low correlation with business survival.

Business age ( $X_{10}$ ), proportion of relatives employed ( $X_{13}$ ), number of employees ( $X_{12}$ ), and chain ownership ( $X_{14}$ ) have negligible correlations with the dependent variable. As predicted in hypothesis 4, the most substantial organizational predictor of survival is whether a business made a profit in 1965-66. The correlation between  $X_{15}$  (profit in 1966) and  $X_1$  is  $-.21$ . The final organizational variable examined is whether a business was a riot target in the civil disorders of 1968 ( $X_{16}$ ). Earlier analyses showed that being a riot target had a substantial short-run financial impact on small businesses in riot-torn areas (Aldrich and Reiss 1971; Berk and Aldrich 1972), but the long-run direct effect is negligible in reducing the business population.

Of the 15 variables examined for their impact on whether a business survived to 1972 at its 1966 sample site (table 6), nine were statistically significant at the .10 level and therefore were chosen for a stepwise discriminant function analysis. The first variables entered were  $X_4$  and  $X_5$ —the indicators of census-tract median income and income change—because of our hypothesis that the concomitant of residential succession with the most immediate impact on small businesses is the expansion of a lower socioeconomic group into an area: we did not wish to confound the effect of race change with aggregate income change. The second set of variables to be entered, one at a time, were those characterizing the race impact of succession ( $X_2$ ,  $X_3$ , and  $X_0$ ), but none of them remained: each was dropped because it made no statistically significant contribution to the discriminant function after  $X_4$  and  $X_5$  were entered. The entry of the organizational variables was controlled so that business type entered first: only retail business ( $X_8$ ) remained in the analysis. After all other organizational variables had been entered,  $X_7$  and  $X_{15}$  were entered. This order was chosen because it could be argued that both desire to move and profit are results of other organizational variables, for example, chain ownership.

Only five variables remained from the stepwise analysis for the analyses displayed in table 7. Since discriminant function analysis and regression

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TABLE 7

DISCRIMINANT ANALYSIS OF SURVIVAL STATUS, 1966-72, ON CENSUS-TRACT AND BUSINESS CHARACTERISTICS, WHITE-OWNED BUSINESSES ONLY

Independent Variable	<i>b</i>	$\beta$	<i>F</i> When Entered
1960 census-tract median income .....	-.00003	-.08	5.79
1970 residualized census-tract median income ...	-.00003	-.07	4.36
Retail business (1 = yes) .....	.10	.10	4.80
1966 profit (1 = yes) .....	-.28	-.20	21.96
1966 desire to move (1 = yes) .....	.16	.14	11.30
Constant .....	.845	...	...

NOTE.—Dependent variable = business gone by 1972 (1 = yes, 0 = no);  $R^2 = .09$ ;  $F = 9.94$ ;  $df = 5,505$ ;  $P < .001$ .

analysis are formally equivalent when only two groups are involved (using a 0,1 dummy variable in regression), we present coefficients from a regression analysis of these variables so that the reader may more easily interpret the results. Also presented is the conditional *F*, or the *F*-value at which a variable entered the function. The relative values of *F* and the  $\beta$ -coefficient make possible an assessment of each variable's relative importance.<sup>7</sup>

Two conclusions emerge from examination of the zero-order correlations and the discriminant function analysis. First, the race composition of a business's environment in 1960 and subsequent change between 1960 and 1970 are not important predictors of 1972 survival at the 1966 site, supporting hypothesis 4. Nor is distance from the line of movement a significant predictor. This finding is congruent with research on race residential succession showing that changes in race composition per se are unimportant in accounting for white families leaving areas with in-movement by blacks even though race residential succession has an impact on whites' desires to move *into* a neighborhood undergoing succession. The fact that the economic status of a business's environment in 1960 and subsequent change in it between 1960 and 1970 do have an impact on survival confirms hypothesis 4.

Second, the best predictor of whether a business survived during race residential succession is whether it was profitable in 1966. Two intra-organizational variables, retail trade composition and the owner's ex-

<sup>7</sup> The  $R^2$  is presented chiefly for the sake of completeness, since its use in the case of a 0,1 dependent variable gives only an approximate indication of the explanatory power of the independent variables (it is the complement to the *U*-statistic in discriminant analysis). The discriminant function correctly classified 63% of the observations into the two groups of survivors and nonsurvivors, whereas 50% of the observations could be correctly placed on a chance basis, given the marginal distribution. For a statement on the equivalence of discriminant function analysis and multiple regression in the case of a dummy variable, see Kerlinger and Pedhazur (1973, p. 337).

pressed desire to move, appear to be as important as the economic status of the business environment in predicting survival. The inclusion of other intraorganizational variables such as managerial competence (Mayer and Goldstein 1961) might have increased our ability to account for business survival. Our major interest, however, was not which owners or managers cope best with the difficulties of doing business in the central city, but what characteristics of a business and its local environment predict business survival during race residential succession.

#### SUMMARY AND CONCLUSIONS

Ecological succession in the residential and business populations of the central city is an aspect of more general processes of urban differentiation and change. This paper has investigated whether the process of succession in the small-business population is similar to that of residential succession. Our major finding is that the two processes are quite similar, both being orderly sequences of change through which communities pass as a consequence of population growth and movement.

Some, but not all, studies of race residential succession find that attitudes of residents change as an area undergoes race residential succession. We found that white businessmen's perceptions of their neighborhoods are affected in some ways by such changes, since white businessmen perceive general deterioration of the neighborhood and rising crime rates in all stages of succession.<sup>8</sup> Yet we also found that such fears had no discernible impact on whether a business left the area between 1966 and 1972. In this respect our findings are congruent with those of earlier investigations of race residential succession which show that attitudes toward blacks are a poor predictor of which white residents will move.

The business population generally changes in the same manner as the residential population, with businesses leaving at a fairly low but constant rate and with no evidence of panic selling. The proportion of white-owned businesses decreases not because whites *leave* but because when they do, whites do not *enter* these areas as new business owners. In areas undergoing race residential succession, the market for newly vacant housing units *and* small businesses is almost entirely black.

Whereas the housing vacancy rate does not increase and in fact often decreases during race residential succession, the same is not true of small businesses. There may well be a pent-up demand for small-business opportunities among blacks, but the probability of realizing such aspirations is much less likely for business than for housing units. Minorities lack

<sup>8</sup> An analysis of the impact of changing racial composition on businessmen's attitudes is not reported here because of space limitation, but detailed tables are available from the authors.



both the capital requirements and the business experience to fill business opportunities opened to them by the withdrawal of whites. The black- and Puerto Rican-owned business population does increase slightly in the early stages of succession, but not enough to absorb all the businesses left by whites.

The rate of departure of white-owned businesses is fairly constant across types of industry in the early stages of succession, with retail and service businesses only slightly more likely to leave; in later stages, however, the loss of retail and service businesses is substantial and disproportionate, compared with other kinds of business. Not surprisingly, much of the gain in the black- and Puerto Rican-owned business population occurs in precisely these industries, as minority entrepreneurs take advantage of newly vacated niches. The industry distributions for white- and nonwhite-owned businesses are thus complementary as succession proceeds.

The white-owned businesses sampled in 1966 which were no longer at the sample sites in 1972 probably left because of conditions associated with operating a small business and metropolitan area changes more than because of neighborhood changes. The income distribution of neighborhood residents and changes in it as well as profit status explain some departures, but many simply reflect the high risk of small-business enterprise. The rate of leaving, whether to move or to go out of business, is a little over one in 10 every year. During periods of economic growth, such a rate would go unnoticed in most urban areas, since most business sites do not remain vacant but are soon occupied by aspiring entrepreneurs hoping to succeed where others have failed.

The future of the small-business population in areas undergoing race residential succession can be predicted if the trends we have uncovered continue. First, the business population will continue to decline in absolute size as whites leave and the replacement rate remains low. For central city residents, disadvantages follow in both the labor and consumer goods markets (Aldrich 1973). The loss of jobs in the area causes local residents to seek employment elsewhere. The loss of retail and service businesses means longer shopping trips for consumers, especially if they wish to shop in the high-volume, low-markup businesses in large shopping centers on the fringes of the city.

Second, the departure of white entrepreneurs opens new business opportunities for blacks and other cultural minorities such as Puerto Ricans. Government promotion of minority entrepreneurship has encouraged some to go into business for themselves. Two barriers to minority-business success remain, however, even with an absolute loss of competitive businesses. Credit remains as difficult as ever to obtain—about 90% of the new minority businessmen at every wave of our panel relied solely on personal savings or funds from friends and relatives to capitalize their businesses.

The other barrier is, of course, the continuing economic differentiation of urban areas which has turned central city neighborhoods into high-risk business environments. Whether minority businessmen can succeed in such environments is an open question; turnover rates due to business failure should be predictably high.

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# On the Concept of Face<sup>1</sup>

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The concept of face is clarified and distinguished from other closely related constructs: authority, standards of behavior, personality, status, dignity, honor, and prestige. The claim to face may rest on the basis of status, whether ascribed or achieved, and on personal or nonpersonal factors; it may also vary according to the group with which a person is interacting. Basic differences are found between the processes involved in gaining versus losing face. While it is not a necessity for one to strive to gain face, losing face is a serious matter which will, in varying degrees, affect one's ability to function effectively in society. Face is lost when the individual, either through his action or that of people closely related to him, fails to meet essential requirements placed upon him by virtue of the social position he occupies. In contrast to the ideology of individualism, the question of face frequently arises beyond the realm of individual responsibility and subjective volition. Reciprocity is inherent in face behavior, wherein a mutually restrictive, even coercive, power is exerted upon each member of the social network. It is argued that face behavior is universal and that face should be utilized as a construct of central importance in the social sciences.

The renowned Chinese writer Lu Hsün once wrote, "But what is this thing called face? It is very well if you don't stop to think, but the more you think the more confused you grow" (1934, p. 129). Although everyone appears to have some notion of what face entails, a precise definition of it proves to be a most difficult task. Lin Yü-tang felt that face was "impossible to define" (1935, p. 202); he said of face that, "abstract and intangible, it is yet the most delicate standard by which Chinese social intercourse is regulated" (p. 200).

The concept of face is, of course, Chinese in origin, and the term is a literal translation of the Chinese *lien* and *mien-tzu* (cf. *The Shorter Oxford English Dictionary* [1944]; *Webster's Dictionary* [1958]). Hu (1944) makes an important distinction between two Chinese concepts of face, *lien* and *mien-tzu*, based on two distinct sets of criteria for judging conduct. *Mien-tzu* "stands for the kind of prestige that is emphasized in this country [America]: a reputation achieved through getting on in life, through success and ostentation" (p. 45). *Lien*, on the other hand, "represents the confidence of society in the integrity of ego's moral character,

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the loss of which makes it impossible for him to function properly within the community. *Lien* is both a social sanction for enforcing moral standards and an internalized sanction" (p. 45). It should be pointed out, however, that the concept of *mien-tzu* is not altogether devoid of moral content. Furthermore, the meanings of *lien* and *mien-tzu* vary according to verbal context and in addition are not completely differentiated from each other in that the terms are interchangeable in some contexts. Consequently, although the distinction between the two sets of criteria for judging face—based on judgments of character and, broadly, of the amoral aspects of social performance—is justified, it cannot be anchored to a linguistic distinction between the two terms, *lien* and *mien-tzu*, as proposed by Hu. However, we may continue to use these terms in the senses that Hu has defined.

The concept of face has not, to date, gained general acceptance as a technical term in the social sciences. It is not, for example, listed in *A Dictionary of the Social Sciences* edited by Gould and Kolb (1964) or in the *Dictionary of Behavioral Science* (Wolman 1973). Goffman (1955, p. 213) defines face as "the positive social value a person effectively claims for himself by the line others assume he has taken during a particular contact. Face is an image of self delineated in terms of approved social attributes." Thus, he seems to treat face as situationally defined, meant to refer only to the immediate respect a person expects others to show in each specific instance of social encounter. He interprets what he calls "face-work" as a subtle style of interpersonal encounter, found in all societies, calculated to avoid personal embarrassment, or loss of poise, and to maintain for others an impression of self-respect (Goffman 1955; 1956; 1959). Stover (1962), however, finds the common interpretation of face as "other-directed self-esteem" to be insufficient for his theory of Chinese social interaction. According to this theory, given the hierarchical structure of Chinese society with its built-in permanency of statuses, social exchange occurs essentially between unequals. Face is "the [Chinese] social ideology which legitimizes status rectitude" (p. 375). More recently, Agassi and Jarvie (1969, p. 151), like Lin Yü-tang, treat face as a "standard of behavior." However, without regard for consistency or precision, these authors also refer to face as "status plus something else, like dignity" (p. 139) (see Ho [1972] for a criticism of their study).

Other writers, taking the meaning of face for granted, have failed to clarify their use of the term. Thus, one finds in the literature that face has been variously confounded with an assortment of sociological as well as nonsociological concepts, such as status, prestige, dignity, honor, and the like. Since a scientific term is rendered superfluous if it can be shown that other terms suffice to describe, explain, or account for the phenomenon in question, in the interests of parsimony "face" should not be added to our

already overgrown body of scientific terms if the concept is reducible to constructs already available in the repertory of the social sciences. If one substitutes other terms, such as "prestige," for "face," however, one is invariably left with a dissatisfaction that some essential component of the concept of face has been left out. It is this "something else," to which Agassi and Jarvie referred, that needs to be delineated. The distinction between face and other constructs, which I want to make in this paper, would justify the incorporation of the term "face" into the scientific vocabulary.

Another major issue regarding the scientific status of the concept of face concerns its range of applicability. It is natural to raise the following question: is face behavior distinctive of the Chinese (and other Oriental peoples which have been heavily under Chinese influence) or is it no less significant in other societies? In other words, to what extent is the concept of face useful in the analysis of social behavior outside of the context of Chinese culture? An adequate answer to this question requires that we turn first to a semantic clarification of what the concept of face entails.

#### AN ANALYSIS OF FACE

##### Quantitative versus Qualitative Aspects of *Mien-tzu*

A man of high social standing is said to be *yu mien-tzu* (having *mien-tzu*), while *mei yu mien-tzu* (not having *mien-tzu*) may be used to express having a humble status, or it may be taken to mean that *mien-tzu* has been lost. How much *mien-tzu* a person has is, in general, a function of his social status. But the quantity of a person's *mien-tzu* usually varies according to the group with which he is interacting. Thus, a leader in the Chinese *Triad* society has *mien-tzu* within the underground, but such *mien-tzu* should not be given recognition, at least under proper circumstances, by law-enforcement agents; a military officer should have *mien-tzu* before the men he leads, but may have little *mien-tzu* in the company of a group of intellectuals; again, an academic may have *mien-tzu* among his colleagues, but this *mien-tzu* may not be of much utility in the business community. How much *mien-tzu* a person has, therefore, is not fixed in amount but varies largely according to the social situation in which he is interacting. In losing or gaining *mien-tzu*, too, the group making the judgment must be specified: the loss or gain will vary according to the audience. Only national heroes, scholars who have made lasting contributions to human knowledge, poets of time-honored repute, and the like, come close to having nation-wide *mien-tzu*, unrestricted to specific groups.

*Mien-tzu* can be characterized, furthermore, not only in quantitative terms but also in qualitative terms. For the claim to *mien-tzu* may rest on a variety of grounds. The status on which *mien-tzu* is based may be as-

cribed or else achieved through competition and individual effort. In the case of achieved status, it is possible to further differentiate *mien-tzu* which rests on the personal qualities underlying achievement from *mien-tzu* which derives more directly from the nonpersonal factors, such as wealth, social connections, and authority, obtained through personal effort. Only nonpersonal factors, of course, are relevant to ascribed status.

It should be pointed out that the remarks pertaining to *mien-tzu* above do not apply to the case of *lien*. For *lien* is something to which everyone is entitled by virtue of his membership in society and can be lost only through unacceptable conduct. As Hu stated, "All persons growing up in any community have the same claim to *lien*, an honest, decent 'face'" (1944, p. 62).

### Losing Face versus Gaining Face

Both losing face and gaining face refer, of course, to important changes in the status of one's face; but these changes are not to be construed as simple quantitative ones and, furthermore, must be specified as being relative to the level of what is expected, that is, to the amount to which one is originally entitled. In other words, face is lost or gained only when the changes constitute a departure from the quality or quantity of the individual's claim.

Previous writers on face have treated losing face and gaining face simply as if they were opposite outcomes in a social encounter and have thus failed to notice the basic difference between two social processes that are involved. In the first instance, while it is meaningful to speak of both losing and gaining *mien-tzu*, it is meaningful to speak only of losing *lien*. One does not speak of gaining *lien* because, regardless of one's station in life, one is expected to behave in accordance with the precepts of the culture; correctly conceptualized, exemplary conduct adds not to one's *lien*, but to one's *mien-tzu*.

In the second instance, assessments of social performance leading to losing or gaining face are not made according to criteria which fall on the same unidimensional continuum. It is simply not sufficiently accurate to say that face is lost through unsuccessful social performance and gained through successful performance. Face can, of course, be gained in recognition of what one "deserves," through a diversity of routes such as exemplary behavior, superior performance in some role (as in demonstrating one's competence, trustworthiness, or superior knowledge—particularly when done in modesty), or enhancement of status (as through ostentation or formal promotion to higher office). In all of these, social performance goes above and beyond duty, expectations, or requirements. On the other hand,

face is not necessarily lost as a result of unsuccessful or inferior performance.

Strictly speaking, the opposite of gaining face is a process of erosion, distinguishable from what is called "loss" of face. Decrements in face remain dormant, and social intercourse continues as if the individual were still in possession of his face, until a single incident arises in which his face is put to the test and he fails to protect or "save" it. A physical analogy might help to make this clearer: adding weights to a ship will lower its level at sea, but it will not sink until the overloading goes beyond a critical point—or until the ship meets with a storm beyond its capacity to withstand. "Losing face" is an expression which, properly used, refers only to *public, discrete* events, as do the Chinese expressions *tiu mien-tzu* (losing *mien-tzu*) and *tiu lien*.

A decrease in the amount of face due the individual need not mean losing face. As a common example, we may cite the case of someone whose status is lowered on account of circumstances beyond his control and in which no personal disgrace is implied. A man who turns from rich to poor due to some misfortune will have less claim to *mien-tzu*, but he does not *lose face* in the process. Face may be lost when conduct or performance falls below the minimum level considered acceptable or when certain vital or essential requirements, as functions of one's social position, are not satisfactorily met. To investigate the conditions under which face is lost, therefore, one must delineate which expectations placed upon the individual are regarded as the vital or essential requirements, that is, as the prerequisites or imperatives. The individual has no choice but to satisfy these requirements. For failing to do so would reveal basic incongruities between the individual's social inaptitude or unworthiness and the social recognition he claims to deserve.

Thus, face is not lost merely on account of a failure to gain it, but face must be protected from being lost precisely because of the demoralizing repercussions which otherwise follow. At stake is nothing less than the effective maintenance of one's standing in society. And face which has been lost may be regained, through compensation, corrective actions, making up for one's shortcomings, etc.; but regaining face does not constitute gaining face—it is merely a restoration to the individual of what ought to have been due him in the first place.

It is profitable, in this connection, to relate face to the psychological concept of defensiveness. Face behavior takes on a defensive quality when the individual appears to be excessively concerned with protecting his face—relative to the objective requirements of the situation *in his cultural context*. This is more likely to occur when at some level he senses danger signals (which may be exaggerated) that his face is being threat-

ened and that he does not have the resources to protect its integrity. In any event, the more defensive the individual is, the more awkward and ineffective he is likely to be in his face-protection maneuvers. That the concept of defensiveness is not directly relevant to the dynamics involved in gaining face is another illustration of the fact that losing face and gaining face are not simply opposites of each other.

Now it seems to me that the social dynamics involved in losing face are more deserving of our attention than those involved in gaining face—in terms of both the knowledge that can be derived with respect to social demands and prerequisites and the seriousness of the implications for the one whose face is under judgment. Not everyone is eager or needs to gain face; but everyone who cares for maintaining a minimum level of effective social functioning must see to it that his face is protected from being lost. Whether face is gained or not does not in itself bring into question one's social aptitude, decency, or adequacy, but whether face is lost or not does.

While losing face is, as has been stated, a discrete event, it need not be an all-or-none phenomenon. There are gradations in the severity of losing face, gauged according to the consequences. It is the extent to which a particular person's social functioning is adversely affected that constitutes the true measure of what losing face means to him. The notion of social adequacy is of particular relevance here. An individual's face, and hence his social adequacy, is maintained, relative to his social position, to the extent that he is able to satisfy the minimum requirements society has placed on him; his social adequacy is not maintained, or at least it is questioned, to the extent that he has lost face as a result of his inability to measure up to expectations in his social performance. Since different facets of one's social functioning may be affected, the extensiveness of losing face can be construed to lie on a continuum ranging from adverse consequences affecting only a circumscribed area of social life to the total question of one's fitness as an acceptable member of society. Failures to maintain one's social status, to function adequately in a given role, or to safeguard integrity of character in one's general conduct will all make the loss of face a likely possibility, but to varying degrees of seriousness. Thus, losing face must be understood with respect to the facet of social life affected—which in turn must be tied to the different sets of expectations placed upon the individual—if we are to have an idea of how serious the implications are to the person concerned (see Ho [1974] for a further discussion of this issue).

Another way of viewing the severity of losing face is to consider the temporary-permanent or reversible-irreversible dimension. In most instances, face lost can be regained. However, the loss of face may well be permanent in cases where the misconduct is serious and in direct contradiction to role-imperatives and/or taboos. Examples of such cases are a



captain found guilty of cowardly abandoning his ship and crew to save his own life, a priest caught in adultery, or a family disgraced by incestuous relationships. Not infrequently, the loss of face has such serious consequences that it leads to suicide. Committing suicide may be a final attempt to prevent a total loss of face; or perhaps even to reclaim some measure of it. Thus, in traditional Chinese society women often had to commit suicide to demonstrate their innocence, should the misfortune of being raped (or being widely suspected of being raped) befall them. Outside of the Chinese context, we may also enumerate examples of suicide intended to prevent a total loss of face, as in the case of a commander-in-chief beaten in battle who shoots himself to avoid the humiliation of being captured alive. (These examples illustrate how a person's face is often at the mercy of circumstances beyond his control—see the discussion on the relationship between face and individual responsibility below.) Indeed, face can be more important than life itself. As Goffman (1955, p. 219) puts it, one's face is "a sacred thing."

### The Reciprocity of Face and Social Control

Since social expectations are reciprocal in nature, potential conflicts arise when there is a discrepancy between what a person expects or claims from others and what others extend to him. The possibility of losing face can arise not only from the individual's failure to meet his obligations but also from the failure of others to act in accordance with his expectations of them—that is, not only from the individual's own actions, but also from how he is treated by others. Moreover, the individual's face may be threatened by actions which are not aimed directly at him: a lack of deference shown by others to his friends, relatives, or subordinates, for instance, could also be interpreted as depriving him of face. (The individual would not, however, interpret a lack of deference shown toward his superior as a direct challenge to his own face, although he too may lose face if his superior does.)

A man who has *mien-tzu* is in a position to exercise considerable influence, even control, over others in both direct and indirect ways. At the same time, however, he is under strong constraint to act in a manner consistent with the requirements for maintaining his *mien-tzu* as well as for reciprocating a due regard for the *mien-tzu* of others. Thus, the concern for face exerts a mutually restrictive, even coercive, power upon each member of the social network. Much of the time the individual's actions, far from being directed by his own wishes, are in effect dictated by the necessity of meeting the expectations of others.

A clarification should be made with respect to face and its relationship to social control. Control over others on the basis of *mien-tzu* is different

from that based on authority. First, a person occupying a position of authority certainly has *mien-tzu*, but a person who has *mien-tzu* need not have authority, at least not formal authority. Control based on authority is legitimate control, whereas that based on *mien-tzu*, even when derived from authority, often extends beyond the realm of legitimacy. For instance, insofar as an employee is performing his duties as specified by his job, he is obligated to follow the instructions of his employer; but it may well be out of a consideration for the *mien-tzu* of his employer that he complies with a request which goes beyond the formal requirements of duty, for example, accompanying his employer to social functions, or doing things of a personal nature for him. Second, whereas face is attached to persons, authority, or institutionalized power, as Bierstedt has argued (1950), is always attached to statuses, not to persons. Third, authority relationships may be either unilateral or bilateral, depending on the amount of power that one party may exert on the other. Authority is unidirectional, and it is possible to specify clearly which individuals in a social domain are under one's control. By contrast, social control based on face is invariably reciprocal in nature.

#### WHAT FACE IS NOT

We are now in a position to make a more precise distinction between face and other concepts and to say what face is not—an analytic exercise which should help to eliminate much of the muddled thinking found in the literature on the subject.

#### Face Is Not a Standard of Behavior

Face is not a standard of behavior; rather, a more accurate formulation is that judgments concerning the extent, loss, or gain of face are based on sets of criteria or standards which vary both cross-culturally and over time within a single culture. These standards are rooted ultimately in the value orientations of a given culture at a particular point in time. Changes in the criteria for judging face are both a manifestation and a cause of social change; they are particularly pronounced when rapid social changes are taking place. The kinds of behavior judged to be face losing serve as an especially sensitive indicator of the prevailing mores and morality of the times. Thus, a society in which, as a Chinese saying puts it, "The poor are laughed at, but not the prostitutes," is one whose moral fibers have obviously decayed.

The study of face, therefore, gives us insight into not only the nuances of social interaction but also the kinds of values that are upheld in a given society, particularly those values which are deemed to be prerequisites for

all its adult members. Two types of comparative studies of face are meaningful: (a) charting the changes in the criteria for judging face through time within a given culture (or subculture), and hence the corresponding changes in values orientations, and (b) highlighting the prevailing modes in which people maintain, lose, and gain face in different cultures, to reveal the underlying cross-cultural differences in social relationships and values.

### Face Is Not a Personality Variable

Questions of face arise, not in the private process of self-evaluation, but in social encounters where the evaluations by others of oneself are perceived to be of significance to the maintenance of one's standing in society. Face is not a personality variable: it is not invariant with respect to all situations. That is, face is not an attribute located within the individual; instead, it is what others have recognized and extended to him. Of course, face may be enhanced or lost, as I have indicated, on account of one's personal qualities—but only when these qualities are manifest, given notice by society, and where there is a consensus in judgments of them.

That face is not a personality variable has been recognized by both Goffman (1955) and Stover (1962). Goffman states that "the person's face clearly is something that is not lodged in or on his body, but rather something that is diffusely located in the flow of events in the encounter and becomes manifest only when these events are read and interpreted for the appraisals expressed in them" (p. 214). As expressed by Stover, "One's face is somehow really one's *location* in a social system, not a personal attribute" (p. 367).

Face, *ming-yü* (reputation),<sup>2</sup> and prestige all differ from personality in that they comprise not the individual's characteristics themselves but the consensus of the collection of judgments passed upon him by others. A concern with face, *ming-yü*, or prestige is indicative of other-directedness, that is, having a sensitivity to how one appears in the eyes of others and a tendency to act in ways which meet their approval.

The theoretical distinction made between face and personality has important methodological implications. Face is not to be regarded as a con-

<sup>2</sup> *Ming-yü* refers not to the personal qualities of the individual but to the sum total of judgments passed on these qualities by others on the basis of his overall conduct in life. *Ming-yü* differs from prestige in that it focuses on the evaluative dimension of good-versus-bad rather than on the amount of respect and confidence one commands. Thus, one speaks of a person as having a good or bad *ming-yü*. A person with little prestige may still have a sound *ming-yü*, but it is unlikely that a person with an overall bad *ming-yü* can have much prestige. Another difference between the two concepts is that, while prestige may be earned or ascribed, a sound *ming-yü* must be earned.

struct pertaining to personal qualities inferred from behavior. Accordingly, any measurement of face should not be based upon a direct study of the individual himself; rather, a person's face should be ascertained by conducting enquiries on the opinions that others have of him. Stated simply, a person's face is assessed in terms of what others think of him; the assessment does not include what a person thinks of himself, but may include what he thinks others think of him. Of course, misjudgments concerning one's own face can be made; that is, one's subjective assessment of one's face may be at variance with that of the group. Such misjudgments will act as a source of strain in one's interpersonal relationships.

In viewing face as being external to the individual and hence not as a personality variable, I have treated face in this paper as a sociological, rather than psychological, construct. The psychological sequel to a loss of face is the experience of shame.<sup>3</sup> But one must be careful not to conclude that the reaction of shame is inevitable. Whether or not shame is experienced depends on the subjective definition of the situation by the individual concerned. Among other things, the degree of ego involvement is a major factor in the definition. It is possible that the individual may play the game of face with emotional detachment, particularly when it involves an area of social functioning having to do with such things as the ostensible display of one's standing in society rather than with what would reflect upon him as a person. In any case, losing *lien* is more likely than losing *mien-tzu* to be accompanied by the reaction of shame. The relationship between losing face and shame can be further clarified. One does not

<sup>3</sup> My own interpretation of shame and guilt is that they differ from each other in the following respects: (a) Guilt is internalized (i.e., may be felt even in the absence of an audience or when a person's wrongful actions are unknown to others), but shame may or may not be internalized. (b) The judgmental-evaluative dimension underlying guilt is moral. Guilt is experienced when a person "senses" that his thoughts and/or deeds are morally unacceptable to his internalized societal values. The possible conditions leading to shame, however, are much broader: one can feel ashamed not only of his thoughts and actions but also of his body (e.g., its lack of attractiveness), incompetence (despite having tried one's best), humble condition in life, heritage, country, etc. (c) Personal responsibility is always involved in guilt but not necessarily in shame, as the examples above illustrate—that one can be ashamed of nonpersonal entities or conditions over which one has no control. (d) The psychological correlate of guilt is the fear of punishment or retaliation by an adversary more powerful than oneself. In the case of shame, it is the fear of being ridiculed or "laughed at," consequential to the exposure, real or imaginary, of one's weaknesses, failures, "evils," etc., in front of others: it is like a person left in a psychological state of nakedness, after having been stripped of his persona or external protection. (e) The effects of shame are more pervasive and incapacitating (even devastating and paralyzing) than those of guilt. It is easier to make up for guilt and pacify the conscience through confession, atonement, work, self-denial, etc.; shame often persists like a psychic scar, the prescription for the healing of which is less certain.

In view of these differences, there are sound theoretical reasons for linking the loss of face to shame rather than to guilt. For a further discussion of shame and losing face, see Eberhard (1967) and Ho (1974).

lose (or gain) face alone but always before some group of people; however, the feeling of shame can be experienced privately even without the presence of an audience—that is, the sense of shame can be internalized (see Eberhard [1967], especially p. 124, where shame is defined as “the reverse side of honor”). One might conceive of internalized shame as “losing face before oneself.”

### Face Is Not Status, Dignity, or Honor

Status relationships possess properties of their own which can be characterized independently of the individuals occupying the respective statuses. A status, relative to other statuses, defines the location of the person within the social system, regardless of what his individual personality may be. It functions as a major determinant of how much *mien-tzu* a person has. Nevertheless, face is not directly attached to statuses, but to persons occupying them.

*Webster's Dictionary* defines face as “the outward appearance or semblance of dignity or prestige” (1958, p. 906). The connotative meaning of dignity differs from that of face in that the inner qualities of the individual are emphasized. Dignity is what one strives to attain through leading a way of life superior in quality to that of merely keeping face. *Lien* is, however, more basic than dignity in the sense that having *lien* is a prerequisite for achieving dignity.

Face is a more inclusive concept than honor as well as dignity. Honor may be viewed as a special kind of face that is claimed by certain elitist groups in society. Membership in these groups entitles the individual to special privileges and honor and yet, at the same time, obligates him to observe a set of well-prescribed stringent requirements (i.e., a code of honor) that go far above and beyond those for the masses. For the Confucian scholar-officials of traditional China, the knights of medieval England, and the samurai of Tokugawa Japan, honor was, at least according to ideology, more important than life.

Lin Yü-tang (1935, p. 200) asserts that “To confuse face with Western ‘honor’ is to make a grievous error.” He explains the difference between face and honor by means of specific examples: “In the West, the man who is slapped on the cheek and does not offer a challenge for a duel is losing ‘honor’ but not losing face. On the other hand, the ugly son of a *taot'at*, who goes to a sing-song girl's house, is insulted and returns with a company of police to order the arrest of the sing-song girl and the closing of the house, is getting ‘face,’ but we would hardly say he is guarding his ‘honor’ ” (p. 201). The quotation above illustrates simply that different standards of behavior are involved in judgments of face versus Western “honor,” made in different sociocultural contexts (though one could seriously question

Lin's interpretation that a man who is slapped and does not respond with a challenge "is losing 'honor' but not losing face").

### Face Is Not Prestige

Among the sociological constructs, prestige appears to come closest in meaning to face. Prestige may of course be earned or ascribed. However, for the sake of the following analytic exercise, I shall focus only on the distinction between face and earned personal prestige. Personal prestige may be defined as the amount of respect and confidence invested in the individual by the group; if earned, it is through having a commonly acknowledged performance record of demonstrated competence, expertise, trustworthiness, integrity of character, and other valued personal qualities. Defined in this way, prestige is more closely associated with the individual rather than directly with his rank or office.

In making a distinction between authority and prestige, Simmel wrote (1950, p. 184), "prestige leadership stems from pure personality, even as authority stems from the objectivity of norms and forces." In a similar vein, Bierstedt argued that prestige should not be identified with power. He wrote (1950, p. 731), "knowledge, skill, competence, ability, and eminence . . . are all components of, sources of, or synonyms of prestige, but they may be quite unaccompanied by power." It can also be said that, whereas prestige is essentially rooted in public judgments of the personal qualities of the individual as they are manifest in his social performance, status judgments can be made on the basis of some objective criteria which differentiate individuals hierarchically within the social order quite independently of their personal qualities. (Status judgments, however, can be made subjectively and are thus dependent upon the value hierarchy held by the person making them as well as upon his own assessment of the one being judged [cf. Goldhamer and Shils 1939, pp. 171-82]. In this case, the criteria for judgments of prestige and status need not be dissimilar.) As mentioned by Linton (1936, pp. 113-19), among the irreducible bases for the determination of status are age, sex, and occupation. The first two of these are determinants of ascribed statuses, and as such they are obviously not directly relevant to judgments of personal prestige. The last, occupation, may be included among the determinants of prestige; but even here, how the person obtained his job (whether through ascription or achievement, and whether through *jen shih kuang-hsi* [personal relations] or on the basis of qualification) and especially the record of performance can be of greater significance to the determination of prestige than the nature of the occupation itself.

What I want to argue is that the correlation between changes in the amount of face and that of personal prestige is not a perfect one. The

bases for status judgments, and hence of face, are more inclusive than those for judgments of prestige. Whereas judgments of earned prestige (and of *ming-yü* as well) involve only personal factors, judgments of face include nonpersonal as well as personal factors, as has already been pointed out. Face is thus more encompassing than *ming-yü* or prestige.

Accordingly, it is altogether possible that a person may have face but not personal prestige or vice versa. Like those of face, prestige judgments depend largely on the audience involved. An official of high rank certainly has face, but not necessarily prestige; a poor scholar may have prestige, but not much face; the son of a prominent man has face, but, unless he has demonstrated himself to be worthy in some respect, he may have little or no prestige; but the man of social prominence, who has also earned the respect of the community, has both face and prestige. In sum, it may be said that prestige is one of the sources of the claim to face, but the converse of this proposition cannot be made unequivocally without doing injustice to the distinctiveness of the two concepts as delineated above.

Bearing in mind the distinction between the dynamics involved in gaining versus losing face, I shall keep the analysis of their respective relationships with prestige separate. The interaction of "losing versus gaining" and "face versus prestige" results logically in a matrix of four specific relationships which I shall discuss one by one. It would appear, though, that as compared with the relationship between losing face and losing prestige, that between gaining face and gaining prestige involves social processes which are less dissimilar.

1. When prestige is gained, face is gained—though not necessarily proportionately.

2. When face is gained, prestige is not necessarily gained—as when the face is gained through an ostentatious display of wealth.

3. When prestige is lost, there will result a corresponding (though, again, not necessarily proportionate) decrease in the amount of face, or at least an erosion of the basis for one's claim to face. But one must be careful not to conclude that face is necessarily *lost* concomitantly with a loss of prestige. We are reminded of the conditions for losing face which I have mentioned earlier, namely, when performance is judged to have fallen below the minimum acceptable level or when certain essential expectations placed upon the individual are not met. Herein lies the secret of how the mechanisms differ in the loss of face versus the loss of prestige. Prestige may be lost *gradually*; the process is continuous, and the effects are cumulative. The entire past record of performance is taken into account, however imperfectly, in prestige judgments.

4. When face is lost, prestige is lost, but the effect is most serious when face is lost on account of manifest flaws in personal qualities. Also, one can certainly think of events in which face is affected without a correspond-

ing effect on prestige, in particular, events for which the person concerned can be hardly held responsible. An unfortunate loss in family fortune, for instance, will result in a decrease (not a loss) of the amount of one's face in the community—but one does not lose prestige in the process.

Two more distinctions deserve emphasis. First, the claim to prestige is clearly not an inalienable right, but everyone, regardless of his station in life, is entitled to the claim of *lien*—unless it is forfeited on account of his own unacceptable conduct. One can live quite comfortably without prestige, but can hardly do so without *lien*; to strive to preserve one's face is a prerogative.

Second, the individual alone is held accountable for his conduct and hence how much prestige he deserves; but a person's face can be lost or gained as a result of the behavior of someone else (particularly someone with whom he is closely related). Furthermore, one's actions can affect someone else's face—even that of one's ancestors. For instance, among the Chinese, "My face is totally lost because of you" is an accusation that the offending party's action has resulted in an injury to one's face. In particular, the individual's face and the good name of his family (his *chia sheng*) were virtually inseparable in traditional Chinese society.

It might be objected that the distinction I have just made is not intrinsic to the two concepts of prestige and face, and that the basis for this distinction rests on an inconsistency—in that I have applied the Western individualistic model to prestige judgments, and the Chinese social-behavior model of mutual dependence to face judgments. Surely it can be argued that, given the high degree of mutual dependence in Chinese interpersonal relations, not being held solely accountable for the fate of one's own face is to be expected. Moreover, the behavior of closely related persons is included in the evaluation of one's prestige. Thus, the prestige of a scholar-official suffers as a result of his son's misconduct.

The objection raised above is a valid one, and it may be said that the distinction between prestige and face (with respect to the differing weights of the effects of someone else's behavior on judgments of them) is only a relative one. However, it should be pointed out that in Chinese society prestige is affected by the behavior of others only when there is a tacit implication that one is not to be absolved of responsibility for this behavior. One loses prestige via someone else's misconduct only if it reflects on one's own failures. In the example of the scholar-official above, the son's misconduct is taken to be a reflection of the father's failure to educate the younger generation properly. Like his son, he is guilty of unfiliality—in having brought disgrace to the family and hence to the ancestors—and this, of course, is damaging to his prestige. The conclusion, therefore, is that personal responsibility is inevitably involved, however indirectly, in judgments of personal prestige but not necessarily in judgments of face.



FACE AS A UNIVERSAL

One of the points of contention concerning face centers on the question raised in the beginning of this paper: does the concept of face have universal applicability? To put the question differently: is face behavior distinctively Chinese, or at least more prominent by far in Chinese society than in other societies?

The controversy concerning whether or not face is a universal arises partly from the level of generality at which the concept is defined. In the preceding analysis, face was treated in terms of its general significance to adequate social functioning, which is, of course, a matter of universal concern. Throughout the writing of the present paper, I have been conscious of the fact that the presentation of self vis-à-vis others is a basic problem that no one, in any society, can avoid. It must have occurred to many readers that, outside of the Chinese social context, social encounters which may indeed be regarded as instances of face behavior can be observed—even though they are typically not described or interpreted in such terms. Most of the illustrations I have used above are not specific to the Chinese.

Not to embarrass people in public may be cited as an example of *liu mien-tzu* (saving someone else's *mien-tzu*). "Keeping up with the Joneses," games of "one-upmanship" (popularized by Stephen Potter [Goffman 1959, p. 191]), and the challenges to gun duels in the 19th-century American West are more aggressive forms of face behavior. In diverse cultural contexts, men continue to perform daring feats which may cost them dearly, including the loss of their lives, for fear of the consequences of losing face. Of late, the face of males vis-à-vis females (or what has been referred to as the "male ego") is being seriously challenged, and is perhaps undergoing changes, with the advent of the women's liberation movement. Codes of honor, to which elitist groups in many societies are bound, constitute a special kind of face. Thus, among the European aristocracy challenges to a duel had to be accepted to avert the stigma of cowardice, even though the one challenged might well know in advance that his chances of survival were grim. And anyone who is skilled in the negotiation of disputes knows very well that face is a factor to be reckoned with, aside from the more obvious issues of self-interest of the parties concerned. Indeed, face is immanent in human conflicts, both in their avoidance and resolution (see Ho [1974] for a discussion of face and its relationship with conflicts).

The point is that face is distinctively human. Anyone who does not wish to declare his social bankruptcy must show a regard for face: he must claim for himself, and must extend to others, some degree of compliance, respect, and deference in order to maintain a minimum level of effective social functioning. While it is true that the conceptualization of what constitutes face and the rules governing face behavior vary considerably across

cultures, the concern for face is invariant. Defined at a high level of generality, the concept of face is a universal.

Now, the Western mentality, deeply ingrained with the values of individualism, is not one which is favorably disposed to the idea of face. For face is never a purely individual thing. It does not make sense to speak of the face of an individual as something lodged within his person; it is meaningful only when his face is considered in relation to that of others in the social network. Thus, in social encounters, the question of face frequently arises beyond the realm of the individual's own responsibility—that is, not only from his actions, but also from (a) the actions of people closely associated with him which have a bearing on his face, and (b) how he and people with whom he is closely associated are treated by others. Reciprocity is the key to the understanding of face behavior: to extend face to others is no less important than to safeguard one's own.

The analysis of face behavior thus leads to a more explicit recognition of how the roles of individual responsibility and subjective volition are circumscribed in social interactions. This would appear to have the effect of diminishing the stature of man as an individual who seeks to gain mastery over the environment and over his own psychic life and destiny. I submit, however, that the portrayal of man under the ideology of individualism represents only one idealized version of his social existence. It does not reflect the true state of affairs—namely, that much of the time man is subject to the impact of social actions beyond his control and responsibility and that his subjective volitions are constrained by the necessity of having to meet the social expectations of others, to a greater extent than individualism would lead us to appreciate. I do not wish to imply that the expectations of others toward oneself are excluded from consideration in the ideology of individualism. Rather, others' expectations are existent insofar as they have been incorporated into the individual's own subjective frame of reference, that is, into his own definition of their significance for his own action. The individual, and not the reciprocity between individuals, remains the focal point of concern. In relegating the concept of face to the status of having only particularistic, rather than universal, applicability, or in saying that face behavior is of minor significance in Western societies, the individualism-dominated social sciences fall victim to one of their blind spots. To be sensitive to the variations in which face behavior is manifest in the daily lives of men under a diversity of cultural conditions would give fresh ammunition to the social sciences toward a major breakthrough from their present intellectual encapsulation.

#### CONCLUSION

The foregoing analysis serves to elucidate the ramifications of the concept of face and to distinguish it from other closely related concepts. We are

now prepared to advance a definition of this complex concept. Face is the respectability and/or deference which a person can claim for himself from others, by virtue of the relative position he occupies in his social network and the degree to which he is judged to have functioned adequately in that position as well as acceptably in his general conduct; the face extended to a person by others is a function of the degree of congruence between judgments of his total condition in life, including his actions as well as those of people closely associated with him, and the social expectations that others have placed upon him. In terms of two interacting parties, face is the reciprocated compliance, respect, and/or deference that each party expects from, and extends to, the other party.

In the course of attempting to achieve a conceptual clarity for face, it is also necessary to clarify the relationship between it and other sociological concepts. In this paper, I have treated face as a key concept that ties together a number of separate sociological concepts, such as status, authority, prestige, and standards of behavior. Face is a concept of central importance because of the pervasiveness with which it asserts its influence in social intercourse. It is virtually impossible to think of a facet of social life to which the question of face is irrelevant. The desire to gain face, to avoid losing face, and to save face when it is threatened is a powerful social motive. The study of face dynamics promises to elucidate the subtleties of social interaction—and if face is a complex concept, it is so because social interaction is itself a highly complicated affair. It is probably on account of its potential to reflect the complexities of social interaction that the study of face is rendered intriguing to students of human behavior.

The distinction which I have made between face on the one hand and personality and personal prestige on the other is a basic one with great theoretical import. It reflects two fundamentally different orientations in viewing human behavior: the Western orientation, with its preoccupation with the individual, and the Chinese orientation, which places the accent on the reciprocity of obligations, dependence, and esteem protection. These two orientations need not, and should not, be regarded as mutually exclusive. Rather, they are complementary. Neither, when taken alone, is capable of yielding a complete account of the total complexity of social phenomena.

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## Commentary and Debate

### MANAGEMENT CONTROL IN THE LARGE CORPORATION: COMMENT ON ZEITLIN

As demonstrated recently by his article "Corporate Ownership and Control: The Large Corporation and the Capitalist Class" (*AJS* 79 [March 1974]: 1073-1119), Zeitlin is an able theorist as well as a competent researcher. By any standards, his systematic critique of the conventional sociological wisdom concerning the relationship of the capitalist class to the large corporation is most profound. Although I am sympathetic with his concern for the issue of control in the large corporation and concur with many of his conclusions, I must take exception to his discussion at several points. For the most part, my comments are restricted to the issue of management control in the large corporation.

#### Separation of Ownership and Control

Certainly the most important problem involves Zeitlin's interpretation of the theory of the "separation of ownership and control." Indeed, he does not seem to appreciate fully some of the central components of that theory. He is quite correct in his criticism of the "teleology of bureaucratic imperatives" espoused by several sociological theorists. However, it must be noted that this teleology is not to be found in the original theory proposed by Berle and Means (1932). To the contrary, they assert that the separation of ownership and control occurs only as a result of the capital requirements of the large corporation. As they put it, "In general the larger the company, the more likely is its ownership to be diffused among a multitude of individuals" (p. 53). In short, large corporations tend to be management controlled simply because their capital requirements have resulted in a dispersion of stock ownership. Also, Berle and Means clearly consider the separation of ownership and control to be more of an emergent historical process than an accomplished historical fact. For example, they declare that "the dispersion of ownership has gone to tremendous lengths among the largest companies and has progressed to a considerable extent among the medium sized" (p. 53).

After reviewing the results of the recent studies of corporate control, Zeitlin concludes that their findings are "discrepant" and that the separation of ownership and control may well be a "pseudofact." Zeitlin may have overstated his case in this regard. Although the studies he cites arrive at somewhat different results, that fact is almost entirely attributable to the different samples and criteria for management control employed by

TABLE 1

TYPE OF CONTROL BY SIZE RANK OF CORPORATIONS FOR 500 LARGEST CORPORATIONS

	1-100	101-200	201-300	301-400	401-500	Total
Larner study:						
Minority .....	22	17	22	34	38	133
Management .....	78	83	78	66	62	367
Burch study:						
Family .....	36	43	49	58	50	236
Management .....	64	57	51	42	50	264
Sheehan study:						
Family .....	10	23	39	38	37	147
Management .....	90	77	61	62	63	353

NOTE.—The original control categories have been collapsed, whenever necessary, to ensure the comparability of results. In the Larner study, minority control includes all forms of control other than management control. In the Burch study, management control includes all forms of control other than family control.

each researcher. In table 1 I present a summary of the results of the three major studies of corporate control by the size rank of the corporations. Although Larner (1970) and Burch (1972) study the same sample of the 500 largest nonfinancial corporations as ranked by their assets in 1963, Larner uses 10% stock ownership as the minimum criterion for minority control, whereas Burch uses 4% stock ownership and representation on the board of directors as the minimum criteria for family control. Sheehan (1966) studies the 500 largest industrial corporations as ranked by their sales in 1966, using 10% stock ownership as the minimum criterion for family control. Given these differences in samples and criteria, the minor inconsistencies in results are hardly surprising. Nevertheless, each of the three researchers finds that a majority of the 500 largest corporations are subject to management control. More important, these empirical results consistently confirm the theory of the separation of ownership and control inasmuch as management control is positively associated with the size of the corporations. Among the 100 largest corporations, the proportion that are management controlled is between 64% and 90%, while among the 100 smallest in the samples of the 500 largest, the proportion is between 50% and 63%. It is apparent that the original theory of the separation of ownership and control advanced by Berle and Means is generally substantiated by these results. Although management control among the large corporations may not warrant the status of an incontrovertible social "fact," management control does appear to be the predominant form of control among large corporations.

Unfortunately, in his attempt to document the extent of private ownership and family control among the large corporations, Zeitlin largely neglects the empirical adequacy of the theory of the separation of ownership and control. For example, he notes that the use of the *Fortune* directory

of the 500 largest industrial corporations as the sample for studies of corporate control entails an inherent bias, since privately owned corporations are excluded from the directory unless they publish annual financial statements. Citing the Sheehan study (1967) which lists 26 privately owned corporations that would otherwise qualify for inclusion among the 500 largest industrial corporations as ranked by sales, Zeitlin concludes that the addition of these corporations to those studied by Larner (1970) raises the number of privately controlled corporations among the 500 largest from five to 31, or sixfold. This methodological point is well taken. Nevertheless, these data are entirely consistent with the theory of the separation of ownership and control in the large corporation. Specifically, none of Sheehan's 26 privately owned corporations would rank among the 100 largest industrial corporations in 1966; and well over half, 15 to be exact, would rank among the smallest 100 of the 500 largest in terms of sales.

#### Corporate Interlocks and Corporate Control

At one point, Zeitlin implies that director representation, through interlocking directorates, constitutes an alternative form of corporate control to direct stock ownership (p. 1101). He notes astutely that it is the large corporations, those most likely to be management controlled in terms of the dispersion of stock ownership, which maintain the greatest number of interlocks with financial institutions and nonfinancial corporations. Unfortunately, none of the studies of corporate interlocks or corporate control have examined directly the relationship between stock ownership and interlocking directorates. However, it is possible to examine that relationship among the 200 largest nonfinancial corporations in 1970 (Allen 1974). Table 2 presents the average number of interlocks with the 50 largest financial corporations and the 200 largest nonfinancial corporations, as well as the proportion of directors drawn from management, by

TABLE 2  
AVERAGE NUMBER OF FINANCIAL AND NONFINANCIAL INTERLOCKS AND PROPORTION OF  
MANAGEMENT DIRECTORS FOR 200 LARGEST NONFINANCIAL CORPORATIONS  
IN 1970 BY DEGREE OF MANAGEMENT CONTROL

Type of Control	Financial	Nonfinancial	Directors from Management (%)
Probable management (135) .....	3.6	5.9	31.8
Possible family (33) .....	3.1	5.0	38.0
Probable family (32) .....	2.6	3.9	44.5

NOTE.—N's in parentheses.

the degree of management control among the 200 largest nonfinancial corporations. If any individual or family held more than 10% of the common stock, the corporation was classified as being under probable family control. If any individual or family represented on the board of directors held less than 10% but more than 1% of the common stock, the corporation was classified as being under possible family control. All other corporations were classified as being under probable management control. Information on stock ownership was drawn from earlier studies of corporate control (Lerner 1970; Burch 1972) and from reports filed with the Securities and Exchange Commission. It is evident from these data that family control is associated with fewer interlocks with financial and nonfinancial corporations and with a larger proportion of directors drawn from management. This pattern can be attributed, in large part, to the fact that family-controlled corporations often have members of the family as directors. For example, family members serve as chairmen of the boards of such family-controlled corporations as Ford Motor, W. R. Grace, Getty Oil, Weyerhaeuser, North American Rockwell, Reynolds Metals, Loews, McDonnell Douglas, Lykes Youngstown, and Amerada Hess.

#### Financial Control of Nonfinancial Corporations

Certainly one of the most important issues raised by Zeitlin is the extent to which financial corporations control nonfinancial ones through a combination of stock ownership and director representation. To support his argument that large commercial banks control many nonfinancial corporations, Zeitlin presents a secondary analysis of data demonstrating that bankers occupy a disproportionate share of the multiple directorships held by interlocking directors. It is possible to demonstrate the influence of bankers even more clearly through a comparison of the average number of interlocks held by directors who are management officials of different types of corporations. Table 3 presents original data on the average number of directorships held by chairmen and other management directors among 800 major corporations in 1970 by type of corporation. This sample of 800 corporations includes the 550 largest industrial corporations and the 50 largest commercial banks, life insurance companies, public utilities, transportation companies, and retailing companies as ranked by either sales or assets in 1970 (*Fortune* 1971a; 1971b). In general, these data confirm the results presented by Zeitlin, but with one important qualification: position within a corporation is as important as the type of corporation in determining the number of directorships held by a management official. Chairmen of the boards, typically the chief executive officers of their corporations, are far more influential in terms of the number of



TABLE 3

AVERAGE NUMBER OF INTERLOCKS FOR MANAGEMENT DIRECTORS AMONG  
800 MAJOR CORPORATIONS IN 1970 BY TYPE OF CORPORATION

Type of Corporation	Chairmen	Other Directors
Bank (50) .....	3.20 (50)	1.50 (126)
Life insurance (50) .....	1.84 (50)	1.14 (148)
Large industrial (50) .....	2.50 (50)	1.29 (268)
Other industrial (500) .....	1.88 (500)	1.16 (1,952)
Utility (50) .....	1.98 (50)	1.38 (98)
Retailing (50) .....	1.90 (50)	1.20 (264)
Transportation (50) .....	1.68 (50)	1.17 (121)

NOTE.—N's in parentheses.

directorships held than are other management officials. Also, the chairmen of the large commercial banks are more influential than chairmen of other types of corporations, although the chairmen of the large industrial corporations also appear to be quite influential in terms of the number of directorships held.

Further evidence as to the dominant position of financial institutions among the large corporations is to be found in the reports of recent congressional investigations (U.S. Congress 1967; 1968; 1973). These reports disclose that large commercial banks, particularly the major ones in New York, are often principal stockholders in many large corporations. The stock in question is generally held by the trust departments, much of it on behalf of employee-benefit plans. In 1973, approximately \$400 billion were held in trust accounts administered by the 3,800 commercial banks with trust departments; approximately \$114 billion, over 25% of the total, were administered by 10 large New York banks. Such funds are typically invested in the common stock of large corporations. A recent congressional investigation of the 30 principal stockholders of the largest American corporations disclosed that seven large New York banks combined held an average of 11.1% of the common stock in 25 of the 99 largest industrial corporations (U.S. Congress 1973, p. 26). The remaining 74 industrial corporations failed to disclose voluntarily the identities and holdings of their principal stockholders. Similarly, the same seven New York banks combined held an average of 14.6% of the common stock in 15 of the 19 largest railroads and 8.4% of the common stock in 20 of the 51 largest public utilities. Consequently, commercial banks are often the single largest stockholders in large corporations. For example, the seven large New York banks were the single largest stockholders in 15 of the 25 industrial corporations which disclosed their principal stockholders.

The existing evidence on director representation and stock ownership among major nonfinancial corporations by large commercial banks leaves little doubt as to the influential position of these banks with respect to

nonfinancial corporations. However, it does not necessarily follow that banks represent an institutional mechanism for the indirect control of nonfinancial corporations by principal stockholders. The critical issue is the extent of management control among the large commercial banks. Although some very large banks are controlled by particular families, such as the Mellons and Rockefellers, minority or family control does not appear to be any more prevalent among commercial banks than among nonfinancial corporations. As Zeitlin acknowledges, approximately 75% of the 200 largest commercial banks are management controlled, if 10% stock ownership is used as the minimum criterion for family or minority control (Vernon 1970). It might also be noted that 25 of the 27 largest banks are management controlled. As in the case of nonfinancial corporations, management control seems to be associated with larger size among commercial banks. Indeed, congressional investigations have disclosed that the principal stockholders of most large commercial banks are other large commercial banks. For example, the six largest banks in New York combined held an average of 15.7% of each other's common stock. Moreover, the major New York banks are often their own principal stockholders. For example, the six largest banks held an average of 5.9% of their own common stock (U.S. Congress 1967, pp. 82-83).

#### Corporate Elites and the Capitalist Class

The distinction between managers and principal stockholders is not always easy to establish or maintain in practice. Certainly the senior management officials of the large corporations do not fit the stereotype of the bureaucratic manager suggested by sociological theory. It is not unusual for senior management officials, particularly chief executive officers, to receive extremely large salaries and bonuses from their corporations. The only source of accurate information on the remuneration of chief executive officers is the Form 10-K annual report filed with the Securities and Exchange Commission. According to these reports, the chief executive officers of the 200 largest industrial corporations received an average of \$307,170 in total remuneration in 1973. This figure includes salary, bonus, director fee, and deferred compensation. It does not include capital gains from stock options or dividends from company stock owned by the chief executive officer. These salaries frequently reach almost astronomical levels. Among the chief executive officers of the 200 largest industrial corporations in 1973, 35 received more than \$400,000, and six of the 35 received more than \$600,000, in total remuneration for that year.

It is also not unusual for senior management officials to own considerable amounts of stock in the corporations they manage, owing in large part to their participation in stock-option plans. According to the official data on

the 200 largest industrial corporations, the chief executive officers owned common stock in their corporations worth an average of \$22,611,915 as of the end of 1973. However, this aggregate figure is somewhat misleading inasmuch as it includes the stockholdings of several multimillionaire entrepreneurs or their descendants. Such entrepreneurs include Edwin H. Land, J. Paul Getty, Sanford N. McDonnell, Charles Revson, and William R. Hewlett. The descendants of entrepreneurs include Richard S. Reynolds, Jr., George H. Weyerhaeuser, J. Peter Grace, Amory Houghton, and Henry Ford II. If the 28 chief executive officers with more than \$10,000,000 in corporation stock are excluded from the analysis, the average value of the common stock held by the remaining 172 drops to a more modest \$1,531,331.

The fact that senior management officials receive large salaries and own considerable stock in their corporations does not necessarily imply that these managers constitute an autonomous class or that they have been incorporated into the capitalist class. If the family is the basic unit of social class, as Zeitlin maintains, it is far more accurate to speak of a managerial elite rather than a class as such. Managerial positions are transmitted on the basis of familial relationships only when a family is also a principal stockholder in a corporation. In sociological parlance, managerial position is usually an achieved status, whereas the position of principal stockholder is typically an ascribed one. The senior management officials of the large corporations may become relatively wealthy as a result of their direct remuneration and their participation in stock-option plans, but their wealth is rarely of the same magnitude as that of principal stockholders. More important, management officials often have socioeconomic backgrounds very different from those of the principal stockholders. This differentiation between the managerial elite and the capitalist class of principal stockholders is viewed by some with no little apprehension. Baltzell, for one, suggests that "a powerful, wealthy, yet declassed elite may be one of the greatest threats to freedom in modern American society" (1971, p. 215).

### Profit Maximization and Management Control

Zeitlin devotes considerable attention to the notion of "managerial discretion" in the large corporation. On the basis of a review of the literature on profit maximization and management control, he concludes that "purportedly management-controlled and owner-controlled corporations are similarly profit oriented" (p. 1097). However, it is equally possible to draw the opposite conclusion from the empirical research on profit maximization and management control because of the largely inconclusive results of this research. For example, Larner does not find "systematic negative

evidence" concerning the hypotheses of managerial discretion, as Zeitlin asserts (p. 1095). Instead, Lerner finds that "this evidence, though by no means conclusive, provides some support for hypotheses of managerial discretion, but suggests that the magnitude of this discretion is not as large as it is often implied" (1970, p. 29). Two other studies of managerial discretion (Kamerschen 1968; Hindley 1970) cited by Zeitlin report similar differences in the profit rates of management-controlled and owner-controlled corporations but conclude that the differences are not statistically significant. Finally, the original study of profit maximization and management control found that "the owner controlled group of firms outperformed the management controlled firms by a considerable margin" (Monsen, Chiu, and Cooley 1968, p. 442).

Despite his conclusions, Zeitlin is well aware of these inconsistencies and cogently identifies one of the major sources of the problem. Given the different criteria employed to classify particular corporations as either management controlled or owner controlled, it is difficult to put much store in the results of studies using this imperfectly measured variable of "corporate control" as the primary independent variable. Indeed, the fact that different classifications result from different criteria of management control is one of the major points offered by Zeitlin. Specifically, he argues that the lack of accurate information on the proportion of common stock held by principal stockholders leads to the misclassification of many owner-controlled corporations as management controlled. This possible source of bias could well account for the inconclusive findings on profit maximization and management control. If there is a significant relationship between owner control and profitability, it would be greatly attenuated by the misclassification of several owner-controlled corporations as management controlled. This interpretation is consistent with the literature in this area, inasmuch as the only study to have found a significant relationship between owner control and profitability also employed the most stringent criteria for management control (Monsen et al. 1968, pp. 438-39).

### Conclusions

As I stated at the outset, my comments pertain chiefly to the issue of management control in the large corporation. I have sought to demonstrate not only the empirical adequacy of the theory of the separation of ownership and control proposed by Berle and Means but also some of the institutional developments associated with management control among the large corporations. I do not wish to imply that the owners of capital have been expropriated by the managers of capital; however, I do suggest that members of the managerial elite are far more influential in relation to the capitalist class of principal stockholders than many social scientists are

prepared to admit. Furthermore, I contend that the theoretical aggregation of managers and principal stockholders into an amorphous capitalist class may obscure many issues of profound theoretical and substantive significance. By resurrecting the theory of the separation of ownership and control, I hope to redirect attention to the fundamental issue of cohesion and integration among the members of the economic elite. Even on the core issue of profit maximization, it is by no means clear that the problem of divergent economic interests between managers and owners has been effectively resolved by the incentive system of the large corporation. Moreover, the unification of the social and political interests of managers and principal stockholders is far more tenuous and problematic. The critical issue in this regard is the degree to which quantitative differences in the wealth and qualitative differences in the social origins of management officials and principal stockholders serve as barriers to the complete integration of the managerial elite into the capitalist class of principal stockholders. Unfortunately, this issue remains largely unresolved. Despite our differences with respect to the extent of management control among the large corporations and some of its consequences, Zeitlin and I agree on the importance of examining the often subtle processes of differentiation and integration among management officials and principal stockholders.

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## ON CLASS THEORY OF THE LARGE CORPORATION: RESPONSE TO ALLEN<sup>1</sup>

Allen's note raises several interrelated issues (and a few quibbles) none of which can be dealt with fully here because of space limitations: my response is much shorter than I could wish.

### "Management Control": Fact or Pseudofact?

To begin with, the three recent studies Allen cites do *not* "consistently confirm the theory of the separation of ownership and control." I presented an extensive analysis of the problems entailed in obtaining reliable and valid evidence on the actual ownership interests involved in a given corporation and concluded that the separation of ownership and control is probably a pseudofact. Allen presents no evidence or reasoning to alter that conclusion or to substantiate Larner's or Sheehan's findings. Further, Allen's assertion that "each of the three researchers concludes that a majority of the 500 largest corporations are subject to management control" is not correct. In fact, Burch (1972) classified only 124 of the top 300 under "probably management control" (PM), whereas he put 48 under "possibly family control" (F?) and 128 under "probably family control" (PF) (pp. 36 ff.). Employing "less intensive study" of the firms ranked by sales from 301 to 500, Burch classified them as follows: PM, 76; F?, 16; PF, 108 (pp. 160 ff.), yielding a total for the 500 of PM, 200 (40%); F?, 64

<sup>1</sup> I wish to call attention to an error in the original article (*AJS* [March 1974]). In table 3, p. 1104, under Other, the percentage in the column headed 4 plus should read 1, not 11.

(12.8%); and PF, 236 (47.2%)—an estimate that Burch thinks *exaggerates* the extent of management control. With exemplary scholarly caution, he classifies firms under F? when the evidence is not conclusive enough to classify them as PF but when “one or more families have been identified as being prominently associated with the company in question” (p. 34). Without explanation, Allen has simply recategorized Burch’s 64 “possibly family controlled” firms under “management” control, thereby creating a “majority” in the latter category.<sup>2</sup>

### Management Control and Corporate Size

If we take the 500 industrial corporations studied by Burch and retain his classification, we find only a modest relationship between size rank and type of control ( $C = 0.169$ ) (table 1). The relationship between size and

TABLE 1

TYPE OF CONTROL OF 500 LARGEST INDUSTRIAL CORPORATIONS, 1964,  
BURCH CLASSIFICATION, BY SALES RANK OF CORPORATION

SALES RANK OF CORPORATION	TYPE OF CONTROL (%)			
	PM	F?	PF	
1-50 .....	58 } 44	20 } 20	20 } 36	
51-100 .....	30 }	18 }	52 }	
101-50 .....	44 } 42	18 } 15	38 } 43	
151-200 .....	40 }	12 }	48 }	
201-50 .....	36 } 38	12 } 13	52 } 49	
251-300 .....	40 }	14 }	46 }	
301-50 .....	34 } 34	8 } 8	58 } 58	
351-400 .....	34 }	8 }	58 }	
401-50 .....	40 } 42	8 } 8	52 } 50	
451-500 .....	44 }	8 }	48 }	
Total <i>N</i> .....	200	64	236	
Total % .....	40.0	12.8	47.2	

SOURCES.—Distribution calculated from Burch (1972, table 3-1, pp. 36 ff. and table C-1, pp. 160 ff.); rank from *The Fortune Directory for 1964*.

NOTE.—PM = probably management; F? = possibly family; and PF = probably family.

$$C = \sqrt{\frac{\chi^2}{N + \chi^2}} = 0.169.$$

<sup>2</sup> Chevalier's (1969) findings were inadvertently omitted from my article. With 5% as the management control threshold, and dominant influence (DI) referring to probable minority control, using public and numerous private sources, he classified the control of the 200 largest industrials as follows: management, 80; individuals (including directors) and families, 81 (plus 4 under DI); majority owned, 4; financial, 19 (plus 12 under DI). Chevalier considers this an *overestimate* of the number under management control.

management control is particularly weak. Within deciles, the relationship is anything but consistent. Moreover, of the three types of control, F? is most clearly associated positively with size rank, which probably means only that the *visibility* of controlling proprietary interests is negatively associated with size: the larger the corporation, *ceteris paribus*, the more difficult it is to discover the locus of control.

Otherwise, a finding that size and management control are positively associated is meaningless, since there are cogent reasons to doubt that the methods of analysis and data utilized by Larner (1970) and Sheehan (1966) provide valid and reliable evidence on the locus of control, and Burch's, while unquestionably superior, are also inadequate. After all, Larner (1970) himself presented a table that showed fewer management-controlled firms in the lowest assets groups (301-500); and he tabulated Berle and Means's classifications and found "that management control . . . was concentrated among the larger firms" (pp. 18-19). Would that not mean, by Allen's logic, that the "theory" had already been confirmed by Berle and Means over three decades ago?

Three aspects of the "dispersion of ownership" are often confused: as the size of a corporation increases, there tend to be (1) an increasing number of shareholdings, (2) a decreasing proportion of shares held by management, and (3) a decreasing proportion of shares held by principal share-owners. Assuming their empirical validity, none of these developments necessarily means that proprietary interests are shorn of control of the large corporation. In fact, dispersion among a multitude of shareholders, or a slight ownership interest by management, may be precisely the basis for proprietary control. As Berle and Means (1967) themselves noted: "The larger the company and the wider the distribution of its stock, the more difficult it appears to be to dislodge a controlling minority" (p. 75). Therefore, as I argued in my article, the potential for control represented by a specific proportion of shares held cannot be discovered without a "case study of the pattern of ownership within the given corporation [and] . . . the system of intercorporate relationships in which the corporation is implicated" (p. 1091). I have since sought to demonstrate this in a study of the large corporation in Chile (Zeitlin, Ewen, and Ratcliff 1974). Much as in the United States and England, the largest corporations were most likely to be classified under management control using Berle and Means's methods and procedures. Rather than take appearances for reality, however, employing a method of analysis focusing on intercorporate relationships and the web of kinship revealed the actual controlling proprietary families and associates in 14 of the 15 firms originally classified under management control. A causal interpretation of the association between size and management control is spurious.



Management Control and the Interlocking Directorate

Allen's evidence concerning the association between the frequency of corporate and financial interlocking and types of corporate control is consistent with what I suggested in my article, namely, that the corporations classified under management control interlock more frequently than others with the 50 largest banks and insurance companies, as well as with other top 200 corporations. Of course, since the largest corporations interlock more frequently, and also tend to be classified under management control more frequently than others, the effect of corporate size on the association between type of control and interlocking has to be investigated. Assuming that the type of control, even taking size into account, is associated with a greater frequency of interlocks, the question is: What relevance does this have for the managerial versus the class theory of the large corporation? On this, Allen is silent. Yet he has shown that one of the basic assumptions of managerial theory is false. In managerialist imagery, the large management-controlled corporation exists in a state of splendid autonomy, each one's management independent of the other, impregnable, and invulnerable to "external authority." Indeed, nary a word is uttered by managerialists about interlocking directorates. "Management," it is said, simply "selects itself and its successors as an autonomous and self-perpetuating oligarchy" (Galbraith 1967, pp. 88, 409). The managerialists also argue that, because of the "changed origin of finance capital" (*sic*)<sup>3</sup> the large corporation is emancipated from control by principal owners of capital and that the corporation's management, therefore, gains "complete decision-making power" over its capital. The large corporation, in Berle's words (Berle and Means 1967), "runs on its own economic steam. . . . Management thus becomes, in an odd sort of way, the uncontrolled administrator of a kind of trust, having the privilege of perpetual accumulation" (pp. xiv-xv). If corporate managements were really "uncontrolled administrators," because of decreased and decreasing dependence on outside liabilities for financing (itself an assumption contradicted by the evidence [Lintner 1967, pp. 179-84; Payne 1961, pp. 130-39; Kuznets 1961, pp. 248, 264, 268]) we should certainly not expect to find, as Allen has, that "management-controlled" corporations interlock more frequently than the family controlled with the largest financial institutions, as well as with other large corporations. These findings are contrary to the implicit hypotheses of managerial theory. (Elsewhere, Allen [1974] also concluded that the implicit managerial hypothesis that the frequency of interlocks between large corporations and banks has

<sup>3</sup> Berle, of course, is using the term "finance capital" merely as a synonym for "loan capital" and not in the specific sense in which, following but departing slightly from Hilferding (1910), I have defined it. See below.

"declined through time, . . . because these corporations have attained an increased capacity to generate capital internally . . .," was "*disconfirmed*" [pp. 402-3; italics added].)

Though contrary to managerial theory, Allen's findings are quite consistent with the class theory of corporate control elaborated in my article. In reality, "management-controlled" corporations are not shorn of proprietary control; their ownership is merely relatively dispersed in comparison with other corporations. Put with desperate brevity, they tend to interlock more frequently with other large corporations and the largest financial firms for the following reasons: The corporate form multiplies the potential for control inherent in every unit of capital by permitting several corporations to have common principal shareowners as well as to own shares in each other. The interlocking directorate is one formal method of corporate combination that enhances the ability of owners of large blocks of stock in several corporations to assure that these corporations' actions do not adversely affect each other—or the common principal shareowners' investments in them. Further, the consolidation of small minority control may be enhanced by formal representation in management. Therefore, the tendency for common principal shareowners in several corporations (even if those corporations are not under their control), as well as corporations that have intercorporate holdings, to attempt to place themselves or their representatives in those corporations' managements, is reflected in a pattern in which the putatively "management-controlled" corporations are more tightly interlocked than others with other large corporations.

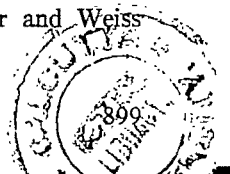
This hypothesis, *mutatis mutandis*, also applies to Allen's finding that the "management-controlled" corporations interlock more frequently than others with the largest banks and to his other finding that bank directors, particularly board chairmen, have a higher average number of directorships than other directors. (The latter data, as Allen notes, "confirm the results presented by Zeitlin.") A large bank typically has principal interests in several large corporations, and usually plays a critical role in mergers, acquisitions, and reorganization. It will, therefore, strive to prevent those corporations from taking actions that might be adverse to the others' (and, thereby, its own) interests. Diversification and conglomeration increase the extent to which the largest corporations are actual or potential competitors throughout the economy; and a bank that has common dealings with several such corporations, therefore, finds it even more essential to attempt to coordinate their policies in the common corporate interest. The same bank may interlock several corporations in which it has interests (and which are prohibited from direct interlocks with each other) by placing a different representative in each. If several large banks form a lending syndicate for a given corporation, or if the banks interpenetrate in

their ownership (see U.S. Congress 1974, p. 7), the necessity for them to coordinate their policies and reconcile their interests is even more imperative. The large corporation's "vulnerability to external authority" is greater if its stock is scattered among a multiplicity of small shareowners. Therefore, we should expect, as Allen finds, that ostensibly management-controlled corporations are more likely than others to have representatives of the largest banks on their boards. This permits, as J. P. Morgan once explained, "a certain number of men owning property . . . [to] do what they like with it . . . and act toward mutual harmony" (*New York Tribune*, March 27, 1902).

### Who Controls the Banks?

Although Allen also emphasizes the "dominant position of financial institutions among the large corporations," he doubts my hypothesis that the large banks are units in, and instrumentalities of, the system of propertied interests controlled by principal capitalist families. For him, the "critical issue . . . is the extent of management control among the large commercial banks," and he refers to Vernon's (1970) finding (which I discussed) that 75% of the 200 largest commercial banks are under management control. However, Allen ignores Burch's (1972) finding (which I also discussed) that, at most, only 24 of the 50 largest commercial banks were management controlled. I suggest that studies with appropriate data would reveal that the other largest banks, behind the managerial veil protecting their proprietary modesty, are also controlled by principal owners of capital. But what if this were not so? What if, as Allen avers, the largest banks were management controlled? What implications would this have for the hegemony of the capitalist class and for the political economy? Allen states none. No longer under the control of specific owners, would the banks forsake the maximization of profit for "satisficing" (*sic*) (Simon 1962) conduct? Would the executives of such management-controlled banks be transformed, as was hoped (Berle and Means 1967) of their corporate counterparts, into a "purely neutral technocracy" allocating "the income stream on the basis of public policy rather than private cupidity" (p. 313)?

The startling fact is that bank trust departments "manage assets substantially exceeding the assets of the 100 largest corporations in the United States" and have "larger securities portfolios than all other institutional investors combined" (Lybecker 1973, p. 997). Whose investments are managed by the largest bank trust departments? Generally, they will not administer personal capital of less than \$100,000. Morgan Guaranty, for instance, has a \$200,000 minimum. Thus, these trust departments probably serve fewer than 1% of Americans (see Projector and Weiss



1966). What might these principal owners of capital do if the management-controlled banks pursued anything but profit-maximizing investment policies? Would these banks be long permitted to retain their trust (*sic!*)? Allen accepts Vernon's finding that most of the largest banks are management controlled. What investment policies have their trust departments pursued? Those "on which they have leaned very heavily in the last few years" have been to trade in the "market of a select few securities, usually with high price-earning ratios, so-called glamour stocks, . . . to the virtual exclusion of the other market" of less favored stocks (Loomis 1973, p. 83; U.S. Congress 1974, p. 16). Apparently the glamor has not gone out of the pursuit of profit, even for the management-controlled banks. Certainly, there would be far-reaching reverberations for a bank's survival if the principal owners of capital (whose trusts and investment portfolios constitute one of its prime sources of capital) found that the bank was giving them, on the average, a lower rate of return than other banks and, therefore, shifted their trusts elsewhere. It would set in motion a rapid succession of similar and related moves by major corporate clients, other large banks, and large depositors such that a general "loss of confidence" would drain the bank's funds, render it incapable of honoring its contractual obligations, and drive it into collapse. Thus the "discipline of the market," which reflects the investment decisions of principal individual and institutional owners of capital, requires the banks not to deviate significantly from profit-maximizing policies. To the extent, therefore, that the management-controlled banks are both the creditors and principal shareholders of the largest management-controlled corporations, and interlock tightly with them, these banks will, in turn, impose their own profit-maximizing requirements on them.

#### The "Inner Group" and "Finance Capitalists"

With this coalescence of financial and industrial capital, as I argued in my article, there also tends to emerge a new social type, the "finance capitalist." Neither "financiers" extracting interest at the expense of industrial profits nor "bankers" controlling corporations, but finance capitalists on the boards of the largest banks *and* corporations preside over the banks' investments as creditors *and* shareholders, organizing production, sales, and financing, and appropriating the profits of their integrated activities. Thus, even were certain banks and corporations to become so large that they were not ordinarily under the control of specific minority ownership interests, it would not mean that power had passed to the "new princes" of the managerial realm (Berle and Means 1967, p. 116; and see Zeitlin, Ewen, and Ratcliff 1974, pp. 113-17). Although the largest banks and

corporations might conceivably develop a relative autonomy from *particular* proprietary interests, it would be limited by the *general* proprietary interests of the principal owners of capital. To the extent that the largest banks and corporations constitute a new form of class property—of social ownership of the means of production by a single social class—the “inner group” (U.S. Congress 1965, p. 4 and *passim*) of interlocking officers and directors, and particularly the finance capitalists, become the leading organizers of this system of classwide property. That is why I hypothesize that they “represent a special social type in contrast to other officers and directors of the largest corporations and banks” (Zeitlin 1974, p. 1103; also see p. 1110). They should be far more likely than ordinary corporate executives to be drawn from the “upper” or “dominant” or “capitalist” class—the social class formed around the core of interrelated principal owners of capital.<sup>4</sup> Since Allen has already provided supporting evidence for other hypotheses suggested in my article, I hope he will also carry out research on this particularly critical hypothesis concerning the internal differentiation of the capitalist class.

#### Profit Maximization and Management Control

Here Allen raises mainly quibbles. He forgets that Larner tested not one but three interrelated propositions: on *level* of profit rates, on *variability* of profit rates (to measure “risk taking”), and on executive compensation. That is why I summarized Larner’s (1970) findings as providing “systematic negative evidence” concerning the main managerial propositions. Immediately after the words Allen quotes from him, Larner continues: “It appears that proponents of theories of managerial discretion have expended much time and effort in describing a phenomenon of relatively minor importance. Management-controlled corporations seem to be just about as profit oriented as are owner-controlled corporations” (p. 29). Larner’s conclusion states: “No fundamental differences in the level or stability of profit rates which might be attributed to management control were found” (p. 63). Also, although I noted the opposite finding on

<sup>4</sup> Michael Soref, a graduate student in sociology at the University of Wisconsin, has found a pattern of upper-class membership via clubs, Social Register listings, and preparatory-school attendance among directors of large U.S. corporations consistent with this hypothesis. Recent findings in England are also consistent with it. Whitley (1974), in his analysis of the directors of “very large industrial companies” and “large financial institutions,” has found that not only are “the ‘City’ and ‘industry’ . . . remarkably closely linked,” but also “while aristocratic kinship links do not appear to be particularly relevant for the industrial companies considered alone, they are important in connecting these companies to financial institutions and in producing a large, integrated network of the two groups combined. The directorship connections between industry and financial institutions, that is, are reinforced by, or alternatively can be seen as reflections of, kinship ties” (p. 77; see also Zeitlin, Ratcliff, and Ewen 1974).

relative profit rates by Monsen, Chiu, and Cooley (1968), I stressed Lerner's work because of its greater sophistication and reliability: it took account of critical variables and interrelations that Monsen et al. ignored, as readers can verify. It also should be noted that while Monsen et al. stressed the large differences found between the mean ratio of net income to net worth of 12.8% for "owner-controlled" versus 7.3% for "manager-controlled" firms, over 12 years, they also presented an appendix of findings on other ratios they thought "less interesting for our purposes," which they did not discuss. They found small differences between owner- and manager-controlled firms on the mean ratio of net income to total assets, 7.65% versus 6.09%, and the mean ratio of net income to sales, 5.86% versus 5.29%.

Econometric studies have in common the attempt to test theories of managerial discretion by comparing the reported differences in profit performance of putatively management- versus owner-controlled firms. However, managerial theory rests on a theory of managerial motivation, as I noted, and posits different motives for managers than for owners. "Never," as Dahrendorf has put it, "has the imputation of a profit motive been further from the real motives of men than it is for modern bureaucratic managers" (1959, p. 46). Recently, two British sociologists (Pahl and Winkler 1974) investigated this proposition. They carried out systematic in-depth interviews with directors, examined their daily work diaries, held short discussion groups with selected directors and unobtrusively observed each director of 19 firms (a "rough quota sample of British industry") for a full day of his working life in order to discover how "directors perceived and negotiated their role" (p. 102). Pahl and Winkler's findings were quite contrary to the managerial proposition. They found that "the argument that managers still operate in a capitalist market and hence must be just as oriented to the traditional capitalist goal, profit, as owners . . . was completely confirmed by our observations. . . . The professionals saw their *purpose* and their *legitimation* in improving profitability markedly compared with the last years of the family owners' management. In *all* cases we encountered, they were successful. Not only were they more *oriented* to profit, they were more capable of obtaining it" (p. 118; italics added).

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## Errata

In the review of *Japanese Blue Collar: The Changing Tradition* and *British Factory-Japanese Factory: The Origins of National Diversity in Industrial Relations* (January 1975), the first sentence of the second paragraph (p. 1033) should read:

A central issue in both books is the convergence theory of modernization, the theory predicting that societies will become increasingly similar structurally as they become highly modernized, and attempting to explain this process.

Also, on page 1034 the second sentence of the second complete paragraph should begin "Both Cole and Dore . . ." instead of "But Cole and Dore . . ."

\* \* \*

In the review of *Comparative Urban Structure: Studies in the Ecology of Cities* (July 1975, p. 218), the heading should read "Edited by Kent P. Schwirian" instead of "By Kent P. Schwirian."

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In the table of contents of the July 1975 issue, the title of the article by Morris Janowitz should read "Sociological Theory and Social Control." Also, in the references to that article (p. 107), the authorship of the three entries following "Hughes, Everett C. 1946" should have been credited to Morris Janowitz:

Janowitz, Morris, ed. 1966. *W. I. Thomas on Social Organisation and Personality*. Chicago: University of Chicago Press.

———. 1970. *Political Conflict: Essays in Political Sociology*. Beverly Hills, Calif.: Sage.

———. 1974. "Toward a Redefinition of Military Strategy in International Relations." *World Politics* 26 (4): 473-508.

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One of our readers has called our attention to an error concerning the number of square miles in Poland made in "The Course of Mother-Tongue Diversity in Nations" by Stanley Lieberman, Guy Dalto, and Mary Ellen Johnston (July 1975). The senior author has responded as follows: "Lines 10 and 11 on page 50 of the article in question should read '120 thousand' and '150 thousand' instead of, respectively, '120 million' and '150 million.' Since the boundary ratio remains unchanged at .80, none of the results are altered by this error in the text. Neither the *Journal* nor Copernicus is responsible for the error—rather it is due to the undersigned [Stanley Lieberman]."

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In the heading of the review of *The Roots of Urban Discontent: Public Policy, Municipal Institutions, and the Ghetto* (September 1975, p. 428) and in the table of contents, the name of one of the authors was misspelled Bettye K. Eidson instead of Betty K. Eidson.



## Author's Note

The *Journal* has received from Kenneth L. Wilson and Alejandro Portes the following note on their article "The Educational Attainment Process: Results from a National Sample" (*AJS* 81 [September 1975]: 343-63).

Several careful readers have inquired about the lack of correspondence of zero-order correlations presented in table 1 (p. 351) and coefficients reported in subsequent tables and graphic models. The confusion arises because correlations in table 1 are not identical to those employed for calculation of path coefficients elsewhere in the paper. The purpose of table 1 was to test for linearity of relationships by comparing product-moment correlations with corresponding  $\eta$  coefficients. To render this comparison feasible, continuous independent variables had to be categorized so as to make possible calculation of both types of coefficients on variables with the same metric. Specifically, occupational aspiration, self-assessment of abilities, academic performance, socioeconomic level, and mental ability correlations were based on the continuous variables, a fact which accounts for their generally higher value.

We are also indebted to Thomas N. Daymont for pointing out a subscript error in table 2 (p. 355). To agree with variable labels in figure 1, the subscript "7" throughout the first row of table 2 and the subscript "6" throughout the second row should be reversed. For example,  $r_{17}$  should have read  $r_{16}$  and vice versa. This is a regrettable error for which we apologize.

## Review Essay

*The Sociology of Science: Theoretical and Empirical Investigations.* By Robert K. Merton. Edited, with an introduction by Norman W. Storer. Chicago: University of Chicago Press, 1973. Pp. lx+605. \$12.50.

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The collection of papers before us is a selection from the 40-year-old Mertonian corpus, carefully chosen and clearly introduced by Norman Storer. The book is divided into five sections, each containing at least one paper which grew into a whole subdiscipline. The novelty of the approach, the erudition and elegance, and the unusual breadth of vision make this volume one of the most important contributions to sociology in general and to the sociology of science in particular.

Merton's work started when classical sociology of knowledge was at the crossroads, when the search for objective, certified knowledge had come up with empty hands while its relativistic, subjective antithesis culminated in the logical tangle of the Mannheim Paradox. On the one hand, Merton gave a helping hand to Sorokin's search for a general, all-inclusive cognitive survey on the development of science; on the other, he took issue with Alfred Weber's interpretation of culture and began to create a different kind of sociology of science in his seminal *Science, Technology and Society in Seventeenth Century England*.

It is typical of Clio's ironic sense of humor that Merton has repeatedly been accused of positivism and undue emphasis on the institutional at the expense of the cognitive aspects of science. Had Storer included some of the early Mertonian pieces from Sorokin's *Social and Cultural Dynamics* and the fascinating "Civilization and Culture" (1936), not only would the record have been corrected but the reader would also understand better why Merton's strategy was first to develop an institutional sociology of science, while some of his students today, together with him, are laboring on the more cognitive aspects. The weaving of the Mertonian fabric is such that the major papers in sociology of science must be read and absorbed simultaneously with the general theoretical sociological papers, like "Social Structure and Anomie" (1938) or "On the Sociological Theories of the Middle Range" (1957). Only then do we gain insight into the criteria of problem-choice and the strategy of research. ("Notes on Problem Finding in Sociology" [1959] would also have been a worthwhile inclusion in the book.) It is a valuable guide to Merton's procedure to realize that strategic research sites in the sociology of science were for him mainly such problems, which could be then resolved by "theories of

the middle range." This awareness allowed him to escape both arid empiricism and empty generalities of the early-20th-century *Kulturgeschichte* type. Moreover, the strategic research sites all cluster around a few themes which run like leitmotifs through Merton's work: the paradigm for sociology of knowledge, the ethos of science, and the reward system of science which follows from it.

According to Merton, the social context in which sociology of knowledge developed was the growing distrust between groups and a tendency to discount the face-value contents of statements. It is a context in which one "no longer inquires into the content of beliefs and assertions to determine whether they are valid or not" (p. 9) but instead introduces an entirely new question: how does it happen that these views are maintained at all? *Wissenssoziologie* shares this approach with "psychoanalysis, Marxism, semanticism, propaganda analysis, Paretanism and to some extent functional analysis" (p. 9).

The breakthrough came when it was realized not only that error and uncertified opinion could be accounted for at least in part by extratheoretic considerations but also that the discovery of truth was socially conditioned: "The sociology of knowledge came into being with the single hypothesis that even truths were to be held socially accountable, were to be related to the historical society in which they emerged" (p. 11). This is a very strong thesis. It came to underlie the research programs for most social sciences, including large parts of anthropology and some of psychology. It introduced separation between the study of the natural sciences and their study on the metatheoretical, sociohistorical level. It separated historians of science from sociologists of science for three decades. It is reflected in Charles Gillispie's pregnant remark in his review of Merton's book in *Science*: "[referring to Merton's 'Priorities in Scientific Discovery' in 1957] I wrote, that... I did wonder whether the matter wasn't a bit trivial.... Only a few years later, when I began to study and teach materials in the social and institutional as well as the more traditional internal and intellectual history of science, did I come to take the full thrust of what he had in fact said and said clearly and convincingly" (*Science* 184 [1974]: 656).

The above thesis also became the core of the Mertonian conceptual framework in which most of his students and critics saw his greatest contribution. In order to provide a basis for taking an inventory of extant findings in the field, Merton proceeded to develop a scheme of analysis. He entitled his presentation of it a "Paradigm for the 'Sociology of Knowledge'" (1945!).

This paper, the first in the volume, is central to the whole Mertonian world view. Merton's major theoretical step was to demarcate mental productions from their existential base. Having done that, the paradigm then

consisted of concentrating on five questions, the first three of which are: (1) where is the existential basis of mental productions located? (2) what mental productions are sociologically analyzed? (3) how are mental productions related to the existential basis? These questions are broken down into different categories which are then utilized to illuminate the theories of Marx, Durkheim, Weber, Scheler, Mannheim, and Sorokin. The analysis shows how well the tool fitted sociological theories of knowledge. It does not address itself directly, by Merton's chosen strategy, to the connection between the social context and the changing content of science. As Merton himself put it: "Much remains to be investigated. . . . the shifting role of the intellectual and the relation of these changes to the structure, *content* and influence of his work require growing attention" (p. 37; my italics). For Merton, this was a programmatic statement for the future. But 30 years later, standing on his shoulders, we can detect in the original paradigm also the beginnings of greater emphasis on the contents of science.

To give an example: one approach would demand a functional explanation for the Hopi rain dance. The explanation is that it serves as a vehicle of social cohesion. The other approach would demand a Hopi theory of how, according to them, a rain-dance brings down the rain. The images of knowledge in their explanation would then have to be related to the given sociocultural milieu, and the implications of this explanation for the functional explanation sought.<sup>1</sup>

From the Mertonian paradigm emerges quite naturally another major theme: the ethos of science. Having asked for the existential base of mental products, and having suggested an analytical scheme of dealing with it, one must now ask: what are the behavioral norms which regulate the institutional life of the academic community—norms that, according to the paradigm, are independent of the mental productions themselves. The Mertonian norms are universalism, communism, disinterestedness, and organized skepticism (the original four), and a fifth, ethical neutrality. This value system is the "gatekeeper" of the validity of scientific activity. The Mertonian paradigm and ethos are closely connected with historical re-

<sup>1</sup> It might perhaps be worthwhile, instead of demarcating mental productions in general from their existential base, to introduce a distinction among different kinds of mental productions. First, there are those included in the body of knowledge: ideas about the world or society or the individual, depending on the universe of discourse. These are not purely ideal, objective, time-independent and culture-independent Popperian third-world mental productions; the fact that these and not other mental productions are temporarily considered "objective" is contingent on a second, different kind of mental production: ideas about knowledge—its sources, frontiers, legitimacy, methods of verification or falsification, applicability, utility, progressiveness or decline, its power or its moral fiber! Images of knowledge determine the relations between ideas in the body of knowledge and in a sense belong to their existential base. Finally, yet other mental productions are ideologies, normative belief systems, and political credos.

search by Merton and others which has been striving to trace the norms and values to the 17th-century scientific revolution. This ethos has been much admired and much criticized, and within the sociology of science Mertonians and dissenters have emerged.

The norm most challenged has been that of disinterestedness; yet it is important to note that most critics of Merton have understood disinterestedness as a subjective, individual norm and have rightly argued that scientists qua individuals do not conform with this norm. One should remember that this is not Merton's meaning. For him, disinterestedness, like other norms, is institutional and not individual. "Science as is the case with the professions in general includes disinterestedness as a basic institutional element," writes Merton (p. 275). Thus, what results from the norm is institutional pressure of evaluation and reward; this is what contributes to the pursuit of the extension of certified knowledge.

The Mertonian conception of institutional norms is intimately connected with his emphasis on the rational, empirical nature of science. A very important problem is the study of other norms—like metaphysical coherence, aesthetic completeness, and compliance with images of knowledge—which are less precise and which may very often clash with the previously mentioned norms, especially those of organized skepticism and universalism. If the theory of the growth of knowledge by accumulation, or, alternatively, a Kuhnian theory of the growth of knowledge by revolutions, were replaced by a theory which views the dynamics of scientific change as a continuous critical dialogue between competing scientific metaphysics and their theories (that is, competing paradigms) and between competing images of knowledge, perhaps these other norms could be included. This, however, would imply a looser view of science than the one underlying the Mertonian norms.

Related to the ethos of science is the reward system of science. This has preoccupied Merton from his very early papers, and it interacts in a lively way with all the other themes. Involving the problem of priorities in scientific discovery or in a related formulation, the reward system of science is rooted in the ethos of science. The problem Merton has had to grapple with is the mechanism which keeps the norms of science valid. An understanding of the dialectical tension between the normative structure of science and its institutional complement in the reward system of science is required to explain why scientists abide by the norms of universalism, communism, disinterestedness, and organized skepticism. The priority disputes, or the frequent simultaneous discoveries, exhibit how indispensable a system of institutional recognition of scientific achievement is and yet how dysfunctional it can become. True, there is no attempt to question whether discoveries held to be multiple are indeed so in the body of knowledge; very often, different questions differently an-

swered turn out to be simultaneous discoveries only by hindsight. This introduces a small problem: a new question to be faced is, why do scientists choose problems which are related enough to be conflatable into a simultaneous discovery by hindsight? For an answer, one must look to the reward system of science as a social institution.

These themes, and the several others which are interwoven with the Mertonian fabric, illustrate the strong thesis of functional sociology of science. But alongside this, one discovers the beginnings of a historical sociology of scientific knowledge. According to this other thesis, all knowledge, certified or not, has to be taken first at face value—this is on a cognitive level—and only then related to the historical milieu. The interpretation given in the body of knowledge has to be related to its social context. In the later papers of this book, such emphases become more and more frequent. It is not accidental that in the last essay in the volume, "Age, Aging and Age Structure in Science" (written with Harriet A. Zuckerman in 1972), the last sentence is: "That aim [scientific investigation of age] is to find out how age and age structure variously interact with the cognitive structure and development of science" (p. 559).

Merton's *Sociology of Science* is both a magisterial summary of the field up to 1973 and the source of a new historical sociology of scientific knowledge which combines a sociological study of the contents of science with a study of the institutional setting of science.

## Review Essay: On Boudon's Model of Social Mobility<sup>1</sup>

*Education, Opportunity, and Social Inequality: Changing Prospects in Western Society.* By Raymond Boudon. New York: John Wiley & Sons, 1974. Pp. xxvii+220. \$12.50.

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In the 30 years since World War II, participation in higher education has grown dramatically in Europe and in North America. What does this growth imply about inequality of access to education among socioeconomic classes? Are rates of occupational or social mobility across generations altered by increasing education and changing patterns of access to post-secondary schools? How do changing opportunities affect the educational goals of youth? These are the major questions posed by Raymond Boudon in *Education, Opportunity, and Social Inequality*.

Not since the publication of Jencks's *Inequality* has a book so clearly captured the interest and attention of students of social stratification. At the Eighth World Congress of Sociology in Toronto, the hallways fairly buzzed with favorable anticipation. And in S. M. Lipset's laudatory foreword, we read, "In this volume, sociological theory comes of age" (p. vi). For these reasons, I read *Education, Opportunity, and Social Inequality* with high expectations.

Boudon states the book is an "attempt to synthesize the main findings accumulated by empirical research" (p. xi) about inequality of educational opportunity (IEO) and inequality of social opportunity (ISO), which the author defines as differentials by social background in educational attainment and in social achievement, respectively. The distribution of schooling

ED. NOTE.—A comment by Raymond Boudon on this review essay has been solicited by the book review editor and is scheduled to appear in the March issue of this *Journal*.

<sup>1</sup> This work was supported in part by the Division of Social Systems and Human Resources, RANN-NSF (grant no. GI-31604X), by the National Institute of Mental Health (grant no. MH-06275), by funds granted to the Institute for Research on Poverty at the University of Wisconsin, Madison, by the Department of Health, Education, and Welfare pursuant to the provisions of the Economic Opportunity Act of 1964, and by the Center for Demography and Ecology, National Institute of Child Health and Human Development (grant no. HD05876). Peter J. Dickinson, Harry P. Travis, and Neil D. Fligstein helped in the preparation of this review. Seymour Spilerman, David L. Featherman, Hal H. Winsborough, Nathan Keyfitz, Ross M. Stolzenberg, Paul M. Siegel, and William H. Sewell were helpful critics. The opinions expressed herein are those of the author.

is determined endogenously, that is, within the process of mobility, by the desire of youth to attend college. At the same time, the social structure (distribution of achieved statuses) is fixed exogenously by technological and organizational forces which are unrelated to socialization and social selection through the schools. "An individual cannot create a job just because he wants it, but he can go to college if he wants to, provided he is qualified" (p. 21). Thus, there is a potential disjuncture in the flow of persons from schools to jobs.

With rising aspirations, educational attainment has increased in Western societies, and concomitantly, inequality of educational opportunity has decreased. However, growth in the number of high-status positions has not been commensurate with growth in the supply of educationally qualified persons, so the occupational chances of persons at some levels of education must fall. Those with the very highest levels of schooling retain their traditional advantage, and those at the bottom of the educational distribution are not affected because they never did enjoy much of a chance to enter high-status positions. Since something has to give, occupational opportunities decline substantially at intermediate levels of schooling. Consequently, potential status gains through increased schooling are immediately lost through declining occupational opportunities, and the pattern of social mobility between generations remains undisturbed. Thus, Boudon explains the apparent stability of occupational mobility regimes within and across Western nations, even where there are significant variations in levels of educational attainment and in access to education. Another outcome of the process is pressure on middle-class youth to stay in school longer, so they can continue to compete for high-status jobs. This explains why middle-class youth have higher aspirations than lower-class youth, and it leads to a temporal spiral of ascending aspirations and attainments. The main policy implication which Boudon draws (and with which I agree) is that reductions in social and economic inequality are not likely to be accomplished by such indirect means as the equalization of opportunities for schooling.

Boudon's thesis is seductive, for it promises to integrate a theory of cross-sectional class differentials in aspiration and attainment with global shifts in educational and occupational distributions. And its appeal is timely, corresponding as it does with a depressed labor market. Moreover, the support for Boudon's thesis looks impressive, since it includes extensive calculations based on a model which simulates mobility processes, and both the assumptions and implications of the model are assessed and evaluated by reference to a variety of North American and European data.

Unfortunately, this is more appearance than reality. The premises of Boudon's thesis prove to be sociologically naive, and the argument lacks cogency. The relationship between evidence and conclusions is often weak,



is sometimes artifactual, and in a few instances is contradictory. The analytical and observational evidence is frequently flawed by errors of fact, of method, and of logic. In reading the book, I was reminded of Mark Twain's (Clemens 1961, p. 120) observation: "There is something fascinating about science. One gets such wholesale returns of conjecture out of such a trifling investment of fact."

The lead chapter of the book elaborates the supposed paradox whereby the relationship between intergenerational educational mobility and occupational mobility may be weak, even where the effect of class origins on schooling and the effect of schooling on occupations are strong. Of course, there is no paradox here; one simply cannot think about relations among mobility variables and among achievement variables in the same way. This issue is treated exhaustively by Blau and Duncan (1967, pp. 194-99) without the erroneous suggestion—found in Boudon's book (p. 14)—that the "paradox" can be associated with discrepancies between the educational and occupational distributions. In any case, instead of a formal demonstration of the paradox, Boudon offers only a numerical example based on fictitious data.

Chapters 2-6 focus on inequality of educational opportunity. The first of these is a brief review of theories of educational inequality. Both socioeconomic background and school achievement (or ability) affect educational outcomes, and Boudon says this discredits "cultural" theories of class differences in educational aspiration and attainment. In discussing theories of class-differentiated achievement values, Boudon refers to Hyman's influential but outdated (1953) paper "The Value Systems of Different Classes," but he never states the "value" theory clearly or completely enough to draw any falsifiable implications from it. He does not mention any of the several unsuccessful efforts to represent the operation of class values as variables intervening between social background and aspirations, nor does he refer to *any* of the recent developments of multivariate causal models of educational aspirations and attainments. A concluding statement reads like unintended irony: "My contention is that we may gain more insight . . . if we try to devise a theory accounting for a wider range of data" (p. 36).

How does Boudon reject the value theory? He argues that there is an important interaction effect among social background, academic achievement, and educational outcomes, such that the effect of ability is inverse to social background. The "value theory" does not predict this interaction effect, so Boudon rejects it in favor of a rational decision-making model. The latter explains class differences in aspiration and attainment by postulating that the benefits of staying in school are higher and the costs lower for higher-status than for lower-status persons. Boudon says that this theory does not account for the interaction effect any better than does the

theory of class values but that it could be modified to accomplish this end. Boudon's theory of educational inequality is simplistic, if it is not tautological. For example, if social origins influence adult social standing above and beyond their effect by way of education, the opportunity costs of remaining in school are greater for persons of socially favorable than of lowly origin. That is, Boudon's assumptions may be invalid because social background affects adult standing directly, and not just by way of schooling.

But perhaps the most disturbing aspect of this chapter is its erroneous analyses of data. For example, consider Boudon's table 2.1, taken from Joseph Kahl's influential paper, "Educational and Occupational Aspirations of 'Common Man' Boys" (1953; the figures from Kahl's paper are those shown without parentheses in table 1). Boudon writes:

The most interesting feature of the table is that as father's occupational status increases, the effect of IQ on the level of aspiration decreases. Thus the first row of the table indicates that 56 percent of the low-IQ upper class youngsters want to go to college, as opposed to 89 percent of the high-IQ group. Among youngsters whose fathers are in skilled manual occupations, the figures are 4 percent for those with low IQ, and 40 percent for those with high IQ. However, the difference is smaller (29 percent vs. 9 percent) when fathers are in unskilled manual occupations. This interaction effect of IQ and social status on level of aspiration is still more

TABLE 1  
PERCENTAGE OF BOYS WHO EXPECTED TO GO TO COLLEGE BY IQ AND FATHER'S  
OCCUPATION, BOSTON AREA, 1950

FATHER'S OCCUPATION	IQ (QUINTILES)					All Quintiles
	1 (Low)	2	3	4	5 (High)	
1. Major white collar .....	56* (57)†	72 (70)	79 (77)	82 (81)	89 (91)	80
2. Middle white collar .....	28 (27)	36 (38)	47 (47)	53 (54)	76 (74)	52
3. Minor white collar .....	12 (12)	20 (19)	22 (25)	29 (30)	55 (52)	26
4. Skilled labor and service ....	4 (7)	15 (12)	19 (16)	22 (20)	40 (38)	19
5. Other labor and service ....	9 (5)	6 (8)	10 (12)	14 (15)	29 (30)	12
All occupations .....	11	17	24	30	52	27

\*  $N = 3,348$ . Source is Kahl (1953).

† Parenthetical entries are predicted percentages in an unweighted, additive logit model.

noticeable if we examine the differences in aspiration between the third and the fifth IQ quintiles. [P. 25]

Reading down the table, and comparing the first and fifth IQ quintiles, we see percentage differences of 33, 48, 43, 36, and 20. The effect of IQ on aspiration increases as we move from the highest to the next highest category, and thereafter it decreases regularly. What about the comparison between the third and fifth quintiles? Here the percentage point differences are 10, 29, 33, 21, and 19, increasing, to be sure, between the highest and the middle socioeconomic category, but decreasing thereafter. Certainly Boudon's conclusion that "the sensitivity of the aspiration level with respect to IQ is roughly greater, the lower the social status" (p. 26) does not square with the evidence.

The author goes on, "This interaction effect appears still more clearly when ratios rather than differences are used" (p. 26), and we are told to take ratios of the percentages aspiring to attend college within each social background category, rather than percentage point differences. Boudon erroneously calls these ratios of percentages "odds-ratios." For proper examples of odds-ratios, see Goodman (1969). Returning to table 1 and computing ratios of percentages, we find greater, but far less than perfect nominal support for Boudon's hypothesis. More important, the procedure is patently wrong, for it confounds the effect of IQ within categories of social background with that of social background itself. That is, the average aspiration level is higher among boys with more favorable social background, so there is a ceiling effect on the ratios of percentages which induces them to vary inversely with social standing. To make this clear, try to make the same argument using ratios of percentages in the complement of table 1, that is, a table of percentages of boys *not* expecting to go to college.

How should the table be interpreted? By inspection we see there is a wide range of percentages in the table, from fewer than 10% to almost 90% expecting to go to college. Thus, there may be ceiling or floor effects in extreme categories, and that alone could account for the variations in percentage differences across categories of father's occupation. To test this notion, I fitted the percentages in the table using an unweighted logit model. The details of the logit transform are not important here, except to say it eliminates floor and ceiling effects (Theil 1970). The model I used had only row and column effects; that is, the effect of each category of father's occupation was constant across categories of IQ, and vice versa. The percentages predicted by this model *without interaction effects* are shown in parentheses below the observed percentages (table 1). It is easy to see the correspondence is rather close; over the 25 cells of the table the correlation between observed and predicted percentages is .997. The reader may verify for himself that the pattern of predicted percentage point

differences in the additive model is virtually identical to that in the original table.

This is possibly a tedious and trivial example, but I have developed it in detail because it is representative of the analyses of secondary data throughout *Education, Opportunity, and Social Inequality* in two important respects: the correspondence between table and text is weak, and the interpretation of the table, where it is not inaccurate, tends to confound main effects with interactions and to depend heavily on arbitrary choices between absolute and relative measures of effect.

Boudon makes no claim to comprehensiveness in his review of secondary data. On the contrary, he explicitly disavows such ambition, saying his review is "sketchy" and that it is "beyond the scope of this book to present a complete review of the extensive literature dealing with IEO" (p. 22). Without attempting to reconcile such disclaimers with the avowed purpose of the book, we may say the author is as good as his word on the issue of coverage. It is easy to point to well-known and readily accessible published data which give even less support than Boudon's tables to his hypothesis (e.g., Sewell and Shah 1967 or Sewell, Haller, and Strauss 1957).

The third chapter of the book reviews trends and cross-national differentials in educational attainment and inequality of educational opportunity. The exposition relies heavily on compendia of educational statistics provided by the Organization for Economic Cooperation and Development (OECD) for a 1970 conference on educational growth. Here we find doubtful demographic assumptions, for example, that "the male population 45 to 54 years old includes most of the fathers whose sons are [college] students at that time"; that "fertility is nearly the same for all social groups" (p. 43); that comparisons of school attendance rates across nations require the use of "age-group categories" which are "*necessarily* arbitrary" (pp. 41-43, italics in original); that age-specific school enrollment rates of 14-19-year-olds can be interpreted as "the survival rate from, say, the last elementary school year to secondary  $X_k$ th grade, that is, the grade attended by youngsters on the average [that many] years old" (p. 59).

After describing a table of grade-continuation ratios for several cohorts in Hesse, Boudon writes, "I have not been able to discover any [other] tables . . . that would give the progression of yearly survival rates for a series of cohorts" (p. 58). As is well known, to construct historical tables of progress from grade to grade, however, one needs only a classification of age by educational attainment for cohorts old enough to have completed school. For example, see Beverly Duncan (1968, especially tables 9, 18, 27, 28).

One conspicuous flaw is the treatment of trends in access to education in the United States with respect to socioeconomic background. In this

chapter, Boudon neither presents the evidence from nor refers to U.S. time series, such as those based on the 1962 Blau-Duncan survey, "Occupational Changes in a Generation" (OCG), which are found in papers by Beverly Duncan (1967, 1968), O. D. Duncan (1968), and William Spady (1967). All of these source publications say there has been essentially *no* trend, except Spady's paper, which uses methods akin to those of Boudon and concludes inequality of educational opportunity has been decreasing in the United States at the point of entry into high school, and *increasing* in respect to college entrance and graduation. Yet Boudon writes, "the case of the United States suggests the hypothesis that a general overall increase in school attendance rates may have the consequence of reducing IEO" (p. 48). Without offering a single datum, he criticizes Alain Touraine, "whose conclusion on the stability of IEO in the United States contradicts all available data" (p. 64). To bolster this criticism Boudon cites Jencks's *Inequality* (1972), but after a careful search I have not been able to find a single passage in Jencks's text which supports Boudon.

Chapters 4-6 describe, analyze, and interpret Boudon's dynamic model of unequal educational opportunity. The model is "dynamic" because it simulates the temporal process by which the educational attainment distribution of a cohort is generated, and because rates of attrition vary systematically among cohorts. Each cohort leaves grade school with the same (positively correlated) joint distribution of "social background" (three categories) by "school achievement" (three categories). At each of eight "branching points" up to college graduation the joint distribution is subjected to a single set of social background- and school-achievement-specific survival rates. That is, the survival rates vary by background and achievement but not (for any one cohort) from one branching point to the next. Attrition is irreversible; once having left the "higher curriculum," no one returns to it. In accord with the claims in chapter 2 there is *really* an interaction effect of social background and school achievement. Finally, between each of four successive cohorts, survival probabilities at each branching point are increased by 10% of the absolute difference between the current survival probability and unity. Since this procedure increases initially low probabilities faster relatively and absolutely than high probabilities, there is a gradual convergence in the probabilities among the background-achievement subgroups, that is, a nominal reduction in inequality of educational opportunity.

The major analyses based on the model are a comparison of the educational distributions among the four cohorts, a test of the sensitivity of the distribution in the first cohort to an increase in the school achievement of the lower background groups to equal that of the upper group, and a comparison of some properties of the model educational distributions with those described earlier in the text. Boudon's conclusions (pp. 109-14, not

in this order) are sweeping: (1) "The general consequence deriving from the foregoing analysis is that society rather than school is responsible for IEO" (p. 114); (2) inequality of educational opportunity is inevitable so long as societies are stratified or have differentiated school systems; (3) while increasing educational attainment may partly be a consequence of "technological progress and economic change," the aggregation of changes in individuals' demands generates "an overall change whose structure can be unrelated to economic change or to change of the social structure" (p. 113); (4) persistent inequality of educational opportunity may be consistent with drastic changes in the social composition of the student population; (5) the results confirm Boudon's theory of educational decision making and disconfirm "value theory"; and (6) social background has only a minor "primary" effect on educational attainment by reason of cultural inequality (differential school achievement), while its "secondary" effects are "exponential."

Some of these conclusions may be correct in spite of faults in Boudon's efforts to confirm them. In no way does the validity of the first three depend on the outcomes of the model. At most they are restatements of some of Boudon's assumptions, and as we shall see, none of the assumptions was verified empirically. Several (if not all) of Boudon's major conclusions rest on the logical fallacy of affirming the consequent: if A, then B; we observe B, whence A must be true. The fourth conclusion is a trivial outcome of increasing educational attainment. The fifth conclusion is unrelated both to Boudon's assumptions and to his analyses. Only the last conclusion, which is partly correct, depends in any important way on Boudon's analytic efforts.

The reader should not take Boudon's model of educational opportunity as representative of the state of the art in demographic models of the educational sector. Doubters may wish to consult Richard Stone's lucid monograph (1971) or the recent OECD survey of more than 100 *Mathematical Models for the Educational Sector* (1973). Boudon's model suffers the disadvantages of simulation without exploiting its advantages. Many of its properties could be deduced analytically, yet except for a brief appendix on exponential decay (pp. 103-5), Boudon eschews analysis in favor of arithmetic.

The simplicity of the model invites sensitivity or response surface analysis, that is, exploration of the effects of systematic variation in parameters. Yet, with the exceptions already noted, Boudon rests his analysis on one set of arbitrarily designated parameters. He explicitly rejects the response surface technique:

Rather than trying to explore the behavior of the model throughout the parametric space, I have selected particular points. The criterion . . . is simply that of sociological relevance: the points are chosen in such a way

that the system they generate can be considered to be portraying a potential Western society . . . an ideal-type society rather than . . . an actual Western society. . . . It is assumed the particular values chosen for the parameters have no effect on the conclusions. [P. 69] . . . And it is obvious that these general properties would also have been generated by other sets of parameters, provided the values given to the parameters were in agreement with the basic axioms. [P. 77]

It is clear that the model and parameters are unrealistic, for example, in respect to the interaction effect discussed above and the constancy of continuation rates across branching points. (We shall shortly take up the realism of model outcomes.) It is not clear how one knows a model is insensitive to variation in its parameters without varying them. If empirically verifiable outcomes of the model were invariant in this sense, one might begin to suspect they were trivial, and in no case would such findings bear on the validity of implications which do vary with parameters of the model. Again, recall the fallacy of affirming the consequent.

Boudon also rejects the possibility of using real data to estimate the parameters:

It is clearly impossible to build a system- and individuals-dynamic model and at the same time to fit it to any set of actual data. . . . An empirical estimation of its parameters is impossible, given the present stage of our information. [Pp. 68-69] . . . The model defines a fictitious system that certainly does not describe adequately any particular existing system. But the statements that define it—both the general relationships stated in the basic axioms and the numerical values given to the parameters in the auxiliary axioms—can be considered to be realistic, even though no attempt has been made (nor could be made) to derive the values from empirical estimations. [P. 76]

Boudon says that all of the parameters of his model for any one cohort can be estimated from any of several European panel studies (p. 68), and there is certainly no dearth of similar data in the United States. The problem, as Boudon correctly indicates, is the lack of sufficiently detailed time series, that is, observations on successive cohorts. But does that problem justify ignoring the available data? Should not Boudon have started with observations on a real cohort and then varied some of the parameters in correspondence with hypothesized shifts in educational attainment and inequality of opportunity? Temporal variations in unobservable parameters might sensibly be made to correspond with variations in parameters whose time paths are known (see Stone 1971, pp. 113-19). Or might not Boudon have chosen to contrast the behavior of the model across the several societies where parameters could be estimated for real cohorts? Any of these alternatives would have been better than the one he chose.

Boudon validates the educational distributions generated by his model against "school bookkeeping data." For example, the validated properties

of the model include increasing rates of school attendance; larger rates of increase in the extreme upper tail of the education distribution than toward the center; larger absolute increases toward the center of the education distribution than in the upper tail; and various permutations of these observations with comparisons among social background categories. One must agree with Boudon about the insensitivity of his model to variation of its parameters, for it would be hard not to generate such "findings" with *any* model of growth in educational attainment.

Let us consider other features and implications of the model. We have already noted the unrealistic constancy of survival probabilities across branching points. The time path of those probabilities (across cohorts) is also unrealistic; their growth has a ceiling effect but no floor. That is, one would expect growth to follow a logistic path with small and accelerating absolute increases where the base rate is low. But Boudon assigns the largest absolute rate increases to the lowest initial rates. Moreover, the ceiling and point of convergence of all rates is unity, no matter what the initial value. If one takes the hypothesized time path of the model seriously, inequality of educational opportunity *will* disappear eventually. It is an interesting contrast that in the United States the continuation rate from high school to college has fluctuated erratically about 0.5 during the past half-century of rapid growth in educational attainment.

Boudon notes repeatedly and emphatically that the effect of social background on educational attainment by way of school achievement is not large and that it tends to fade with the passage of time, while the secondary (direct) effects of social background persist: "This differential evaluation of the primary (cultural) and the secondary effects of stratification may result in large measure from what we can call a *cross-sectional illusion*. By its nature a cross-sectional survey is unable to show that the primary effects necessarily become attenuated over time, whereas the secondary effects are exponential" (p. 111; italics in original). How is this conclusion supported? Boudon equates the initial school achievement distribution of the two lower social categories with that in the highest, but the disparity in educational attainments between the high and low social categories is not thereby eliminated.

Boudon says this occurs because the achievement distributions of the social categories are gradually equalized by the attrition process. Also, "If [lower-class youngsters] are relatively brighter, . . . they are as likely to survive beyond a given branching point as are higher-class students" (p. 85). The latter explanation is falsified by Boudon's characterization of class differences; his parameters do not give the brightest lower-class child the same chance to continue in school as the dullest upper-class youngster. The former explanation is correct, and Boudon understates its importance, for his choice of parameters forces the correlation between social back-



ground and school achievement to become negative at an early stage of the attrition process, and the correlation takes on a larger negative value at each successive branching point. For example, in the first cohort, lower-class survivors are higher achievers than those in the middle category after the third branching point, and they are higher achievers than those in the top background category after the fifth branching point. That is, after a time the "cultural" effects of social background do not just disappear in Boudon's model; they become obstacles to the attainment of high-status youngsters!

How realistic is this aspect of the model? It is not clear what Boudon intends by "school achievement," but from his choice of secondary data in chapter 2, one might think it proper to look at correlations between mental ability and socioeconomic status for persons at varying levels of school. In Sewell's Wisconsin data there is a positive relationship between socioeconomic status and ability among the 18% of the sample who graduated from college; even among the 5% who entered graduate school the relationship is not negative.

One other factor which Boudon did not mention has a bearing on the rapid dissipation of "cultural" effects of social background. Given his parameters, those effects were not very large even at the beginning of the attrition process. For example, at the first branching point, 0.4 more of the upper-class than of the lower-class students survived, but the (un-weighted) average difference in survival rates between upper- and lower-class students within achievement levels was 0.35, and the class difference was larger (0.45) than the marginal difference (0.40) within the low achievement level.

One might have expected Boudon to validate the "cross-sectional illusion" against empirical data, given its importance as an implication of his model. Yet no evidence is offered of the importance or of the decay of "cultural" effects in any real cohort. If we look at Sewell and Shah's (1967, p. 17) path analysis of educational attainment in the Wisconsin cohort, we find among more than 4,300 males that the ability-background correlation accounts for 24% of the association of background with college plans, 26% with college attendance, and 29% with college graduation, and it accounts for 24% of the association of background with college graduation among college attenders. Among 4,600 women the corresponding percentages are 16, 16, 22, and 28. Moreover, both for men and women the direct effects of socioeconomic background are smaller in the case of college graduation than in the cases of college plans or of college attendance. In short, the Wisconsin longitudinal data give absolutely no support to the implication of Boudon's model that there is a "cross-sectional illusion" in the "cultural" effects of socioeconomic background.

Finally, we may ask whether inequality of educational opportunity

actually declines across the four cohorts which Boudon carries through his model. Nominally, of course, there is such a reduction, since the survival rates at successive branching points increase more rapidly for initially low than for initially high values. But over several branching points the import of a given absolute rate increase is smaller when the initial rate is low than when it is high (pp. 87-90). Boudon says there is increasing equality of educational opportunity (p. 102), but the data are equivocal. While the ratios of graduation rates between the upper and lower social categories decline over time, the absolute differences between rates of college graduation increase over time.

Since Boudon feeds the outputs of his educational model into his occupational model, it is a matter of some analytic importance that the model clearly show declining inequality of educational opportunity—yet this does not appear to be the case. For example, I contrasted the education distributions of Boudon's upper and lower social groups in the first and fourth cohorts. If we merely look at the mean number of branching points completed, the difference between the extreme social groups is actually larger in the later than in the earlier cohort. Moreover, there is more variation in schooling in the later than in the earlier cohort in both the upper and lower social groups. In short, the main results of Boudon's variation in survival rates across cohorts are an upward shift in the educational distribution for everyone and an increase in its dispersion. Boudon's fictitious data do not show substantially more equality of opportunity in later cohorts, and by some measures they show less.

Chapters 7-10 of *Education, Opportunity, and Social Inequality* provide an exposition, analysis, and validation of Boudon's model of inequality of social opportunity (intergenerational occupational mobility) which parallel the treatment of educational opportunity. The model takes the four schooling distributions generated by the educational opportunity model, and it allocates persons at each level of schooling to an "achieved social status," subject to the constraint that the distribution of achieved social status is the same as that of social background. Since schooling increases across the four cohorts, this constraint represents the notion that occupational structures are insensitive to increases in the supply of educated persons.

While Boudon "intuits" what the consequences would be of an upward shift in the distribution of occupational positions (p. 157), he never uses his model to demonstrate them. Rather, he argues, "the pyramidal shape of the social structure will cause the *probabilities* of moving upward to remain smaller, on the whole, than the probabilities of moving downward" (p. 157, italics in original). Moreover, if the shape of the occupation distribution did shift, Boudon says, "the meaning of occupational categories with respect to status" would change over time. "From a sociological standpoint, it is much more reasonable to assume that the status ranking

of occupational categories depends somewhat on their distribution" (p. 157). This statement is in striking contrast with the well-known lack of variability in occupational prestige ratings over long periods of time within a single society and across societies with the most divergent social structures (Hoëge, Treiman, and Rossi 1966; Hodge, Siegel, and Rossi 1966).

The assumption that the distribution of social positions is constant, or nearly so, is repeatedly emphasized in Boudon's argument. In his view it is largely responsible for the secular increase in the demand for collegiate education (p. 149). Yet there is no documentation of stability over time in the occupational structures of Western nations. Indeed, only one table (3.10, p. 55) in the book is relevant to temporal shifts in occupation distributions. It gives occupation distributions of Dutch men in 1954–55 and 10 years later, but its possible relevance to occupational change goes unmentioned.

Boudon introduces a "box model" to allocate persons with differing social background or educational attainment to achieved social statuses. This is how the model works. Fill up positions in the social structure in sequence from the top down, giving priority to the most qualified or highest status persons. Follow the rule that for each cell of the table a parameter of the model (which varies between 0 and 1) is applied as a conditional probability to the remaining row sum (i.e., the number of unallocated candidates) when the remaining row sum is less than the remaining column sum (i.e., the number of unfilled positions) and applied as a conditional probability to the remaining column sum when it is less than the remaining row sum. Sometimes a single value of this parameter is applied in all of the cells of a table, and at other times the values vary over cells, giving rise to a pattern of "dominance." Boudon says the logic of this procedure is "obvious" (p. 9) and that it "attempts to relate closely measurement to sociological theory" (p. 139). To this reader, at least, neither of these points is "obvious," and the text does not elaborate them at all.

Boudon dismisses several standard representations of the mobility process as "basically statistical" (p. 139). I can only guess what this means—perhaps that they are rich in formal properties or that sampling distributions of their parameters are known. Neither of these properties strikes me as undesirable, and these models do have coherent and intuitively meaningful interpretations relative to the mobility process.

The "box model" does not appear to deserve wider application. In the book it is used both to generate occupation distributions from fictitious educational and social background data and to describe actual mobility data. In both instances, the pattern of "dominance coefficients" is bizarre. They supposedly vary directly with the social standing of the cell to which they apply, but the coefficients necessarily show a U-shaped pattern,

high in the highest *and* lowest status cells, and lower in cells of intermediate standing (pp. 130–31, 155). The model lacks symmetry, in that different results are obtained if one proceeds from bottom to top, that is, filling the least desirable positions with the lowest status persons, rather than from top to bottom. Moreover, where one is filling in a classification of more than two dimensions, for example, a table of occupation by schooling by social background, it is necessary to assume some order of priority among the combinations of antecedent variables which are only partially ordered. For example, Boudon always assumes that education takes priority over social background, so a lower-class high school graduate stands in line before an upper-class high school dropout. In these ways, the “box model” is sensitive to the order in which cells are filled as well as to its nominal parameters.

Last, the “box model” gives sociologically implausible numerical results in Boudon’s application. Regardless of social background, in most instances the model gives persons in the lowest educational category a higher probability of entering the highest social status and a lower probability of entering the lowest social status than persons at the next higher educational level. Further, in the lowest educational category, persons of middling social background have a smaller chance of entering the lowest social status than do higher-status persons (table 8.7, pp. 158–59).

Applying the “box model” to schooling distributions or to schooling by background distributions, Boudon ultimately generates tables of social background by achieved social status. Since educational attainment increases across cohorts, while the distribution of achieved statuses is constant, education-specific occupational chances tend to fall. The absolute decline in chances for high status is least among the most and least educated, in the former case because the box model gives the educational elite priority in access to valued positions, in the latter because very few of the poorly educated persons achieved high status under the original conditions. Persons in the middle of the educational distribution suffer the largest reductions in chances to achieve high status. It is not clear how dependent this result is on the peculiar assumptions of the “box model.” However, this is the evidentiary basis for Boudon’s conclusion that “over time changes in the structure of status expectations associated with the various educational levels . . . explain the empirical finding that, as a rule, higher education is highly valued by youngsters and families of middle as well as high status, whereas it appears to be much less often desired by lower class families” (p. 149; also n. 2, pp. 162–63).

It is not at all clear how this argument shifts from findings about the middle of the *education* distribution to implications about differential impact on social background groups. Boudon never measures the impact of changing education-specific occupational chances on the social mobility

of the members of each social background group. To gain some insight into this, I undertook a comparison of extreme situations in Boudon's model. Suppose the persons at each educational level in the first cohort had the same occupational chances as members of the last cohort (table 8.3, p. 146, and table 8.4, p. 147). How would mobility change, and how would those changes be distributed across the social background groups? As one might expect, comparing my hypothetical mobility table with Boudon's results in the first cohort (table 8.5, p. 152), Boudon's results show more entries into high-status positions and fewer into low-status positions. However, the impact of this comparison of extremes is not very large, and it is not very different across the social background groups. The shift in occupational chances increases the percentage of high-origin persons in low-status positions by 8.0 points and lowers the percentage in high-status positions by 7.9 points; in the middle background category the corresponding percentages are 9.4 and 6.3, and in the low background category they are 7.7 and 3.3. Considering the extreme choice of comparisons, these results scarcely seem capable of sustaining either temporal shifts or class differences in aspirations.

Perhaps the most striking and important conclusion drawn by Boudon from his model of social opportunity is "that the structure of mobility appears to be almost constant over time. . . . A drastic increase in the rate of school attendance and a nonnegligible reduction of IEO have little if any impact on the probability that a youngster with background status  $i$  will reach status  $j$ , whatever  $i$  and  $j$ , even under the assumption of a completely meritocratic society" (p. 153). Nominally, this may appear "surprising," but it should not be. In fact, the model does *not* change inequality of educational opportunity. It merely raises average origin-specific educational attainment and then correspondingly lowers average education-specific social destination. It is trivial that such a shift in the distribution of an intervening variable need not disturb the relationship between its antecedent and consequent.

Chapter 9 brings selected data to bear on implications of the model. For example, Boudon finds "dominance and IEO are unable to prevent relatively high rates of social demotion, even where meritocracy is extensive" (p. 168). After looking at Glass's British mobility table and a previously unpublished French (INSEE) mobility table, he writes, "The conclusion from the model appears to be confirmed by the data" (p. 168). Again, one need not go far afield to find conflicting evidence. Look at Blau and Duncan's (1967, table 2.2, p. 28)  $17 \times 17$  table of men's mobility from father's occupation to son's current occupation, in which the occupations are arranged roughly in accord with their socioeconomic standing. If we disregard the bordering rows and columns of the table (in which upward or downward mobility is inevitable) and compare outflow

percentages in the 105 pairs of cells pertaining to mobility between the same occupations, we find fewer than 20 cells in which the rate of downward mobility exceeds that of upward mobility. For further evidence of the persistence of upward intergenerational mobility in the United States, see Duncan (1965).

Much of the chapter attempts to show that education-specific occupational opportunities are declining in Western nations. I think the proposition is at least partly true, though not for the same reasons, nor with the same consequences as those suggested by Boudon. For relevant evidence, see James Davis's 1963 paper, "Higher Education: Selection and Opportunity" or Hauser and Featherman (1974). As in other instances, the evidence which Boudon brings to bear on this matter is not very compelling. While the hypothesis appears to require evidence from temporal change in joint education-occupation distributions, Boudon shows a remarkable willingness to accept evidence from cross-national comparisons when he thinks it supports his thesis (pp. 170-74) and to reject such evidence when it does not support him (pp. 184, 191). Similarly, at an earlier place in the book (pp. 134-36), Boudon both misreads and misinterprets Blau and Duncan's (1967, p. 170) basic model of the process of occupational achievement among U.S. men. The commission of these errors by the expositor of dependence analysis (Boudon 1965) is surprising. So is Boudon's criticism of linearity in Blau and Duncan's model, for it ignores their appendix H, which replicates their analyses without assuming linearity.

Where does all of this leave Boudon's thesis? I think it is not very compelling. There is a demand and supply for schooling, as for jobs, but Boudon ignores this in his radically atomized view of persons finding places in an infinitely expandable schoolroom and moving from there to a rigidly fixed system of social positions. It is naive to maintain that the demand for schooling is "endogenous," while the supply of jobs is determined "exogenously." In fact, these two terms are misused in Boudon's model; the "endogenous" variability in educational attainment is not provided by the model but is one of the parameters, and the "exogenous" variable of the social structure is simply a constant.

The adjustment of schooling and occupational distributions involves a delicate and volatile interplay of many social forces. For an individual, increased schooling betters the chance of obtaining a job which is high in social standing and whose performance is intrinsically rewarding. In the aggregate, increased schooling may tend to lower the occupational chances of persons at each level of schooling, but it may have other effects as well. By lowering the price of highly educated labor, it may increase the incentive to create jobs for highly educated persons. Moreover, the increased productive capacity of a more educated work force may shift the organiza-

tion of both production and consumption in directions which lead to greater demand for more educated workers. At the same time, persons making decisions about whether to stay in school are neither unaware of nor insensitive to variations in labor market conditions. For example, it is well known that in the United States variations in the secular trend toward increased schooling have been inverse to variations in the business cycle. Finally, the adjustment between levels of schooling and the occupational distribution is not accomplished mechanically and instantaneously for each new cohort of persons leaving school, but by complex and temporally extensive processes of demographic metabolism whose components include occupational recruitment, movements into and out of the labor force over the life cycle, and intragenerational occupational mobility. Boudon ignores so many of these factors that one cannot take his conclusions seriously.

Suppose education-specific occupational chances really do decline over time in Western nations. The current labor market squeeze for highly educated manpower may far more readily be interpreted as a consequence of the business cycle and of long cycles of birth rates than of an insatiable demand for competitive educational credentials. Moreover, while Boudon acknowledges that rising real income has affected the demand for schooling (p. 72), he does not undertake or even suggest any analyses which would clarify the relative importance of this mechanism and of labor market competition as explanations of the rising demand for schooling. Boudon's emphasis on market competition rests on a reification of the notion, held by human capitalists and some stratification researchers, that schooling is only an investment and not a consumption good, that is, that its only value is in exchange for jobs and social standing. I believe this outlook does injustice to humanistic goals which have long been held by elites in Western society and which rising levels of living have brought within the reach of broad segments of the population. Lowered status expectations may well be the price of mass enlightenment.

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## Book Reviews

*The American Intellectual Elite.* By Charles Kadushin. Boston and Toronto: Little, Brown & Co., 1974. Pp. x+395. \$8.95.

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I feel obliged to mention at the outset that I was in the sample of 110 "elite intellectuals" interviewed in 1970 for this study. This review is therefore an instance of one of the "natives" talking back, as anticipated by the author, who notes in the preface that his subjects not only were not "semiliterate tribesmen . . . helpless to counter the accounts of anthropologists" but also were notoriously prone to contentious theorizing about themselves.

The sample was selected from all writers contributing four or more times in 1964-68 to the 25 most "influential" intellectual journals and also included all the chief editors of those journals. The unwary reader may easily get the impression that the sampled intellectuals picked the influential magazines rather than the other way around, for Kadushin omits an account of the independent procedure he used to choose the journals, although this first stage of the study was fully reported in a 1971 *Public Opinion Quarterly* article. He says he left out methodological details in the hope that his subjects would read the book instead of dismissing it as "one more piece of sociological nonsense" containing "too many statistics and technical words." He must be disappointed, because the reviews I have seen have attacked the book on the familiar grounds that it deploys a complicated and expensive method largely to document what we already know.

Such rejections are scarcely fair, for the study provides a wealth of interesting detail on the social and demographic characteristics of leading American intellectuals that goes far beyond previous, largely impressionistic, knowledge even if it holds few surprises. (What would be a real surprise anyway? That American intellectuals were mostly small-town Catholics from the lower Midwest?) The autonomous social reality of the elite intellectual community is confirmed by the finding that opinions and attitudes show little or no correlation with class origin, occupation, organizational affiliation, or religious background. Differences in opinion reflected past alignments on issues passionately debated within the intellectual community rather than the impact of "external" social variables. At least in this respect, Mannheim's "free-floating intelligentsia" has long been alive and well in America.

American intellectuals constitute more than an "invisible college" but less than an occupational community centered in universities or professional associations. Kadushin uses the concepts of "network" and "social



circle," which he has employed in earlier research on other groups, to describe their loose social organization. He subjects sociometric choices derived from the interviews to a computer process developed by Richard Alba to reveal the structure of the total network and to identify six distinct social circles (though the two largest overlap) within it. Extensive quotations from the interviews are drawn on to supplement and give credence to the computerized model. A separate chapter by Thomas J. Conway, based on a review of a large number of journals, summarizes the different attitudes of intellectuals toward the Cold War since the late 1940s. Kadushin's account of the social structure of the intellectual world is, I think, largely accurate, and sensitive to its historical roots to a degree lacking in most survey research. The shape of the intellectual community, loosely integrated but far less amorphous than is often believed, began to take form as long ago as the 1930s. Like all genuine social formations, it has been resistant to change, though seriously buffeted and fragmented by the political and cultural conflicts of the 1960s.

Kadushin includes a listing of the 70 "most prestigious contemporary intellectuals"—many of whom were themselves in the sample—derived from the nominations of his subjects. Americans love rating games, and predictably the *Time* review of the book was festooned with photos of the top 10. Unfortunately, the list lends an air of factitiousness to the entire study by undermining some of its valid general conclusions. Editors and foundation executives are included who were obviously named for their organizational power rather than their intellectual achievement. For all the talk about circles and networks, at least a dozen eminences on the list almost certainly have little or no personal association with others in the sample or the universe from which it was drawn. (Two of them are staff writers for the *New York Times*, read by 98% of the sample but not itself one of the "gatekeeping" periodicals that provided the starting point and basis of the whole study.) Eight persons on the list—over 10%—have died since 1970.

Kadushin notes that two persons on the list are not Americans and defensively observes that they have at least spent time at American universities; he overlooked two more of whom this is also true. I am not just picking nits, for one wonders why foreigners were excluded anyway. Surely Aron, Leavis, Lukács, and Sartre, to name only a few, would have made the list. The exclusion of foreigners epitomizes the study's most serious omission, its failure even to mention the major task and contribution of the intellectual elite over the past 40 years: the communication to an educated American audience of the best thought and art of Europe. Kadushin never refers to the role that German refugees have played in this process, although six are in the top 70. Doubtless it is true, as so many of the respondents say, that New York has never had the central position in American culture that London and Paris have in England and France. But the New York intellectuals brought the ideas of London and Paris—and the Berlin of the 1920s—to these formerly provincial shores. The

clichés of political protest and “countercultural” revolt in the sixties, still new to so many sociologists, were already clichés in New York intellectual circles 20 years earlier.

The second and third sections of *The American Intellectual Elite*, making up about half of the book, report the opinions of the sample on various political and social issues of the 1960s. The findings are neither novel nor very interesting, and many of Kadushin’s comments and interpretations are questionable. He is often drawn into a scarcely muted running argument with his subjects, who apparently disappointed him because, although overwhelmingly left-liberal in outlook, they were generally unsympathetic to the New Left, the counterculture, and the student rebels. I am mildly surprised—and pleased—to learn that there were so many “closet moderates” in 1970, even in those circles to which the label “radical chic” was originally applied; those of us who did not conceal a distaste for the dangerous puerilities of the New Left were not so alone in our attitude as we thought at the time.

Kadushin understandably chose the Vietnam war as the major political event of the period, although, as he does not sufficiently stress, it was not a good issue on which to test the opinion leadership of “generalists” (his term) who could hardly be expected to know much about the tangled affairs of a small Asian country before the American involvement there became fully visible in the middle sixties. He obviously regards as his major “discovery” the judgment that intellectuals were not “radicalized” by their nearly unanimous opposition to the war but opposed it on narrow “pragmatic” rather than “moral” or “ideological” grounds. One can agree that few critics of the war became radicals, without accepting Kadushin’s constantly reiterated claim that they turned against the war “simply because it did not work,” a claim echoing the “more-militant-than-thou” rhetoric and the false polarities so trumpeted at the time.

For example, I opposed the war from early 1965 when I—somewhat belatedly—became aware of the extent of American involvement. I did not oppose it because I thought all wars immoral, or because I thought all American wars, and this one in particular, were caused by the imperialistic compulsions of American capitalism—the positions Kadushin labels “moral” and “ideological” held by pacifist and radical minorities in the sample. My opposition, reported in the interview and quoted in part by Kadushin as representative of the views of others, rested on the conviction that waging war is *never* morally justified except in self-defense or when failure to do so risks a greater war in the near future, neither of which circumstances applied in Vietnam. It is a gross distortion to regard such grounds of opposition as “pragmatic” in the vulgar sense of stemming only from belief that the war could not be won, or even of implying the primacy of national interest over any other consideration. If Kadushin had pushed his historical analysis back to the late thirties, he would have discovered that much of left-liberal opinion had a *moral* commitment to what was then called “collective security.” (He would also have discovered that it

was not attitudes toward the Cold War but toward Stalinism years earlier that had shaped the outlook of many of his respondents.)

Borrowing Robert W. Tucker's distinction between "liberal-realist" and "radical" views of American foreign policy, Kadushin correctly classifies the majority of his respondents as liberal realists but then falsely equates Tucker's dichotomy with his own crudely simplistic contrast between "pragmatic" and "moral" or "ideological." Tucker provides no warrant for this; he writes, in fact, that "in the liberal realist critique the disparity which defines the tragedy of Vietnam also defines the *immorality* of the war" (*The Radical Left and American Foreign Policy*, p. 48, my emphasis). Kadushin needs to be reminded that Weber identified *two* political ethics, and in seeing the morally absolutist one (*Gesinnungsethik*) as specifically "inappropriate" to politics, he did not consider the other one (*Verantwortungsethik*) any the less an *ethic* because it takes into account the consequences of action as well as its ends and intentions.

This polemical animus against his subjects carries over into the section reviewing their opinions on social problems and the crisis of culture. The last chapter of this section is entitled "No Gurus Here," highlighting Kadushin's foolish conclusion that his subjects "were neither gurus nor sages, prophets nor sons of prophets." Checking the list of the top 70, one must ask whether Kadushin really thinks that Walter Lippmann and Reinhold Niebuhr were not sages, R. Buckminster Fuller and Herbert Marcuse are not gurus, Norman Brown and Norman Mailer not prophets? (As for "sons of prophets," I know personally several men in the sample who had prophetic mothers.) Does Kadushin think that gurus must live in caves and prophets wear robes and long flowing beards, or, perhaps, die in London in exile from the Teutonic heartland of Europe? He goes on to remark that "the intellectual elite in their conversations about social issues for the most part had little more to say than what might be heard in the cocktail party conversation of intelligent, well-educated people." This reminds me of a famous observation by T. S. Eliot: "Someone said: 'The dead writers are remote from us because we *know* so much more than they did.' Precisely, and they are that which we know." Who does Kadushin imagine establishes the topics and themes that "intelligent, well-educated people" talk about at cocktail parties?

The study is too obsessed with the role of intellectuals in directly influencing public policy on "social issues," perhaps because it is based on an opinion poll and uses other polls for comparative reference, perhaps because Kadushin cherishes the wistful—and wishful—belief that the split in the sixties between the intellectual elite and radical youth "missed . . . a great opportunity to transform America." There have been times in modern history when intellectuals have been a political vanguard, but most of the time that has not been their main business. Several times Kadushin cites at length the long essay by Irving Howe (in the top 10) on "The New York Intellectuals: A Chronicle and a Critique" (included in Howe's *The Decline of the New*), but he fails to quote the following passage:

"What drove them, and sometimes drove them crazy, was not, however, the quest for money, nor even a chance to 'mix' with White House residents; it was finally, when all the trivia of existence were brushed away, a gnawing ambition to write something, even three pages, that might live." Not the sort of motive apt to show up in an opinion poll. Too confined to literary people, it might be objected, but what distinguishes the intellectual, even when not a poet or novelist, from academicians in general is less a concern with public affairs than the longing "to make his thought / alive like patterns a murmur of starlings / rising in joy over wolds unwittingly weave" (Auden, on the list of 70).

*The American Intellectual Elite* is a good antidote and corrective to the recent gossip books, often reeking with *ressentiment*, of Philip Nobile and Richard Kostelanetz. I would certainly assign it, especially the first 100 and the last 50 pages, to an Outlander seeking enlightenment on the American intellectual world. But it would have lower priority on my reading list than Howe's essay, the autobiographies of Alfred Kazin and Norman Podhoretz (both on the list of 70), the novel *The Middle of the Journey* by Lionel Trilling (in the top 10), several of Delmore Schwartz's short stories from *The World Is a Wedding*, and H. Stuart Hughes's new history of the influence of refugee social thought in America from 1930 to 1965, *The Sea Change*. All written, to be sure, by "insiders" in the sense of Merton (on the list of 70, where, incidentally, he is joined by no less than six additional *bona fide* sociologists). But perhaps I should close with the confession that, for me at least, the ideas and values of, among others, Hannah Arendt, W. H. Auden, Saul Bellow, George Lichtheim, and Harold Rosenberg (all in the top 70) represent a contemporary standard of quality, often of greatness, against which to measure our sociology rather than the other way around.

*Towards the Sociology of Knowledge: Origin and Development of a Sociological Thought Style*. Edited by Gunter W. Remmling. New York: Humanities Press, 1973. Pp. xxi+463. \$18.00.

Kurt H. Wolff

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Unlike the first reader on the sociology of knowledge in English (Curtis and Petras 1970), Remmling's contains no excerpts from Bacon, Pareto, Durkheim, Scheler, and Grünwald; of forerunners and "classics," there are only Marx and Mannheim (represented in the earlier volume too). There are 26 chapters (all but three, reprints) in nine parts. The first, Remmling's original introduction, is long on names but short on persuasion. Part 2, "Forerunners and Pioneers," consists of three chapters: on Bacon and the French Enlightenment philosophers (from Remmling's *Road to Suspicion*), on elements of a sociology of ideas in Saint-Simon (by George G. Iggers), and on the widely overlooked conservative tradition in the

sociology of knowledge (by Werner Stark). Part 3 presents Arthur Child on the theoretical possibility of the sociology of knowledge, brief excerpts from Mannheim's "Historicism," and Rolf Schulze on the functions of ideology (defined as any belief system to which there is commitment).

Part 4, "Karl Marx and the Social Determination of Consciousness," has three of Marx's autobiographical pages from the *Critique of Political Economy* and 18 from Remmling's earlier book. Similarly, part 5, on Durkheim "and the sociological theory of knowledge," offers Remmling's overview of the French tradition in the sociology of knowledge (from the same source) and Edward L. Schaub's early (1920) neglected critique of Durkheim's "sociological theory of knowledge." Durkheim, Schaub shows, fails to prove the social derivation of the categories of thought. Part 6, on Scheler and "phenomenological sociology of knowledge," contains Mannheim's (1925) pages on Scheler as well as Howard Becker and Helmut Otto Dahlke's still useful introduction to him (but how understandable to one who *needs* an introduction?). Part 7, on Mannheim and "historicist sociology of knowledge," gives us Remmling's view of Mannheim's four-phase development (from his *The Sociology of Karl Mannheim*, scheduled for 1975). Included also are Virgil G. Hinshaw's denial of the epistemological relevance of (Mannheim's) sociology of knowledge, and Thelma Z. Lavine's argument for the historicist sources of his functionalism.

In part 8, "Contemporary Sociology of Knowledge: Symbolic Interactionism, Phenomenology, Quantitativism," Harvey A. Farberman compares Mannheim, Cooley, and Mead (elementary on Mannheim, good on Cooley, enthusiastic on Mead). Peter L. Berger deals with "Identity as a Problem in the Sociology of Knowledge" (the gist of his and Thomas Luckmann's *The Social Construction of Reality* of the same year, 1966). Edward A. Tiryakian's "Existential Phenomenology and the Sociological Tradition" defines neither of the first two terms in the title and is more plausible on Simmel than on Max Weber and Mannheim as phenomenologists. David Martin reviews (but hardly does more than paraphrase) Staude's study of Scheler and books by Alfred Schutz, Berger and Luckmann—and F. A. Hayek. And Franz Adler tries to quantify the sociology of knowledge.

Finally, there is part 9, "Applied Sociology of Knowledge." Alex Simirenko studies immigrant assimilation with the help of Mannheim's generation concepts (sociology of knowledge?). Robert G. Snyder pleads for the change of the expert into an intellectual and of the university as the home of the former into that of the latter (a study in the sociology of intellectuals). Kurt Danziger analyzes "Ideology and Utopia in South Africa"; this I find a far more convincing application than Adler's. Remmling, Georg Maier, and Elba Valdivia Remmling study social classes in Ecuador readably and informatively (but, sociology of knowledge?). And Manfred Stanley examines "The Structures of Doubt: Reflections on Moral Intelligibility as a Problem in the Sociology of Knowledge." This is the third previously unpublished contribution, the longest in the volume, and altogether remarkable in its knowledge, circumspection, imagination,

and wide range; it is likely to be picked up and built on in more than one context.

Under its own name, the sociology of knowledge is half a century old. As a more or less clearly demarked field, it is rather the sociology of science, a relative or subdivision, that has important studies to its credit, while the sociology of knowledge itself resists identification. "Knowledge," well known as a misnomer, is almost as broad as culture or, as Louis Wirth preferred to call it, intellectual life. But the sociology of knowledge is distinguished from other studies of culture (whether cultural anthropology or intellectual history) by its attitude: in the main, this is either critical—often Marxist—or scientific, claiming to apply to culture the natural scientific methods applied to nonhuman affairs. Perhaps the most far-reaching effect of the sociology of knowledge lies in the acceptance of intellectual matters as a legitimate part of the subject matter of sociology. What to do with this acceptance, on the other hand, has *not* been decided, nor do I think that it can be taken for granted that the solution of the difficult problems raised by it will be found during the next half-century.

*Power and Progress: Essays on Sociological Theory.* By Robert Bierstedt. New York: McGraw-Hill Book Co., 1974. Pp. viii+330. \$11.50.

Jessie Bernard

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The title, though not the subtitle, of this book is misleading. It is a collection of papers and reviews on a variety of topics, dating from 1938 on. Part 1 consists of six papers—on Sorokin, Parsons, Lundberg, Toynbee, Schutz, and the Social Darwinists—introduced by an autobiographical chapter in which Bierstedt describes his own contacts with those he has known personally.

The five papers on methodological topics deal with issues "on which responsible scholars have held a variety of opinions" (p. 109) and still do. The author comments on how anthropologists turned during World War II from a concern with preliterate peoples to a concern with literate peoples, with results that seemed to him often meretricious (p. 110). In one paper Bierstedt deals with the flurry of semantic concern in the thirties and forties, and in another with the pros and cons of technical vocabularies.

Part 3, on social organization, deals with the sociology of majorities, social power, and authority. It was, in my opinion, a mistake to single out two of the papers for the title of this book because that calls special and disproportionate attention to them. The paper on the concept of progress, written originally in response to a paper by Morris Ginzberg in 1953 and not greatly modified in 1974, once more gives that concept a drubbing. Progress is an attitude related to temperament: optimists believe in it; pessimists do not. The analysis of social power (1950) distinguishes it from each of the following: prestige, influence, dominance, rights, force, and

authority defined as institutionalized power; it relates power to formal and to informal organization; and it locates the source of power in numbers, organization, and resources. (In this connection, I would also like to call attention to the discussions of power and authority in the first *Encyclopedia of the Social Sciences*, which remain classics.)

Part 4, "On Civilization and Humane Learning," has four papers. The first, on "Indices of Civilization," after a scholarly discussion of the equivocal nature of the concepts of culture and civilization, finds "literate sophistication" to be a prime index of civilization and invites us to entertain the idea that sociology itself is an index of civilization. "The civilized man reflects . . . upon the meaning of society, on its structure and changes, and on the nature of human association" (p. 278). Still, one man's reflections on human association and on the meaning, structure, and changes in human society may lead him to justify slavery, exploitation of all kinds, and sexist and racist institutions. Under this index we would have to recognize the Nazis as civilized men. I am reluctant to do so. Not all those who possess "literate sophistication" are as civilized as Robert Bierstedt.

A 1964 paper on "Sociology and General Education" makes a strong case for the importance of sociology for the liberal arts student, and in this I heartily concur. Bierstedt concedes Marvin Bressler's later comment that other subjects also serve the same liberating purpose, and he waters down his enthusiasm to stating only that sociology cannot be excluded from the liberal arts. I go along with the seven virtues of sociology he specifies. Sociology does, indeed, as Bierstedt says, "liberate the student from the provincialisms of time, place, and circumstances" (p. 296), and it is, indeed, true that "whether the group that claims our parochial loyalty be race or region, or class or religion, or neighborhood or nation, we are all influenced by it and, when uneducated, controlled by it" (p. 298). Why, when reviewing this paper in 1974, did he not include sex among the parochial loyalties by which we are controlled? Or comment on the fact that liberation from this particular parochialism is only now coming to sociologists, well educated all?

There are other parochialisms not always transcended by sociologists. As one who has watched many generations of young sociologists enter the profession, I have been interested to note how each one seems to think not only that sociology is what it was when they entered the scene but even that it was what they learned from their own professors at their university. I have noted the surprise as this parochialism gave way under the impact of professional experience—the amazement, for example, of eastern seaboard Ph.D.'s to learn that there were sociologists trained at great middle-western universities who didn't give a fig about what their mentor had said about this or that and, conversely, the resentment of Ph.D.'s from the great middle-western universities at the ignore-ance of their mentor's contributions. To all of them Bierstedt's book, as a graceful statement of the issues that seemed important between 1937 and 1966, should liberate them from time parochialism. There remains still a kind of specialty parochialism within the discipline itself which shows itself in numerous

separate little "communities of scientists" in the Kuhn sense, often intolerant of one another. Bierstedt's book may appeal to some; I doubt if many hard-liners will pay much attention to it.

In the final paper, a plea for sociology as a humane discipline (1960), Bierstedt concludes that "objectivity may not be as desirable a criterion as it is commonly thought to be" (p. 319). Here he adumbrated the recent sociology-of-knowledge critique of "objectivity." For problems not "amenable to solution with methods currently available," he thinks it would be better "to take a single factor and to push it to an extreme as a possible mode of interpretation" (p. 320). This is precisely what many of the women sociologists today are doing with the sex-role factor.

This book, finally, like everything that issues from Bierstedt's typewriter, is scholarly and elegant; I hope no one will consider it a damning judgment if I add that it is also enjoyable reading.

*From Karl Mannheim.* Edited, with an introduction by Kurt H. Wolff. New York: Oxford University Press, 1971. Pp. cxi+393. \$12.50 (cloth); \$3.95 (paper).

G. H. Müller

*The American University*

The present volume of 11 essays is a representative selection from Mannheim's writings other than the classic *Ideology and Utopia* (1929) and the seminal article on "The Sociology of Knowledge" (1931). The essays are preceded by an introduction and a bibliography of Mannheim's complete works, including the available English translations. I regret only that there is no bibliography of publications on Mannheim and his contribution.

Itself a work of book length, the introduction is judicious but not so lofty as to be above pertinent criticism. Proceeding in an encyclopedic manner which deals with each work separately, including even the young Mannheim's doctoral thesis (1922), it is as meticulous as it is complete, without sating the desire for future monographs.

Kurt Wolff has impeccable credentials, also having edited in 1964 a German edition of Mannheim's complete works on *Wissenssoziologie*. Disciples may deplore the exclusion of "Historicism" (1924), or "The Problem of Generations" (1927), or "The Problem of the Intelligentsia" (1933), but since it is apparently Wolff's aim to complement *Ideology and Utopia* and to avoid redundancy, these omissions are wholly appropriate. The relative brevity with which Mannheim's later work is dealt also seems warranted—it hardly stands comparison with the earlier.

Although the credit for making Mannheim's earlier work accessible in English belongs mostly to Paul Kecskemeti, whose translations constitute the bulk of the present volume, the outcome of Wolff's effort is a first-rate reader in the sociology of knowledge. It renders a double service to American sociology: the selections will considerably broaden the insight



into Mannheim's thought; at the same time, they will balance the prevailing Parsonian overemphasis on Durkheim, Pareto, and Weber with an accent on Marx and Scheler and thus make a welcome contribution to theory in general.

The first six selections show impressively Mannheim's shift from an early idealistic to a sociological vantage point. It turns out that Mannheim was never as strongly Marxian oriented as *Ideology and Utopia*, read out of context, seems to indicate, for Mannheim's familiarity with Marx bears the imprint of Scheler quite as much as of the early Georg Lukács. Indeed, Mannheim's Marxian heritage is predicated first on a phenomenological (1925) and then on an existential (1926-28) interpretation of the economic factor. Had he not been forced to emigrate, Mannheim would probably have expanded his concept of the sociology of knowledge into a "sociology of the mind" (which is the title of another essay [1933] unfortunately not included in the present volume but discussed in the introduction).

The first two selections, a review of Lukács's book *The Theory of the Novel* (1920), and an essay, "On the Interpretation of *Weltanschauung*" (1921/22), give the clearest picture of the early Mannheim who, like his mentor Lukács, was at that time as steeped in neo-Kantian idealism as he was in a neo-Hegelian view of Marxism. In addition, the review is informed by a notion of "ontological [hierarchical] stratification" while the essay distinguishes between the realms of the rational and irrational, or the theoretical and atheoretical, a fruitful and promising dichotomy which anticipates A. Schutz's juxtaposition of objective and subjective meaning by a decade.

Written three years later, "The Problem of a Sociology of Knowledge" (1925) heralds Mannheim's decisive move toward a sociological analysis. Most notably, it demonstrates the strong impact of phenomenological thought (in the original Husserlian sense) which culminates in the insight that economic orientation (primarily, interest) is itself an integral part of that mind which he calls "mind in the substructure" in contradistinction to "mind in the superstructure" (p. 88).

The apt combination of Marxism with phenomenological analysis was to be replaced the next year by an existential interpretation which defines the realm of the irrational and atheoretical as the realm of pretheoretical, existential thought (p. 124) which "involves the whole person" (p. 118). Unfortunately, the perceptive dichotomy is marred by Mannheim's enduring holistic tendency which relates the social dimension with the ideological dimension, not as different domains, or parts, but as a part to the whole (p. 128). Consequently, society and ideology are not placed on an equal footing, but ideology engulfs all. It is this holistic birth defect (an inheritance from the early Lukács) which fatefully pervades Mannheim's thought life-long. It is also the reason why Mannheim had to drop his early concept of theoretical, or objective, thought: as a superstructure to atheoretical, existential thought, it would have to be interpreted as the

whole that engrosses ideology, thus undercutting his own argument in his dispute with Scheler.

Within the frame of the book as well as within his life, Mannheim's articles on "Conservative Thought" (1927) and "On Competition as a Cultural Phenomenon" (1928), immediately preceding *Ideology and Utopia* (1929), mark the apex of his intellectual development. Both are a must for the student of sociological theory. Bypassing Scheler's mystifying belief in absolute values, each of the articles is a penetrating inquiry into the origin of values ("existential thought" in Mannheim's terms), the first in a historical context, the second in a more analytic fashion. Both bear the imprint of Hegel (and only partly of Marx), and together they constitute a compelling argument against Parsons's interpretation of values as integrative (pattern-maintaining) factors and functions.

It is ironic that Mannheim came to perceptive and penetrating insights, such as the affinity between conservatism and morphological thought on the one hand and between liberalism and analytic thought on the other (pp. 248, 252), without departing from his own holistic (implicitly morphological and conservative) stance. Mannheim's failure to overcome historicism and to join the analytic camp marks the underlying tragedy of his intellectual career, quite apart from his emigration and the paroxysms of his age.

*Phenomenology, Language and the Social Sciences.* By Maurice Roche. London and Boston: Routledge & Kegan Paul, 1973. Pp. vii+361. \$15.50.

Don H. Zimmerman

*University of California, Santa Barbara*

In this ambitious book Roche addresses a growing audience which holds the view that the attempt by social science to emulate the natural sciences has produced few, if any, victories in the struggle to gain systematic knowledge of the social world. He sets out to examine two philosophical movements, phenomenology and conceptual analysis, in terms of their fundamental ontological, epistemological, and methodological commitments and to relate these "humanistic" and, from Roche's perspective, antiscientific philosophies to the rekindled debate in the social sciences over the appropriate model of inquiry for the study of person and society. His thesis is that the humanistic bent in philosophy and the emergent humanistic movement in the social sciences share very fundamental presuppositions, and that by virtue of this community, a fruitful dialogue between these otherwise disparate enterprises should be possible.

Roche's reach is all but encyclopedic. His first two chapters deal with phenomenology (pure and existential) and conceptual analysis (or ordinary language philosophy), and it is in these 84 pages that he lays the groundwork for his argument concerning the basic similarity of these two schools. In successive chapters he examines logical empiricism; behaviorist,

Gestalt, and Freudian psychology; humanistic psychiatry and labeling theory; Weber's sociology; and humanistic philosophy and sociology. Humanistic psychiatry (e.g., early existential psychoanalysts such as Binswanger and later figures such as Laing and Szasz) and sociology (the latter including labeling theory, symbolic interactionism, and ethnomethodology) constitute the so-called humanistic approach to social science. The object of Roche's review of logical empiricism, behaviorism, Gestalt psychology, and Freudianism is to recount their shortcomings on grounds furnished by his reading of phenomenology and conceptual analysis. The chapters dealing with humanistic psychiatry and sociology, the sociology of Max Weber, and, in particular, the concluding chapter on humanistic philosophy and sociology appear to deal, more or less, with what a humanistically oriented social science should look like.

Presumably, social scientists and, in particular, sociologists should find the results of Roche's analysis illuminating and useful. Unfortunately, the book adds little, if anything, to the already well-rehearsed arguments over the difference or lack of difference between the natural and social worlds. His attempt to link phenomenology and conceptual analysis as "humanistic" philosophies succeeds, if at all, at a very abstract level and, when extended to certain strands of social science, generates mischief and confusion rather than any clear and useful framework for theory or research.

Humanism is given a very specific definition in this book: commitment to a "personalistic" ontology and "experiential empiricism." The latter commitment is primarily a methodological stance which will be examined below.

According to Roche, a personalistic ontology assumes that there are entities such as persons having the "attributes of being (a) embodied, (b) temporal, (c) intentional and (d) social" (p. 297). Embodiment and temporality mean that persons knowingly progress through the life cycle to ultimate death. Intentionality and sociality mean that persons are goal-directed and have the "tendency to believe that there are other persons in the world besides themselves" (p. 298). The contrasting ontological commitment is to materialism which, of course, reduces the person ultimately to the interaction of forces which can be explained by natural science. The shrillness of Roche's insistence on the virtues of what he identifies as the humanist position is matched by his dogged attack on reductionism in social science, although he does not seem to realize that the nonreductionist view in sociology does not necessarily entail a rejection of the natural science model.

One need not contest Roche's assertion that the assorted philosophical and social science approaches surveyed in this book affirm, if only implicitly, that human beings are persons in Roche's sense. Still, it is unclear what fruit this supposed community is supposed to yield, given the equally fundamental differences among the various philosophical and social scientific schools which Roche himself acknowledges.

For example, one can distinguish between the phenomenological analy-

sis of that which is present to consciousness and the analysis of linguistic and cultural intuitions by the conceptual analyst. Because the phenomenologist employs his/her own consciousness as a field of inquiry and the conceptual analyst employs his/her own knowledge of language and culture as a resource does not necessarily bespeak an interesting similitude between the two. In any event, one can demand a demonstration that the similarities are more crucial than the differences, and this Roche has not provided.

The treatment of what is termed "existential empiricism" reveals one of the book's major flaws. Roche writes: "[I]t has been argued that both schools [phenomenology and conceptual analysis] use a similar method of descriptive analysis which deals with the facts of ordinary social experience, particularly with facts concerning the experience of social interaction and the experience of action. We can call this approach 'existential empiricism,' as it is an empirical, i.e., descriptive and factual, account of experience" (p. 292).

The difficulty with this notion lies in the hopelessly vague sense that seems to attach to the notion of an "empirical account of experience." We are not told in any explicit sense what will count as an adequate description or even told that there might be problems with the adequacy of descriptions. Moreover, Roche provides no discussion of the warrant upon which might be based the assertion that an account of experience is "factual." Part of the problem here stems perhaps from Roche's emphasis on existential phenomenology and the slighting of Husserl's methodological program. What we are left with is an invitation to accurately describe meanings: "The most important procedural presuppositions [of experiential empiricism] are that it is right to describe accurately, and if needs be minutely, the meanings that men use and that they see in the world, and facts about action and interaction" (p. 296).

How do we assure ourselves and, most particularly, others, that the meanings "described" are those that subjects use or find in the world? This question remains unanswered if not unaddressed. The *sine qua non* of a science (even, I think, a social science) is disciplined inquiry. No such discipline is even hinted at here. But Roche's attempt to overcome the radical subjectivism implied by his statement of "experiential empiricism" leads him to a position which is both ironic and informative, albeit negatively.

In order to defeat the criticism that reference to the intentions of actors necessarily entails an appeal to an inaccessible and hence inherently subjective domain of mind, Roche argues that, to the contrary, individual intentions, or meanings, are available through language and that motives and the like are accessible against the cultural grid of norms and role expectations (see pp. 300-301). The irony here (perhaps a double irony) is that Roche solves his problem through recourse to the assumption of "cognitive consensus" which, as Wilson (*American Sociological Review* 26 [August 1970]: 189-93) has argued, is at the heart of the positivist approach to social science against which Roche inveighs. This position is

illuminating in that it demonstrates once again that the methodological problems of the social sciences cannot be addressed by calls to furnish more and more meticulous descriptions of what actors *mean*.

It is impossible within the confines of this review to fully discuss the many questionable assertions and representations put forth by Roche. I shall only list a few in closing. Roche's treatment of phenomenology systematically slights the methodological import of Husserl's phenomenology (a point alluded to above) and places the burden of carrying his thesis on existential phenomenology (in particular, Merleau-Ponty and Sartre). Roche's mastery of either pure or existential phenomenology is questionable, as A. G. Pleydell-Pearce has pointed out (*Journal of the British Society for Phenomenology* 6 [January 1975]: 65-68). While sympathetic to it, Roche misconstrues ethnomethodology (a common enough occurrence, and forgivable). But he also misconstrues Merton and structural-functionalism (e.g., he asserts without argument that latent functions should "logically imply" latent structures [p. 310]). This is a less common and less forgivable thing to do. All in all, Roche is successful merely in caricaturing the intellectual good guys (i.e., humanists) and bad guys (i.e., positivists) that are unfortunate enough to come under his scrutiny. Little else is sharply delineated in this disappointing book.

*The Welfare State and Equality: Structural and Ideological Roots of Public Expenditures.* By Harold L. Wilensky. Berkeley: University of California Press, 1975. Pp. xvii+151. \$8.95 (cloth); \$2.85 (paper).

Mayer N. Zald

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The growth of the welfare state is one of the most important phenomena of modern life. As Harold Wilensky notes, it is astonishing how little serious attention has been given it by sociologists.

Although many sociologists are concerned with specific policy issues related to the welfare state, few have tried to map its major outlines and determinants. Boldly, Wilensky has begun a long-range program of comparative research on the variety of welfare programs and their impact on real welfare. He is interested in welfare outcomes and welfare determinants, the political, demographic, economic factors that shape the nature of the welfare state. His "research strategy for both this book and the larger study is to analyze crude cross-sectional data on gross categories of welfare and military spending for sixty-four countries, to do more detailed time-series analysis of the better data available for the twenty-two richest countries, and then to conduct intensive historical case studies of selected welfare state leaders, laggards and middle rank spenders amongst the richest countries" (p. xii). This short, first book presents the major research questions and hypotheses, outlines methods and measurements, and presents preliminary data. It is a provocative, exciting, and disappointing book.

First to the disappointment: Chapter 2, "Economic Level, Ideology and Social Structure," attempts to extend previous work by Aaron, Pryor, and Cutright on the societal determinants of social security programs and expenditures. It develops causal (path) models to assess the impact of socioeconomic level, political structure and ideology, percentage of the population aged, age of social security system, and military expenditures. The first causal model (p. 25) is based on a heterogeneous group of 60 nations, ranging from Togo and Upper Volta to Sweden, the USSR, and Iceland. Wilensky believes he shows that GNP per capita, percentage of population 65 and over, and age of social security system (an indicator of bureaucratic incrementalism) are the major determinants of social security effort (social security expenditure as a percentage of GNP). He also shows that liberal democratic regime (as a dummy variable) makes a small direct contribution to the proportion of population over 65, and totalitarian regime makes a direct contribution to the proportion over 65 and age of the social security system.

The advantage of causal modeling *should* be that it clarifies our theoretical thinking and gives us more precise estimates of linkage and effects. But in this case we get neither. (1) The three major explanatory variables (GNP per capita, percentage over age 65, and age of social security system) are highly correlated with each other and with the dependent variable. Precision of regression estimates decreases as multicollinearity increases among independent variables. (2) Three of the four key variables are ratio variables with shared terms (population in numerator twice, GNP in the numerator of one and the denominator of another). Where two or more ratio variables share terms, the size of correlation, negative and positive, is forced (to some unknown degree) by the measures. (3) Wilensky's causal ordering may well be wrong. Even in cross-sectional analysis, path models usually locate independent and early in time variables to the left, and later variables to the right. Here age of social security system is listed to the right of population over 65, and totalitarian state and liberal democratic state are listed to the left of population age. I submit that age of population in 1900, but not in 1966, may be a cause of age of system in 1966. Wilensky's regime variables are treated as causes of both age of population and age of system. An underlying variable in his classification of regimes is degree of citizen participation. That relates to degree of modernization. So his correlations with regime are probably spurious. Moreover, some of the countries, like Upper Volta and the Cameroons, are new nations: they cannot have had older social security systems (unless the colonial power instituted one). (4) Most important, the variable percentage of aged is a surrogate variable for health status, urbanization, effective birth control, and modernization, as well as aging. Wilensky argues that his analysis goes beyond Phillips Cutright's ("Political Structure, Economic Development, and National Social Security Program," *American Journal of Sociology* 70 [1965]: 537-50) because he shows the effect of fraction of aged (p. 24). But this variable has much too much theoretical ambiguity.

Percentage of aging may be a pure demand variable: assuming legal entitlement and constant benefits, and assuming that old-age relative benefits are the major component of social security expenditures, then, as the percentage of aged goes up, ratio of social security to GNP *must* increase. But, in fact, family and child assistance, public health services and insurance, and unemployment subsidies are also large parts of social security expenditures. Wilensky does not argue that the aged are a pressure group. Indeed, in the history of social security, it is industrialization and urbanization at an earlier date that cause the emergence of social security systems. A measure of urban and industrial work force might have given a mass pressure variable. But Wilensky leaves out measures of urbanization, or proportion of population in industrial labor forces. (See Cutright, "Income Redistribution: A Cross National Analysis," *Social Forces* 46 [1967]: 180-90, where the latter is used to explain redistribution.)

Thus, the causal modeling is a disappointment in two ways. First, given the earlier work by Cutright, Pryor, and Aaron and given our increasing sophistication in the use of regression analysis, Wilensky has so far missed a real chance to use time-series data, lagged correlations, a fuller model of social-economic process, and a more complex model of welfare expenditure to really advance cross-national comparative analysis. Second, the errors in the causal modeling may well lead people to ignore what is a highly provocative and stimulating set of hypotheses, observations, and interpretations.

Let me turn to what I find stimulating and provocative. In chapter 3, Wilensky turns to a discussion of diversity and uniformity among rich countries. He argues that elites in centralized political structures are more able to implement welfare program reform and innovation than elites in more decentralized nations, and he is able to show how the leaders and laggards differ in these respects.

Later he turns to forms of welfare financing, arguing that contributory and indirect taxation systems lead to less political resistance than direct taxation. This chapter is fascinating. Other chapters and sections deal with the short-term tradeoff between military and social security expenditures; the difference between equality of opportunity (represented by investments and expenditures for education) and absolute equality (represented by universalistic health and welfare services and redistributive measures); the interchangeability of private and public programs and the different effects of each; and the complex relation of war, conscription, and military expenditure to employment and welfare.

On all these topics Wilensky is provocative, even if occasionally he may turn out not to have told the whole story. For instance, I doubt if his account of the relationship of welfare and military expenditure to unemployment among big and little military spenders (p. 84) will hold up to close economic scrutiny.

In his last chapter, Wilensky turns to discussion of the impact of the welfare state on real welfare. I was surprised that he did not use Cutright's

(1967) analysis of the relation of social insurance program experience systems to income redistribution (or redistribution over the life cycle, as Norval Glenn comments and Cutright acknowledges in "Social Security and Income Redistribution [Comments], *Social Forces* 46 (1968): 538–40). However, he is able to untangle a number of relations among forms of taxation and types of public expenditure and welfare.

My general conclusion is that this book is provocative, if sometimes wrong; I suspect the final book from the project will be a major one on the modern welfare state—East and West. Taken together with Frederick Pryor's two books (*Public Expenditure in Communist and Capitalist Nations* [London: Allen & Unwin, 1968] and *Property and Industrial Organization in Communist and Capitalist Nations* [Bloomington: University of Indiana Press, 1973])—which, incidentally, are amazing pieces of work—and Gaston Rimlinger's *Welfare Policy and Industrialization in Europe, America and Russia* (New York: John Wiley, 1971), it will permit us finally to have under intellectual control a major part of the economic and social (though not political) processes and structures of the modern welfare state.

*On Creating Efficient Industrial Administration.* By Arthur L. Stinchcombe. New York: Academic Press, 1974. Pp. xii+208. \$9.50.

Henry A. Landsberger

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What are we going to say about a disarmingly honest and whimsical author (*a*) who describes one of his chief theoretical and policy conclusions (democratic socialism is essential for development) as being justified only "with a few loose joints in the logic" (and arrived at several years before this study, anyway); (*b*) who adds a footnote stating that the theoretical explanations of two key empirical phenomena (the stability of Chilean democracy and the failure of Peron) "look silly" in the light of events; (*c*) who begins his book by stating that it was rejected by the group of high-powered academics who sponsored it; and (*d*) whose ultimate conclusion is that economic development is caused by economic development? I go along with Robert K. Merton, whose exhilarated if slightly confused reaction is reported on the book's cover: "pure Stinchcombe . . . deeply imaginative, thoughtful [occasionally, I would add, to the point of seeming cryptic] and cognitively exciting. It brings important new questions. . . ." But now to the book.

Using as a data base his study of three steel mills in Argentina, Chile, and Venezuela (plus an infuriatingly frustrating encounter with the office issuing drivers' licenses in San Felix, Venezuela), Stinchcombe provocatively reformulates in the following manner the issue of the role of the entrepreneur in economic development. (1) Today, the locus of "entrepreneurship" lies in industrial bureaucracies with sophisticated technology,



not in ready-to-blossom, one-person businesses. (2) The key process is not the inventing of something new, but the solving of problems which arise in the course of running that complex technology. (3) Since this technology comes from outside the developing countries, keeping abreast of the literature from the industrial countries is critical. (4) Rational analysis, thought, and reading—the capacity and motivation to engage in them—are the critical activities on which analysis must focus. (5) The greater or lesser presence of these activities in a society or an organization does not depend on the distribution of certain personality characteristics, à la McClelland but on the enterprise's having a structure in which the staff and senior executives are relatively free from day-to-day supervisory responsibilities and are motivated by the reward of promotion ("career organization") on the basis of merit.

And what are the conditions for setting up such organizations? They are the same as those governing the successful pursuit of economic development in general: a state ready to redistribute power, income, etc., in favor of the underprivileged (a piece of very fast reasoning, engaged in at the beginning of part 4 of the book, which I found intellectually unconvincing though morally satisfying). The attainment of these policies, in turn, hinges on their acceptance by the middle class. And what, in turn, determines *that*? Why, the determinant is having a democratic system and a presidential candidate who appeals to the middle class but needs some lower-class votes to win; such a candidate, therefore, incorporates some egalitarian planks into his platform and thereby reconciles his middle-class followers to losing some of their power and privilege. May that *deus ex electoral machina* have mercy on our souls (to slightly rephrase Stinchcombe)—it sure didn't work in Chile. Nor, on the other hand, did Germany, Japan, the USSR, China, and Cuba need it. And furthermore, strongly organized left-wing parties of the poor aren't necessarily satisfied with middle-class concessions (the poor may be, but the party leaders may not; and what with extremists of both sides unhappy, rising expectations all around, and so on, a system may be undermined quite rapidly).

There are many weaknesses in this book, if it is looked at closely. "Thinking," measured by retrospectively reported time budgets (using very shaky samples), left me concerned; so did the items making up the career organizational scale used to measure the cause of effort (effort itself couldn't be measured). I never did understand whether organizations caused cosmopolitanism, attracted cosmopolitans, or what (the critical words kept changing). How the right organizations get set up, by whom, how, and why is certainly not clear. And until I can be convinced that noncoercive socialism won't result in more operations of the kind Stinchcombe found in the driver's license bureau in Venezuela, I'll remain what I have been (as he has been) for years—a democratic socialist by instinct and moral conviction rather than on the basis of empirical evidence.

Seriously, don't mind the gaps in this book. What's important and excellent are the basic ideas and the attempt to test them. But don't quote the book as if it contained well-established findings, either.

*Building States and Nations: Models, Analyses and Data across Three Worlds.* Edited by S. N. Eisenstadt and Stein Rokkan. Vols. 1 and 2. Beverly Hills, Calif.: Sage Publications, 1973. Pp. 397 (vol. 1); pp. 506 (vol. 2). \$17.50; \$20.00.

Guenther Roth

*University of Washington*

The International Social Science Council, which sponsored these two volumes, was established by UNESCO in 1951, when the massive American effort at aiding the economic and political recovery of Europe and Japan was still under way; academic concern with economic, social, and political development was also just emerging. The Cold War raised the issue of reconstructing or developing countries so that they would follow a pluralist Western instead of a monolithic Communist path. In the fifties, "newly developing" was a term politely chosen by many Western writers to avoid the demeaning overtones of "underdeveloped." But today there is a popular theory that Western development ("the core") has been based on underdevelopment in the former colonial or semicolonial areas ("the periphery"). "Underdeveloped" has become an anti-imperialist battle cry and almost a badge of honor; a negative meaning has turned into a positive one, as has so often happened in the history of protest movements. Gone are the connotations of cultural and social inferiority and even, partly, of inadequate political and military might. A new pluralism has supplanted the alternative of capitalist versus state-socialist development and also the analytical dichotomy of tradition versus modernity. So-called intermediate stages have turned out to be more enduring entities than formerly envisaged.

The two volumes under review reflect the changes in perspectives—the illusions and disillusion—over a quarter of a century. The volumes are the result of several conferences and "confrontation seminars," in particular, of a meeting in Sweden in 1968 and another one in Normandy in 1970. In the first volume, "the editors have assembled a series of general discussions of models and concepts in the study of macro-political change and added a number of papers on the data resources for comparative research on such processes. The second volume presents a series of papers on major variations in political development across the world" (p. 7).

It is not feasible to review all of the two dozen contributions, which vary considerably in quality, as must be expected from the nature of such a wide-ranging symposium. With due apologies to the other contributors to the first volume (R. Kothari, D. L. Sheth, W. Zapf, P. Flora, and E. Allardt), I will refer only to the contributions of the two editors, since they provide most of whatever viable framework the two volumes yield. In a lengthy introduction, Stein Rokkan summarizes the Swedish conference on European developments in a global perspective under the heading of "Centre-Formation, Nation-Building, and Cultural Diversity." In another chapter, he deals with cities, states, and nations by means of

a "dimensional model for the study of contrasts in development"; and in a third article, he reviews data resources for comparative research on development. Rokkan also supervised the assemblage of the selective bibliography of the research literature (120 pp.). The other editor, S. N. Eisenstadt, provides a postmortem for the early development theories, with their assumption that the development process keeps going once some threshold has been reached. He points to our increased awareness of the importance of tradition and historical continuity in shaping the direction of change and addresses himself to "the systemic viability of the so-called transitional systems," explaining that "perhaps one of the most important—albeit somewhat recent—developments in this context was the growth of the 'patrimonialism' concept to describe the political regimes of several new states" (p. 4).

In volume 2, the notion of patrimonialism is given explicit treatment in the essays by Simon Schwartzman on regional contrasts in Brazil and by Stuart Gellar on West Africa. The other contributions make up a medley covering eastern and western Europe, the Americas, Asia, and the old and the new Africa: H. Daalder deals with consociational nations (Switzerland, the Netherlands), N. Pasic with the Balkans, and K. D. McRae with empire, language, and nation in Canada; W. Bell discusses the Caribbeans from the perspective of his elite studies; J. A. Silva Michelena presents underdevelopment theory for Latin America; J. Watanuki compares Japan and Korea; R. Mukherjee diagnoses the Indian subcontinent; C. Heng-Chee and H. D. Evers treat national identity in South-east Asia; A. Zghal is didactic on feudalism and the Asiatic mode of production in explaining the divergent uniqueness of the Maghreb; J. Goody deals with the uniqueness of cultural conditions in Black Africa, W. Foltz with boundaries and competition in tropical Africa, U. Himmelstrand with tribalism, regionalism, and secession in Nigeria, and, finally, A. Mazrui with traditional cleavages and efforts of integration in East Africa.

The most substantial monographic contribution is by Juan Linz, who has added to his previous intensive studies of Spain an inquiry into "Early State-Building and Late Peripheral Nationalisms." Although marred by poor editing, it is a tour de force which investigates cumulative historical causation over four centuries and demonstrates the ways in which the multilayered conflicts between religion and secularism, center and periphery, and between various agrarian and industrial classes have overlapped and thus made of both state and nation building unresolved and perhaps irresolvable political issues.

*Elites in the Policy Process.* By Robert Presthus. Cambridge: Cambridge University Press, 1974. Pp. ix+525. \$19.50.

Allen H. Barton

*Columbia University*

Research on elites is moving from a preoccupation with who occupies elite positions to the study of elite attitudes, interaction, activities, and

effects on society. Robert Presthus has interviewed 1,404 interest-group directors, 518 legislators, and 472 bureaucrats in the United States and Canada, in the national capitals and three state capitals each, asking about attitudes, interactions, and activities. He has two aims: "first to describe and explain the structure, process, and political effectiveness of interest groups, and to determine the social backgrounds and political values of their directors and of governmental elites, as well as the patterns of interaction and influence between them. . . . Equally important we want to explain the larger role of interest groups in the North American political system. . . . In terms of functional theory, how requisite are they for maintaining the stability and continuity of the social and political system?" (p. 40). This enormous effort has produced abundant descriptions and correlations (some chapters read like a codebook and a correlation matrix turned into prose), but it has not shown how to use surveys to measure the consequences of interest groups for social systems.

In chapters 3-7 the unit of analysis is the interest group, with data provided by the executive director of each of 1,404 organizations. These data include: type of interest represented, the geographical area in which it operates; the personal characteristics of the director; size of membership, staff, and budget; which government offices receive the most interaction; tactics used to influence government; frequency of interaction with bureaucrats and politicians; and the director's estimate of the acceptance of his group by government personnel and the effectiveness of its lobbying activities. In chapters 8 and 9 the responses of bureaucrats and legislators are used to analyze the determinants of their interaction with interest groups in general, their perceptions of the legitimacy of and functions served by such groups and their lobbying, and the extent to which they felt they were influenced by such groups. Unfortunately, the bureaucrats and legislators do not seem to have been asked about their contacts with or responses to the specific interest groups whose directors were interviewed, or even the various categories of interest groups. It is therefore not possible to combine reports of interaction, influence, and legitimacy from both sides of the interaction in order to characterize the organizations as units.

Presthus finds that, of the organized interest groups (those listed in the telephone directories of capital cities and meeting certain minimal requirements of staffing), one fourth are engaged in "sustained consultation" with legislators and senior bureaucrats, providing them with information on group needs and technical problems. He shows that interest groups are an integral part of the policymaking and policy-administering process, not only in the United States but in Canada, where many have assumed that the Cabinet and the civil service monopolize these processes.

However, the study reports nothing about the consequences of this interest-group involvement, either on policy outputs or on the interest groups themselves. For all the talk about "elite accommodation," we aren't told whether "effective" directors or their constituents feel less alienated. The substantive nature of the policy demands made by the interest groups

is not analyzed, let alone their effect on policy outcomes. "Effectiveness" of each group is measured only by self-ratings.

A functional analysis of the interest-group system might be expected to ask how representative the influential groups are. The author repeatedly notes the weak lobbying position of organized labor and the greater resources of business groups compared with consumer or "altruistic" groups. But no systematic effort is made to measure how well represented are various categories of the population and types of institutions in the society.

If one is concerned with accommodation of interests to achieve social stability, there should be some analysis of the lines of important social conflicts, and groups should be classified by their power to contribute to instability or stability. Yet in Presthus's analysis all groups are weighted equally, and we learn nothing about the main lines of social cleavage—are they between capital and labor, dominant and deprived ethnic groups, centrist versus right ideologies, producer versus consumer interests, or is society divided into hundreds of competing interests without any larger structuring? Nothing is said about accommodation *between* groups—alliances to support claims on government or to oppose interventions, or implicit agreements not to go "too far" in intergroup conflict.

The discussion of "bases of elite accommodation" (chaps. 10–12) begins by showing that governmental and interest-group elites are socioeconomically homogeneous in that most are highly educated and have careers in high-prestige occupations. However, other elite studies find that socioeconomic background measured in these broad terms has little to do with positions on major issues, compared with present institutional roles. "Affective, ideological, and cognitive cohesion" are measured in a very misleading way: by the extent of difference in attitude by region of the country and political role (legislator, bureaucrat, interest group) without regard for party or economic class. Presthus concludes from the absences of differences on these two dimensions that there is high elite consensus. Yet the marginal distributions show a wide dispersion of attitudes, which probably reflects cleavages along party, economic, and institutional lines.

The final substantive chapter explores the determinants of "effectiveness" of interest groups in general, as measured by reports of legislators and bureaucrats on how often they have changed their positions as a result of interest-group communications. The main predictors of reported influence on legislators are favorable attitudes toward interest groups and the informational functioning of lobbying; this sounds more like a halo effect than a substantive finding. A potentially useful attempt to use Homans's propositions on the mutual effects of interaction, sentiments, and influence results mainly in inconsistent and negative findings. This may be due to weak measures, which ask about interest groups in general rather than specific types of groups, and influence in general rather than influence on types of issues. (Among other problems, the measure of interaction with interest groups for U.S. legislators places almost 90% in the "high" category.)

A good many technical problems exist. Many conclusions are based on a set of "case studies" of attempted influence—each director was asked to describe one such case from the last five years. But each case is not randomly selected (as claimed on p. 170), but respondent-selected. A typical case involves going to higher levels of government than the respondent usually contacts. Thus certain conclusions—for example, that most influence attempts are innovative rather than status-quo oriented—are based on an unrepresentative sample of such attempts. It should not be impossible to inventory recent influence attempts for each respondent and take a true random sample.

Tables are poorly labeled; the book needs a glossary of variables and their conceptual and operational definitions; percentages are reported based on 5, 4, 3, 2, 1, and even 0 cases. Table formats are hard to read—many would be better as collapsed three-variable tables so that the relative "effects" of the two independent variables could be easily read. The author says that "multiple regression analysis provides an alternative test [to zero-order correlations] of the relative weight" of independent variables. But the use of stepwise multiple regression can result in misleading estimates of effects, since those entered later can have most of their effect assigned to correlated variables which were entered earlier. The direct effects could be better measured by standardized regression coefficients which are not influenced by order of entry, while indirect effects require information on time order and the use of path analysis.

In his conclusion Presthus finally gets to the issue of policy outputs, noting that, despite the ideological preference of their political elites for limited government, both the United States and Canada are practicing "big government" as measured by government budgets as a percentage of GNP. He suggests that politicians are responding not to ideology but to piecemeal influences of interest groups demanding subsidies and services. The conclusion is plausible, but one wishes that his 2,400 respondents had been asked not only whether influence took place but whether it was in the direction of new spending or regulations, and if so how much and of what kind. Then it might have been possible to link the micro-level of survey reports of influence to the macro-level of policy outputs on behalf of different sectors of the population and the institutional structure for the eight polities studied.

Presthus begins his book by saying that theorizing has run too far ahead of empirical research in this field and describes his study as "empirical, exploratory, and tentative." This is a fair summary—it is like a big pilot study, in which a very limited part of interest-group theory is crudely operationalized and analyzed in ways only suggestive of what needs to be done. As such it will be useful to those interested in elite research.

*Power in the City: Decision Making in San Francisco.* By Frederick M. Wirt. Berkeley: University of California Press, 1975. Pp. xi+417. \$14.95.

William L. Yancey

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The principal proposition underlying this analysis of San Francisco's power structure and decision-making processes is that the form and content of local decision making are in large part a consequence of the constraints of history and external influence.

The only original data generated by the author come from interviews with 34 community leaders, first in 1968 and again with subsamples in 1971 and 1973. These informants indicated that major decisions affecting the city were the result of temporary coalitions of major interest groups. The pluralistic character of the city's power structure is viewed as a function of the city's historical role as a commercial center of the west coast and of the increasingly complex interdependence of San Francisco with regional organizations and state and federal governments.

A full third of the volume is a description of the structure of local government and politics. The central argument is that San Francisco's power structure is constrained and fragmented. There are multiple processes through which decisions are made, each largely characterized by temporary coalitions reflecting the diversity of groups, the disagreement among major groups, and the continual emergence of new groups. The nonpartisan political system results in what may be labeled program or issue politics, where temporary coalitions of voters support a given candidate, only to fall away at the next election. The city charter, an outgrowth of the 1932 reform movement, divides the structure of governmental power among the mayor, chief administrator, comptroller, boards, commissions, an extensive civil service system, and the frequent use of the referendum.

The major events and issues reviewed in this volume concern the development of the city's commercial center and minority group politics in the 1960s. Central chapters provide anecdotal material about decisions related to the development of the central commercial area, that is, the Bay Area subway system, the building of high-rise office buildings, and development of a center for hotels, sports, parking, etc. F. M. Wirt argues that coalitions of business and labor, constrained by local interest groups, neighborhood organizations, and federal policies brought about a series of major decisions, most of which received popular support, all leading to the maintenance of San Francisco's position as a major commercial center. The chapters reviewing ethnic politics focus first on the "arrived" minorities (the Italians, Irish, and Germans) and second on the "arriving" groups (the Orientals, Chicanos, Indians, and blacks). Here Wirt suggests that the federal government, particularly through the war on poverty and the civil rights policies of the 1960s, added impetus to the new minorities' obtaining recognition, legitimation, and partial access to the means of power.

The impact of external influences on local community decisions is the central focus of this volume. Wirt argues that the development of the federal domestic program in the 1960s, the emergence of regional organizations, and the continued influence of the state government have limited local decision-making autonomy. Although he recognizes their importance, far less emphasis is placed on the commercial and financial activities which have tied the city to the national and world economies. The lack of a detailed elaboration of the commercial and productive functions of the city leaves the implication that these influences may be less important than external governmental influences. Yet even without a full documentation of the commercial interests, the inference that is drawn is not merely that external relationships shape the form of decision making but ultimately that there is little left to be decided. Thus Wirt writes, "what remains is a diminished autonomy, which operates only on leftover decisions—a *local game of residual decision making*" (p. 344).

A major weakness of the volume lies in the author's assumption that the loss of local autonomy is a recent phenomenon. Although this is a somewhat popular view among contemporary social scientists, evidence indicates that it may be incorrect. The limited historical data provided in this volume suggest that San Francisco's external ties have always been important. The city's initial development was intimately tied to its role in the shipment of supplies and gold; its continued development as a cosmopolitan city was tied to its position as a seaport and rail head. Beyond this there is the more general literature indicating that involvement in external relationships—particularly productive, commercial, and governmental activities—has been the defining characteristic of urban life. These activities have had significant impact on urban power structures. Thus, Henri Pirenne describes in detail changes in the governmental structure of medieval cities which resulted from their increased involvement in interurban trade. The large literature, historical as well as contemporary, on urban hierarchies, interurban relationships, and the impact of these relationships on the internal structure of cities is largely ignored in this otherwise convincing account of contemporary urban politics.

*Constituencies and Leaders in Congress: Their Effects on Senate Voting Behavior.* By John E. Jackson. Cambridge, Mass.: Harvard University Press, 1974. Pp. xi+217. \$10.00.

Heinz Eulau

*Stanford University*

This is a study of senatorial roll-call voting during the years 1961–63. There has always been trouble with studies of legislative behavior that rely solely on roll calls—the most notorious example being a "power index" on which the admittedly influential House Speaker, the late Sam Rayburn, scored exactly zero. More statistical and methodological sophistication will not change the inherent weaknesses of the roll call as an indi-



cator of either a legislator's "position" (vis-à-vis other legislators or on issues) or others' "influence" on his position. Simply too much happens in the legislative game before the yeas and nays are counted to make the roll call anything more than a starting point for inquiry. To treat the roll call as the end point (as it is, of course, in the real-world legislative process) is to invite failure. The failure is to confuse an explanation of roll-call outcomes with an explanation of the preceding legislative process. Not surprisingly, perhaps, statements made in this study about the legislative process are summations of what is generally known about the U.S. Senate and about legislative behavior or representation and only secondarily inferences from the data.

Roll-call analysis is at best an auxiliary strategy and can not be substituted for more direct research using interviews, committee hearings and reports, news accounts, and other documentation. All of this must be evident to the author, who has continual difficulties in explaining his explanations, so much so that theoretical statements, methodological pronouncements, and substantive findings constantly interfere with the logic of the argument he is trying to make. The difficulties are clearly due to the inappropriate use of roll-call votes in exploring or testing such familiar models of the legislative process as the organizational, representational, and coalition models (or more precisely, selected hypotheses derived from these models).

Even more disturbing than the use of roll calls is their manipulation in linear regression equations whose additivity is somehow assumed to be isomorphic with the real world and whose substantive validity is never doubted. The mechanical quality of the approach is matched by the author's didactic and tedious style (in this connection and throughout the book). In all of this, "influence" is inadvertently transformed from a statistical variable into a reified quantity as if constituencies, party leaders, committee leaders, the president, and senior colleagues or other informal leaders were in fact exerting measured influence in the political sense. The following statement is not atypical of the author's verbal presentation: "Mansfield's influence was actually more extensive than indicated by these coefficients. This can be seen by considering the set of senators not affected by Mansfield directly but who were influenced by Humphrey, who in turn was influenced by Mansfield. (Mansfield's coefficient in Humphrey's model is 0.33.) Thus, 18 of the 44 nonsouthern Democrats, excluding Mansfield and Humphrey, were directly responsive to the positions of these two leaders" (pp. 63, 65). And so it goes page after page until one's head swims with "influences" whose substantive political meaning or significance eludes comprehension. There is even more difficulty with the measurement of "constituency influence," which is evidently assumed to be operative if there is a relationship between senators' voting behavior and the demographic characteristics of their constituencies (states!). That there are such relationships we learned 75 years ago from A. Lawrence Lowell and 50 years ago from Stuart Rice, but whether such a statistical relationship denotes "constituency influence" in any meaningful political sense

remains as questionable today as it was then. Indeed, it is unfortunate that the author ignores the long debate about the measurement of power. While this debate has been largely fruitless, it might have saved him from the mechanistic, pinball assumptions he seems to make about legislative behavior.

*A Sociology of Belief.* By James T. Borhek and Richard F. Curtis. New York: John Wiley & Sons, 1975. Pp. xii+201. \$8.95.

Karl E. Scheibe

*Wesleyan University*

James Borhek and Richard Curtis have written a book of ideas about ideas (beliefs) and of the relevance of those ideas for social organizations, for the construction of meaning, and for social change. Among the ideas contained in the book is the following: "There is no way to invalidate an open-ended promise—it simply does not confront the real world" (p. 128). Since this book is avowedly an open-ended promise which does not confront the real world (in the sense that it is a theoretical work rather than an empirical one), the authors imply that no effective work of invalidation can take place on it. I accept this position. The worst fate this book could suffer is not invalidation but oblivion—being ignored. Such a fate would be unfortunate, for this is a worthy essay in the tradition of the sociology of knowledge.

The theory which is developed about beliefs and belief systems in this book is middle range in at least two senses of the word. Its focus is neither on individual beliefs nor upon universal belief systems but upon the belief aggregates of associations, concerns, cults, and formal organizations. Also, it is middle range in the sense of being neither a formal deductive structure based upon a small set of axioms nor a completely disjointed set of propositions.

Two sorts of theoretical work are described in these pages. First is the conceptual elaboration of the terms "belief" and "belief structure," including the rudiments of a taxonomy for beliefs. Second, the implications of this elaborated conception are drawn in the form of propositions, at least some of which are amenable to empirical analysis. But it is noted in the preface that the presentation of relevant evidence is not to be expected in this book. And there is none.

The authors are at pains to point out that the usage of the language of beliefs in this book is distinctly sociological and not psychological. They are not concerned with the acquisition, organization, change, or functional significance of beliefs for the individual. Rather, their concern is supra-individual, or cultural in the sense in which Durkheim employs the term. They note repeatedly that belief systems are not possessed by an individual and that beliefs are not "purely cerebral events."

Within the taxonomic account which is presented, the reader is apt to encounter some confusion. In their definitional statement, the authors

assert that "The conditions of permanence, commitment, and connectedness are *variable* characteristics through which we expect belief systems to be related to social organization" (p. 5). Yet in the next chapter, variable characteristics of belief systems are declared to be as follows: system, empirical relevance, willingness to take on innovations, tolerance, degree of commitment demanded, and style of organization of beliefs. While there is obviously some connection between these two lists of characteristics, it is not clear why they are not the same.

Some further taxonomic difficulty is presented by a listing of seven elements of belief systems. These elements are: values, criteria of validity, logic, perspective, substantive beliefs, prescriptions and proscriptions, and technology. One might worry about the conceptual consistency of this list of labels. But it is more troubling that nowhere are these elements related to the other taxonomic dimensions.

However, these quibbles about the classification of beliefs should not detract from what is an important accomplishment. The authors have managed to develop an idiom for talking about beliefs and belief systems in a very general sense, and to get that idiom "on the boards." The payoff is in the implications which they can draw from their tentative and provisional theoretical structure.

The major targets for application of their ideas are modern urban societies and the process of urbanization. The theoretical structure allows a fresh discussion of the traditional problem of the combination in urban societies of disorganized shared beliefs with close interdependence of highly differentiated functions. The discussion of urbanization is linked closely with a characterization of the process of the secularization of beliefs. In the course of this, we are invited to the following exercise in thought: "Think of two grandsons of a white Protestant Yankee farmer. One is a used car dealer in Lorain, Ohio; the other a minor executive for Procter and Gamble in Chicago. Both meet the ghost of Cotton Mather in their dreams, but each has adapted his beliefs for use in his present life-space, one with the work ethic and the other with the social ethic" (p. 171).

The reader is here invited to imagine Borhek and Curtis meeting with the ghost of Durkheim. You may imagine with me that the specter would be gladdened that neither had strayed so far as to have become a used-car dealer or to work for Procter and Gamble but were steadfast and true in keeping the faith.

*Foundations of Interpersonal Attraction.* Edited by Ted L. Huston. New York: Academic Press, 1974. Pp. xvi+422. \$16.50.

Phillip Shaver

*Columbia University*

It was enlightening for me, a psychologist, to read this volume with the intention of reviewing it for sociologists. Like an easterner's subjective

map of the United States, which supposedly shows the Midwest to be not much larger than Connecticut, a sociologist's view of this book (at least as I imagine it) might place more emphasis on the two chapters entitled "The Social Context of Attraction" (Alan Kerckhoff) and "Cross-cultural Perspective on Attraction" (Paul Rosenblatt) than on the six or seven chapters that summarize countless studies of college students' experimentally manipulated affection for each other. Unfortunately, whereas an easterner can be disabused of his misconception by looking at a real map, there is no objective way to compare the conceptual weights of diverse chapters in a sourcebook. One assurance I can offer: the editor of this volume, Ted Huston, has done an admirable job of organization and introduction and has chosen a wide range of expert contributors. Any differences in conceptual weight among chapters can reasonably be attributed to the materials dealt with and not simply to the authors.

Interpersonal attraction is defined in this book as "the affectional component of social relationships" (p. xv). So defined, attraction might be either an independent or a dependent variable; it might either create and maintain interpersonal relationships or arise out of relationships established by other social forces. In fact, attraction is viewed in both ways by most of the 20 contributors to this volume. One of five major sections is titled "Antecedents of Attraction," implying that attraction is due to specifiable causes; in other sections of the book, the authors either hypothesize or take for granted that attraction, once generated, is in turn responsible for such diverse outcomes as the time courses of interpersonal relationships and people's interpretations of what is socially just.

Attraction is to the social psychologist simply a special kind of attitude. In his introductory essay, Huston quotes Theodore Newcomb's definition of attraction: "any direct orientation (on the part of one person toward another) which may be described in terms of *sign* and *intensity*." Without the parenthesized phrase this is the definition of attitude. As Huston recognizes, "Several conceptually distinguishable sentiments (for example, gratitude, respect, like, and the various forms of love) fall within the purview of the definition, though attraction research has concentrated primarily on liking and its antecedents" (p. 7). "Liking" is, of course, another way of saying "attitude."

Definitional restriction is not the only way in which attraction researchers have oversimplified the study of attraction; for the most part, they have focused on heterosexual relations among college students in the United States. There is little in this book about same-sex friendships, friendships between children or between elderly people, and little about cross-cultural variations in friendship patterns. Moreover, even within the limited domain addressed by experimental researchers, the typical study has lasted less than half an hour and been concerned either with dyadic interactions between strangers or with college students' reactions to an opinion questionnaire supposedly filled out by an unseen person (actually filled out by the experimenter).

What has been learned from this kind of study? Very little, as far as

one can tell from this volume. About a third of the chapters are concerned with two main findings: when all other factors are held constant, subjects (1) like people who reward them and (2) like people who are similar to themselves in all sorts of ways. Gerald Clore and Donn Byrne abstract from these results what they call a law of attraction: "Attraction is a positive linear function of proportion of positive reinforcements" (p. 163). Further reading reveals that this "law" is essentially true by definition: people like what they like, and one of the things they generally like, all else being equal, is similarity. (The all-else-equal assumption is worth stressing. If I know nothing about two people except that one smokes and the other doesn't, I have a small warm spot in my heart for the nonsmoker, being a nonsmoker myself. In real life, however, where friendships are based on many factors, I gladly tolerate several friends who smoke.) Why is similarity rewarding? Various contributors speculate about this and together come up with an interesting list of possible explanations. I have no trouble believing that each is correct in at least some situations.

The general weakness in the reinforcement conception of attraction, granting that people "like what they like," is that we still need to know what they like and why. The door is thus opened to all sorts of ethnography, which provides some of the more interesting chapters in Huston's book. Thomas Lickona, relying on the work of Piaget, Kohlberg, and Loevinger, examines changes in patterns of attraction due to cognitive development. Says Lickona, "There are no affects divorced from cognitive structure, no attitudes that exist as pure intensities. . . . Liking and loving, like all other complex psychological phenomena, have their roots in the organization of thought." What Lickona does for cognitive structure, Kerckhoff does for social structure. Attraction occurs within a highly organized social context; two people can generally attract each other only if society has placed them in the same location at the same time and declared them to be suitable partners. Even the ubiquitous similarity-attraction phenomenon is limited to certain socially determined similarity dimensions. Rosenblatt adds an enlightening cross-cultural perspective to Kerckhoff's structural analysis—noting, for example, that intense romantic love occurs mainly in cultures that have few practical (e.g., economic) constraints on marriage choices. These chapters are provocative and sensible, although I wish that the last two had not been limited to mate selection.

Besides oversimplifying the differences between respect, liking, loving, and so on, the attitudinal conception of attraction requires too much from the notion of intensity. Is love merely an intense form of liking in the same sense that strongly agreeing with an attitude statement is different only in intensity from agreeing with it slightly? Three chapters are addressed more or less directly to this problem. All three deal mainly with romantic and sexual attraction between youthful males and females, a form of attraction which in this culture is supposed to precede marriage and is thought to be qualitatively different from liking.

George Levinger discusses three levels of relationship—unilateral aware-

ness (exemplified by most of the similarity-attraction studies), surface contact (most other laboratory experiments), and mutuality. Levinger shows that what is "reinforcing" depends on depth of the relationship, as does the appropriate definition and measurement of attraction. In another chapter, Zick Rubin summarizes research using his liking and loving scales, and shows that these two forms of attraction are not identical (each has its own antecedents and correlates). In one of the most unusual chapters in the book, "A Little Bit about Love," Ellen Berscheid and Elaine Walster dissect passionate romantic love with the aid of Schachter's theory of emotion, according to which all intense human emotions consist of (1) autonomic arousal and (2) cognitive interpretation (a label) based on situational cues. The arousal can come from such diverse sources as fear, anger, intrapsychic conflict, and natural or drug-induced excitement. The label, according to Berscheid and Walster, comes from a wide variety of culturally defined cues encountered in romantic movies, comic books, family stories, etc. This approach, unlike others advanced in the book, helps to explain the ambivalent feelings (e.g., love/hate) so common in romantic relationships and suggests why the experience and symbolic representation of passionate love, religious ecstasy, and insanity are so similar.

The remaining chapters are less easy to organize thematically and therefore more difficult to review briefly. They include "A Symbolic Interactionist Approach to Attraction" (George McCall), a summary of research on "Social Comparison and Selective Affiliation" (Ladd Wheeler), a discussion of the relationship between justice and interpersonal attraction (Melvin Lerner), and an essay on "Attributions, Liking, and Power" (James Tedeschi). Several other well-known interpersonal attraction researchers—Irwin Altman, Albert and Bernice Lott, William Griffitt, and David Mettee and Elliot Aronson—contributed chapters summarizing their work. The book is, therefore, a true "foundation" in one sense: it contains a comprehensive, accurate, sometimes stimulating, and generally sympathetic but critical review of research on interpersonal attraction. The middle ("reinforcement") section of the book could easily have been shortened; the result would have been a less expensive and more interesting volume. The speculative sections might have contained a psychoanalytic chapter; or are we certain that people do not "marry their mothers," feel jealousy toward sibling substitutes, and so forth?

Despite its weaknesses, *Foundations of Interpersonal Attraction* should obviously be consulted by anyone setting out to do research in this area. One hopes, when a successor to this volume is assembled, the psychologists' simple conception of liking (attitude) will have given way to a sophisticated, interdisciplinary account of the "affectional components" of interpersonal relations. If not, this book will make more urgent the major question now facing experimental social psychology: How can such fascinating and important topics—friendship, romantic attraction, love and hate—appear so thin and lifeless when characterized by experimentalists?

*Mental Health and Going to School: The Woodlawn Program of Assessment, Early Intervention, and Evaluation.* By Sheppard G. Kellam, Jeanette D. Branch, Khazan C. Agrawal, and Margaret E. Ensminger. Chicago: University of Chicago Press, 1975. Pp. xvi+213. \$11.00.

Melvin L. Kohn

*National Institute of Mental Health*

Kellam and his associates have elected to work in one of the most difficult areas in social psychiatry: carrying out and evaluating intervention programs in the community. This type of research calls for an unusual combination of talents: the imagination and personal (and diplomatic) skill to conduct novel therapeutic or preventive programs, together with the technical skill to assess these programs systematically and cold-bloodedly. Few people have the courage and ability to do both. Most who try to do so either fail to design and implement an intervention program of much inherent interest or fail to assess the program rigorously. This book reports one of the few investigations that is both conceptually well grounded and appropriately assessed.

The book is an impressive account of psychiatric intervention into the school system of a predominantly black and largely lower-socioeconomic-status urban community, to bring about significant improvement in the psychological well-being of its children. The rationale for concentrating on the school system is convincing; the intricate problems of enlisting community support and being responsive to community needs are handled with impressive sensitivity; the experimental design is sensible; the measures of mental health are meaningful; and the evaluation is carefully done. As an example of innovative psychiatric research in an urban community, this is an admirable study, a model of what is possible.

Both because of the investigators' own views of the appropriate role for community psychiatry and because of the explicit request of the leaders of the Woodlawn community, the authors focused on first-grade classes. To be able to evaluate the outcome of their program, they selected a group of six schools to which they provided consultative psychiatric service and a matched control group to which they provided no service. Since the crucial empirical questions turn on comparing changes in the "mental health" of the children in the experimental and control schools, a great deal of the book's attention is given to developing and assessing measures of mental health. Essentially, there are two: (1) social adaptational status, based on interviews with teachers; (2) psychological well-being, based on three independent assessments, namely, direct clinical observation by psychiatrists of each first-grade class in a standardized group setting, mothers' inventories of symptoms, and self-ratings by the children. The authors demonstrate the reliability of these measures, appraise their validity, examine their interrelationships, and estimate the prevalence of four noteworthy patterns of social maladaptation. While far from scintillating, all this is impressive for its precision, its explicitness, and its clarity.

The description of the actual process of psychiatric intervention is, by contrast, imprecise, general, and anecdotal. We are told a great deal about the rationale for choosing to focus on first-grade children but relatively little about what was actually done for and with the children. There were weekly consultations with teachers and school administrators. There were weekly sessions in the classroom with the children and the teachers. And there were some introductory meetings with parents. This is the "black box" of the study: something was done, but we cannot be sure exactly what, nor can we be sure that it was the same in all schools. To my inexperienced eye, much of it appears to have been group therapy, but just what type of therapy I cannot tell. Whatever was done, though, resulted in statistically significant, even if not very dramatic, differences between the experimental and the control schools in the mental health and academic performance of their children. In an arena where rigorous comparisons generally yield inconclusive results, this is an achievement of considerable importance. But one wishes that the reports of what went on in the sessions were specific enough to indicate which aspects of the program had which effects with which children in which contexts. It is frustrating to learn that the program was successful, but not to know why.

To a sociologist, there is another frustration in reading this book. Although the book displays all the technical acumen one could wish, it lacks something of the sociological perspective in its treatment of the data. There are interesting substantive findings throughout the book, some concerning the interrelationship of the children's social adaptation and their psychological well-being, others dealing with the relationship between children's mental health at the beginning of their school careers and their subsequent psychological development. But in their single-minded devotion to assessing the intervention efforts, the authors didn't wander off course enough to explore the relationships between social structure and the psychological well-being of the children. They should hardly be criticized for sticking to their core problem, but again and again one wishes that they had engaged the fascinating sociological and social psychological issues on which their inquiry touches. There is the material here, for example, for a longitudinal analysis of the social adaptational and psychological problems of black children in an impoverished urban environment. It would be unfair to suggest that such an analysis should have been included in this book; but, please, let us have it some place, some time. The data are much too good and the investigators much too capable to let the opportunity pass.

*Human Subjects in Medical Experimentation: A Sociological Study of the Conduct and Regulation of Clinical Research.* By Bradford H. Gray. New York: Wiley-Interscience, 1975. Pp. xviii+298. \$13.95.

Charles Bosk

*University of Pennsylvania*

Why physicians undertake biomedical research is easily understandable: science can be advanced, human suffering reduced, academic reputations



made, and careers secured. However, what motivates individual patients to accept the risk of being an experimental subject is less obvious—especially since a piece of research may serve to further each of the physician's goals and be of no benefit to the patient. The legal requirement that a physician obtain a patient's "informed consent" before participation in clinical research is the only guarantee society has that the physician will not take advantage of this situation in which he has everything to gain from a patient's cooperation and in which the patient may profit, lose, or barely hold his own. By examining how a major center of biomedical research fulfills the social contract for legal and ethical research—*informed consent*—Bradford Gray makes an important and original contribution to the sociology of medicine and science.

Gray's data on the regulation of research come from the deliberations of a peer-review committee on proposed research and interviews with the subjects of two experiments selected for the modesty of their potential risks and benefits for patients and of their gain for science. The description of the research site, identified simply as "Eastern University," suggests no reason to question the generalizability of this study's more disturbing findings. To the contrary, Gray presents evidence to suggest that regulation of research at Eastern was strict in comparison to other settings—a fact which, if anything, should increase the likelihood that the requirements of informed consent would be fulfilled and the rights of individuals respected. Such was not the case.

Operationally, for an individual's consent to be considered "informed," the patient must know what is experimental about a treatment, what are its possible risks and benefits, and what treatment alternatives exist. Further, the requirements of informed consent obligate a physician to answer a subject's questions and to inform him that he is free to withdraw from the research at any time. Strictly speaking, "informed consent" is all that distinguishes humane treatment from assault and battery. Of course, in everyday life, the lines between such distinctions are usually difficult to draw with precision.

Gray begins his analysis with a review of Eastern's peer-review committee. The law requires that to be eligible for federal research funds an institution have a committee that reviews proposals for research on human subjects. Such committees have two tasks; they determine whether the risk-benefit ratio of an experiment is proper, and they determine whether the consent form a patient is required to sign is sufficiently understandable to the average subject to assure "informed consent." Gray argues that the committee at Eastern is better suited for its first task than its second, but that, in any case, lay people should have some say about when risk-benefit ratios are proper or when the language of forms is understandable. But this is not the critical flaw in the control process. Gray argues that, whoever performs the task of review and however diligently they do so, review in and of itself is no guarantee of ethical research conduct. Review is prospective; there is no monitoring of projects in progress to see whether principle and practice coincide. Even the best of such committees—and

Gray believes that Eastern's is among the best—provide only the form and not the substance of regulation. The law does not require and the norms surrounding professional work positively inhibit the monitoring of research in progress. This means that once a review committee legitimates a project, it is left to the professionalism of the researcher to assure that the informed consent of subjects will be secured.

It is this assumption of the policy regulating research on human subjects that Gray dissects with much care and skill in the major part of his analysis. From his interview material, Gray documents the difference between a patient signing a consent form and a patient giving his informed consent to be a research subject. First, Gray examines how patients were recruited for research. Strong differences emerge between clinic and private patients. It appears that the poor, uneducated, and nonwhite clinic patients were more apt to be recruited to furnish researchers with needed warm bodies than were private patients whose recruitment was more strongly related to medical need or a strong patient desire for a particular intervention (in this case, labor induction).

This established, Gray turns to the decision-making process by which patients volunteer their cooperation. These decisions were made so easily and with so little deliberation that Gray appears awed at the depth of the individual's faith in her physician. When he examined the subjects' understanding of the element of choice in their decision to participate, he found it was extremely limited, despite their signature being on consent forms. Astoundingly, 39% of subjects in the research did not even know they were in an experiment until their interviews with Gray—all of which were conducted after informed consent had been obtained. The remaining 61% of the subjects showed a lack of understanding about the nature of the research—they did not understand its double-blind nature or its risk or all the procedures it involved.

Gray, then, develops a five-category typology of his research subjects. "Unaware subjects" are those that did not know they were involved in the research. "Unwilling subjects" were aware of the research and felt compelled to participate; these subjects felt refusal would have jeopardized their relationship with their physicians and compromised their future treatment. "Benefiting subjects" agreed to participate because of the direct potential gains such research provided. "Indifferent subjects" were just there, doing what the doctor thought best. "Committed subjects" believed in the importance of the research and participated for altruistic reasons. Forty-five percent were "unaware" or "indifferent"; these tended to be clinic patients, a captive audience more used by than users of the research. Forty-three percent of the subjects felt they were benefiting; these tended to be private patients who were able to use the project to gain their own ends (in this case, the quickest termination of pregnancy).

Next, Gray attempts to broaden the typology by ranking the factors that led to the decision to participate. This is the least satisfactory part of the analysis—but that is not really Gray's fault. His subjects seem so blithe about being in an experiment, so eager to accept medical authority,

that ranking the power of the factors in the decision is inappropriate: there's just not that much variance that needs accounting for; rather, it's the similarity in responses that is striking. Gray recognizes this and attempts to explain the incredible acquiescence of patients. Like the physician, the patient in the biomedical research situation has his own built-in role conflict. As a patient, he knows well what to do—follow doctor's orders. As a research subject, he has rights, responsibilities, and obligations; but they are unfamiliar. (Indeed, the medical division of labor does not charge any individual worker with communicating them.) The result is that the patient transfers the behaviors of the patient role directly to his status as a subject in the research situation.

What Gray has done, then, is to give us a richly textured analysis of why "informed consent" is so difficult to obtain in practice. On the one hand, there are the physician-researchers. In the world of academic medicine, it is whether you win or lose and not how you play the game that counts. This makes the signed consent form more important than the patient's fully informed consent which could only complicate and slow the research process. On the other hand, there are the patients, who do not understand because no one has told them that the physician recruiting them for research may not have their best interest foremost in his mind. Faith in a healer is necessary; faith in a scientist can be dangerous. As Gray shows, patients need to be educated about the subject role; and they need the wisdom to distinguish the scientist from the healer in their physicians so that they can distinguish the patient from the subject in themselves. Also, as Gray suggests, some retrospective or on-going review of their research might make the necessity for obtaining truly informed consent seem more compelling to physicians than at present.

This is a good book that raises a number of fascinating questions and does not shy away from their complexity. However, there is throughout an altogether unsubtle tone to Gray's exposition. He is too quick to see informed consent as a problem to be overcome and less ready to see it as a paradox of clinical treatment. It is not by any means clear to what degree a patient's trust in his physician is a necessary part of the healing process and to what degree hard, cold facts undermine this trust and the patient's belief that he is being cared for. Gray's defense would be, I'm sure, that this is precisely the point: the experimental subject is no longer being cared for, the same rules no longer apply, and it is the confusion of clinical and experimental situations that is an abuse of a physician's already considerable authority. No doubt this is so, but it makes the distinction between routine and experiment too rigid and fails to acknowledge the ways in which all treatment is experimental and probabilistic. Gray minimizes to an unhealthy degree the importance and uses of clinical judgment. Nevertheless, this book is a valuable addition to the literature, one which will be of interest to others besides medical sociologists. It raises questions about the uses and abuses of expertise, documents one more indignity that the poor and powerless are subject to, and recognizes the dangers of the division of social from moral labor.

*The First Year of Bereavement.* By Ira O. Glick, Robert S. Weiss, and C. Murray Parkes. New York: John Wiley & Sons, 1974. Pp. xvii+311. \$12.50.

Lyn H. Lofland

*University of California, Davis*

This is a book about the emotional and social journeys of 49 young (aged 45 or less) Boston area widows during the first year following their spouses' deaths. It is based on three or four intensive interviews with each, conducted some three weeks, eight weeks, 13 months, and (in 43 cases) two to four years after the deaths. The book is also, although only briefly and rather superficially in a single late chapter, about 19 young widowers who were interviewed at the same intervals, with 17 of the 19 available for the fourth and final encounter. These 68 people form a "residual" sample of the total of 349 survivors in the relevant age group whose spouses died in Boston of accidental or "natural" causes (not suicide or homicide) during an unspecified portion of the years 1965 and 1966—residual because they were left after the elimination of those who could not be located with reasonable effort, who refused to participate initially or later, or who proved ineligible or unsuitable. While the authors judge the sample to be quite representative of the universe, they overrepresent (perhaps not surprisingly, given what we know about the demography of death), at least for the population of the Boston area, lower-income, especially blue-collar, groups.

In many respects, this is an interesting and meaningful study. Organized primarily on a chronological basis and utilizing a rich and sensitive sampling of interview extracts, the book follows these widows from their initial encounter with the death announcement, through the ceremonies of leave taking, the post-ritual pain and loneliness, and the realignment of social networks, to their eventual creation of new and, for the most part, relatively satisfying patterns of life. Readers of the burgeoning literature on widowhood and the "grief experience" more generally will find only a few surprises (a reinterpretation of the process and consequences of "anticipatory grief"; evidence for "normal" prolongation of at least intermittent discomfort well through the first year, and often beyond, and for the commonness of "strange symptoms"; and the suggestion of a marriage-phobic response among those widows whose husbands' deaths were unanticipated). But the materials do add breadth, depth, and nuance to prior portraits, many of which have focused on older persons.

Readers who have attended primarily to the almost stereotypical criticisms of dying and death institutions in contemporary America, however, may be very surprised indeed. Bearing in mind the overrepresentation of lower-income groups, consider the following: (1) The widows generally felt that their husbands had received excellent medical care involving not only technical expertise but also genuine concern. (2) In the first week after the death, kin and friends surrounded the widow with a warm blanket

of assistance, support, and comfort. (3) The services of the funeral director were viewed with a high level of satisfaction; expenses were judged appropriate both as to financial situation and desire. (4) While the various ceremonies of leave taking did not much serve to assuage pain, they were sources of satisfaction and pride—as expressions of affect for the dead spouse, as symbols of the widow's continued capacity for organization and decision, and as settings for her expression of appropriate deportment under stress. (5) And with the exception of those widows who had been in economic difficulty prior to the death of the spouse, none experienced severe financial problems during the first year.

I have said that in many respects this is an interesting and meaningful study. And yet—perhaps this is a matter of personal sociological taste—it is also profoundly dissatisfying. Despite the many extracts, despite the absence of tables, the book reads more like a report of rather superficial survey research than like a careful, detailed analysis of qualitative materials. The authors are more concerned with *describing* at a fairly concrete level what happened sequentially during the year than with identifying or *analyzing* the subtle processes surrounding death which their own materials suggest were at work—more concerned, that is, with biography than with sociology. For example, one wishes for a more systematic treatment of the strategies which the widows used to deal with their own unfamiliar and often frightening emotional experiences. Or for greater attention to the processes by which expectations relative to both grief and mourning were developed, communicated, complied with, or resisted. Or for a bracketing of the concept of grief itself. And so forth.

Dissatisfactions aside, this is certainly a book to which we “death freaks” in sociology and psychology must attend.

*Circle of Madness: On Being Insane and Institutionalized in America.* By Robert Perrucci. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1974. Pp. xiii+176. \$7.95 (cloth); \$2.95 (paper).

Andrew T. Scull

*University of Pennsylvania*

Sociology, as Sorokin once observed, is a discipline replete with fads and foibles. Nowhere is this more evident than in the sociological study of mental illness, where (doubtless reinforced by the preferences of funding agencies) researchers have proved remarkably adept at tailoring their interests to the fashions of the moment. In the late fifties, for example, social class (or at least mainstream American sociology's version of social class) was undoubtedly “in,” and we were regaled with a series of studies dealing with the relationship between class and mental illness. Time passed and with it the subject's earlier appeal. Nowadays an interest in social class is distinctly *démodé*, and we've turned to exciting new areas like sex roles.

The structure and functioning of mental hospitals is another issue

which once drew considerable attention. In the 1950s, scarcely a year passed without at least one addition to the list of weighty monographs on the subject, Erving Goffman's *Asylums* being at once the most widely known and one of the last of the genre. Robert Perrucci's new book is thus something of an anachronism, for this is precisely the subject he has chosen to reinvestigate. One must therefore ask whether his work has contributed much that is new or insightful, or whether it represents simply a rehashing of old themes.

Perrucci has obviously been heavily influenced by the more extreme versions of the labeling perspective and perhaps also by the work of Thomas Szasz (though throughout the discussion of mental illness as a "problem in living," Szasz remains, strangely, unacknowledged). Mental patients are seen as victims—victims of contingencies and of their own powerlessness, of their families and of professional "experts." Mental hospitals constitute one of our society's nasty little secrets, warehouses which function as custodial dumps for the useless and unwanted—a fact which is disguised by enormous effort not just from the community at large but also from the institutions' inmates (both the "patients" and the staffs of "dirty workers"). "Madness," then, is seen as "a communal affair. It is created, stabilized, sustained, and discarded by groups of people and not by the person who is presumed to be ill" (pp. 18–19).

The book begins with a cursory historical review of the treatment of the mentally ill, focusing on the reasons for the adoption of confinement as the primary means of managing the mad. The account presented rests heavily on the work of three men, Michel Foucault, David Rothman, and George Rosen; it provides little in the way of criticism or an independent stance and is further marred by a lack of historical understanding and a weak grasp of the materials reviewed. By contrast, the remainder of the introductory section is a competent, if unexciting, review of the basic features of the mental hospital as a people-processing organization, stressing that individuals "are not in the hospital because they are mad, but because they have been rejected by society and have no suitable place in it" (p. 30).

Part 2 examines the internal organization of the hospital in somewhat more detail. Drawing on his fieldwork at "Riverview State Hospital," Perrucci describes the nature of routine existence in such a place and distinguishes a number of ideal-typical adaptations to ward life—withdrawal, accommodation, conversion, and rebellion. Subsequent chapters deal with intraorganizational stratification, focusing on staff-patient and staff-staff relationships. The mental hospital is presented as an organization characterized by blocked mobility at all levels of the hierarchy. In this situation the importance of contact across caste lines, which functions as a substitute for promotion, is naturally accentuated. Perrucci suggests that understanding the need simultaneously to reduce and to sustain social distance is the key to grasping the basic dynamics of staff-staff and staff-patient relationships. Finally, he notes some of the "therapeutic" implications of the overriding concern with status protection.

The third, and last, section of the book considers the release process. While in practice "the hospital is nothing more than a 'storehouse'" (p. 113), this is a picture neither the staff nor the patients can readily accept. Accordingly, each constructs and attempts to defend, in the face of a reality which threatens its daily disconfirmation, an ideological account of the release process, "belief systems which make their respective lives tolerable" (p. 129). Inevitably, there are moments when such belief systems break down (most often characterized by collective disturbances of some sort); and, just as inevitably, the ideologies are laboriously reconstructed.

As this brief catalog should suggest, at least to those familiar with existing literature on the structure and functioning of mental hospitals, all of this is depressingly familiar ground. Perrucci does make extensive use of field notes derived from a year of observation "in a large state mental hospital in a Midwestern state" (p. 20), but while these provide some additional support for the conventional wisdom, they generate little in the way of original insight. Indeed, much of his analysis can be read as a kind of derivative version of Goffman, competent enough, and based on slightly more recent research, but lacking the latter's style and wit.

*Interaction and Social Structure.* By Orvis Collins and June M. Collins. The Hague: Mouton & Co., 1973. Pp. 208. \$16.50.

Murray Webster, Jr.

*University of South Carolina*

This is a theoretical monograph which attempts to integrate several diverse fields of sociological inquiry: linguistics, Goffman-like study of situated meanings, and social anthropology. Further, the authors attempt to use conventions of explicit, axiomatic theory construction to clarify their presentation and facilitate extension of their work. Both the integrative and theoretical tasks are worthwhile, and though the authors do not fully succeed, the book represents a worthwhile step in the direction sociology should be moving.

The Collinses build upon and extend a conception of *interaction* which they trace to Eliot D. Chapple and Conrad M. Arensberg, who wrote on the problem in the late 1930s and early 1940s. (This view or its variants has a history of many proponents in linguistics prior to about 1965.) Interaction is viewed as a discrete series of *acts*, roughly equivalent to irreducible meaningful communications, and usually taken as single sentences. Interaction then is two or more coordinated acts, *initiation* and *response*. An interaction is *dyadic* (between two actors where  $A_1$  originates and  $A_2$  responds), *triadic* (where  $A_1$  initiates,  $A_2$  responds by directing his act to  $A_3$ , and  $A_3$  responds to  $A_2$ ), or *nonresponse*. Next the authors distinguish random sequencing of acts (such as sentences which seem unrelated in meaning) from sequencing which is nonrandom (meaningful and related) and seek to specify the conditions under which nonrandom sequencing

occurs. Nonrandom sequencing and the rules which govern it are the basis of a social structure of meaningful communications.

While the Collinses attempt to develop a coherent and explicit theory, and in fact mention several logical and philosophical criteria which theories should meet, the main problems with their work seem to come from their not understanding the criteria they cite. For instance, they include a short essay (pp. 27–34) on the importance of *abduction* in theory construction and say that they plan to follow this program. But abduction involves a two-stage process: (1) generalizing and abstracting from evidence to formulate propositions; and (2) deriving and testing new consequences to guard against faulty abstraction and overgeneralization. There is no evidence here of stage (2), the empirical testing which is essential to the abductive process.

And indeed, there is little prospect that others can derive consequences for testing until the Collinses' work has been extensively reformulated. To begin with, many parts of the theory are misidentified. The basic elements, the "assumptions," often are not assumptions at all but orienting statements or meta-theoretical assertions (assumption 1 on p. 65: "An act is unique") or scope conditions (assumption 2 on p. 67: "An actor issues one act at a time") from which no empirical consequences can be derived. Likewise, some of the "definitions" are probably assumptions (definition 1 on p. 64: "An act sets forth an observable gesture"). And toward the end of the book there are sentences called "axioms" and "propositions," both of which are usually considered equivalent to the term "assumption." The propositions, at least, may be intended as derived empirical consequences (e.g., proposition 6 on p. 148: "An origin transforms a rule into a partitioning of the response field of that origin"), but the derivation is not made explicit, nor are adequate operational definitions provided for the terms. Other examples could be cited, but they would not add to the point that considerable theoretical recasting must be done before we will be able to assess either the logical consistency or the confirmation status of this theory.

In view of their extensive citing of rules of theory construction, the Collinses make some surprising statements in this text. For instance, on page 64 (n. 2) they say their definitions are intended as "real," not "nominal." Real, or essential, definitions are claims about reality (as in the question "Is the United States *really* in a recession?"), and because of the difficulty of establishing such a claim, only nominal definitions are used in most contemporary theory construction. Real definitions have the same logical structure as assumptions (material implication), while nominal definitions are simply convenient replacements (as in "Let us define a recession to be unemployment over 8%"). If the Collinses do intend their definitions to be real, they eliminate the possibility of replacing or condensing the huge number of terms they use. More surprising to me is that after embracing the Hempel-Oppenheim definition of an adequate explanation (p. 112), they claim "he clinched his argument with an analogy" (p. 143). Overall, while I applaud the attempt to build an



adequate explicit theory—and I wish more people would make the attempt—I also wish the Collinses had studied theory construction a bit more.

In spite of its faults, this monograph has several good points. Substantively, the attempt to explain social structure as an outgrowth of meaningful communications offers the prospect of uniting several diverse fields of study. Moreover, a critic must recognize that it is far easier to find flaws in someone else's theoretical writings than to construct one's own. We have to start somewhere, and the Collinses are to be commended for undertaking this task. For the student of linguistics or theory constructions, this book is worth a careful look, for it does contain some good ideas clearly stated.

*Hierarchy in Organizations: An International Comparison.* By Arnold S. Tannenbaum, Bogdan Kavčič, Menachem Rosner, Mino Vianello, and Georg Wieser. San Francisco: Jossey-Bass, Inc., 1974. Pp. xxii+248. \$12.50.

J. L. Bouchet

*Stanford University*

This book reports the results of a program of research carried on in five countries: Austria, Israel, Italy, the United States, and Yugoslavia. The social and ideological conditions of these countries differ widely and have strong implications for the operation of industrial organizations in terms of ownership, formal decision-making structure, and distribution of rewards.

The aim of this research is to investigate (1) the degree to which emphasis on hierarchy differs across countries and (2) the extent to which dysfunctional aspects of hierarchy (such as alienation, conflict, and frustration) are reduced by a high degree of formal participation and equalitarianism among members.

The authors give a concise description of the Yugoslav and kibbutz systems of management which are both socialist in orientation. The Yugoslav system is dominated by state or social ownership of the means of production and an ideology of workers' self-management. Members' participation is formalized in the Workers' Council, which controls key decisions such as production plans, prices, investments, allocation of profits, distribution of wages and salaries, and hiring and firing of employees including top management. The kibbutz system operates under collective ownership and a norm of equalitarianism including nonpersonalized monetary rewards. Although its production is only a small fraction of Israel's industrial output (6.6% in 1972), the kibbutz represents a radically different system of management well adapted to a small scale of operations and a closely knit social structure. Direct participation is formalized in a Workers' Assembly and election to management committees. Participation of the workers is enhanced by a higher level of education compared with the

other countries, and opportunities to improve their managerial skills through involvement in the different functions of the kibbutz. The Italian, Austrian, and American systems adhere to a capitalistic orientation and, despite local differences, resemble more closely the traditional bureaucratic model.

The data come from a survey of 50 industrial plants (10 in each country), of different sizes and involved in various product lines (plastics, nonferrous foundry, food canning, metal works, and furniture). The design of the survey questionnaire involved the close participation of the five national investigators, and great care was exercised in checking the meaning of the items in different languages. However, the authors recognize the possible existence of a cultural bias in this type of comparative analysis. The data also include formal descriptions of the organizational structures of the different plants so as to provide a check against the reports from the respondents.

The major findings of this research involve cross-country comparison concerning the following:

1. *Differences in decision making and control.*—Kibbutz and Yugoslav plants are highly participative both formally and informally while Italian, Austrian, and American plants have a stronger hierarchical orientation, Italy being the closest to the nonparticipative end of the spectrum. This is confirmed by indices of distribution and total amount of control (A. S. Tannenbaum, *Control in Organizations* [New York: McGraw-Hill 1968]): Kibbutz and Yugoslav plants have flatter, more equalized distributions of control with more total control than in Italy or Austria but less than in the American plants.

2. *The degree of emphasis on hierarchy.*—This is indicated by the magnitude of gradients on different measures (attitudes toward advancement, perceived benefits from and requirements for advancement, authority and influence, opportunities on the job, physical qualities of the job, salary, age, level of education, etc.). A flat gradient on a particular measure means that there is little difference across hierarchical levels for that index, or, conversely, the steeper the gradient, the stronger the "hierarchy effect." As expected, the more participative organizations of Yugoslavia and Israel present flatter gradients while Italy consistently has the steepest gradients on a number of measures. Considering these gradients as descriptors of hierarchy leads to interesting remarks on their relationships with plant size and length of hierarchical chains. For example, the authors note that "short chains have sharper gradients than long, and as a consequence, small organizations have steeper gradients than large. However, large organizations are not predictably different from small in the steepness of their hierarchical gradients when chains of equal length are compared. This contradicts our intuitive expectation that large organizations and tall hierarchies imply steep gradients." This formalization will be of great interest to the student of organizations.

3. *The differential impact of hierarchy on organizational members.*—This is assessed by indices of satisfaction and attitude toward the enter-

prise, motivation, psychological adjustment, and perception and ideals regarding aspects of the enterprise and members' roles in it. The hierarchical gradients associated with these measures are flatter under conditions of participativeness and de-emphasis on hierarchy.

Both hierarchical and nonnational effects are more pronounced in small plants than in large ones. Differences among the large plants in the hierarchical gradients and in the degree of participativeness are not as simple and clear as among the small. The authors acknowledge this fact and propose as an explanation a "logic of industrialization" argument: technological and administrative constraints in larger organizations seem to overcome the ideological orientation.

On the whole, this book provides insight for those interested in cross-national research. I was impressed by the rigor of this work: conducting research in five different countries involved coordination problems which were resolved by three basic elements: (1) sound planning, (2) cooperation of national investigators, (3) measuring instruments applied consistently across countries. This is sufficiently rare that this work could well become a model of multinational comparative research.

This book also provides theoretical insight into the effects of hierarchy. Those already familiar with Tannenbaum and his associates' work on control in organizations will find that the present formulation further develops a conceptual framework that has already proved fruitful. A wider variety of concepts and operational measures are used, and this permits Tannenbaum to relate his earlier concept of control to other structural measures. This yields support for the earlier studies and should also serve to overcome some criticisms of the earlier work (e.g., C. Perrow, *Complex Organizations* [Glenview, Ill.: Scott, Foresman, 1972], p. 141). However, the nonnational dimension of this research does not shed as much light as desired on the theoretical argument relating size, length of hierarchical chains, and hierarchical gradients. When checked across plants within the same country, the results are weaker than cross-nationally.

This type of research is confronted with a trade-off between (1) a cross-national design which maximizes differences between organizations at the risk of confounding hierarchical and cultural effects and (2) a national design which bears the risk of finding too little variation in the data. The authors have clearly made the first choice. In addition to revealing cross-national effects, their work enables us to formulate new questions on the effects of hierarchy which could be best investigated nationally.

*Urban Policy and Political Conflict in Africa: A Study of the Ivory Coast.*  
By Michael A. Cohen. Chicago: University of Chicago Press, 1974. Pp. x+262. \$14.75.

Richard E. Stryker

*Indiana University*

Michael Cohen's study of the Ivory Coast is an important addition to the growing body of literature on public policy in Africa. His concern is un-

ambiguously with the "have-nots," the urban poor of Abidjan. The interests of the vast majority of the city's population have not been taken into account, indeed have been systematically discriminated against, in urban policymaking. Cohen examines access to urban land, housing, jobs, secondary education, and social services and concludes that "the Ivoirien population has found itself excluded from many chances for improvement due to the restrictive character of public policy" (p. 69).

This situation must be understood in the setting of a country that has achieved remarkable economic growth (around 8% per year), and with respect to a city that has one of the world's most rapid rates of population growth (over 11% per year). One might well argue that to provide more widespread access to urban jobs, housing, and services would only further stimulate the rural exodus to Abidjan. There are some very real policy dilemmas here to which Cohen's analysis is not always sensitive, but unavoidable trade-offs are required, not only between growth and equality, but across a whole set of development priorities. In any case, the Ivory Coast does represent a rather extreme example of narrowly distributed growth.

The data collected by Cohen to trace patterns of inequality are detailed and imaginatively handled. Especially interesting is the examination of urban land concessions, which are increasingly concentrated among the "administrative class," Cohen's key target: "The extraordinary share of land given to officials [50% in 1970 compared with 12% in 1960] seems to defy any explanation other than that the members of the government take care of their own" (p. 45).

Rejecting the relevance of Marxist interpretations, Cohen contends that the critical class cleavage in the Ivory Coast is between the *rulers* and the *ruled*. Inspired by Ralf Dahrendorf's class theory, Cohen locates the origins of modern African social mobility and access to urban resources in political power. The "rulers" consolidate their position after independence through both political and economic aggrandizement, becoming increasingly conscious of their interests as a "ruling class." A complementary development proceeds among the "ruled." Failures on the part of government to meet the pressing demands of the urban disadvantaged lead to new types of political action based on coalescence around situational interests, such as housing conditions or lack of jobs, which gradually replace more diffuse ethnic solidarities. Public policy thus becomes *the* independent variable which explains both class formation and political conflict.

Cohen argues his theses cogently and forcefully. Neo-Marxists will not be alone, however, in objecting that his class analysis is incomplete because he confines the scope of his subject to the Ivory Coast, considered as an autonomous national system. This is a misleading delineation because the Ivory Coast economy is so dependent upon European capital and cadres that, as Samir Amin has insisted, it "does not have any real autonomy, it can not be understood apart from the European society which dominates it: if the proletariat is African, the true bourgeoisie is

absent, living in Europe" (Samir Amin, *Le Développement du capitalisme en Côte d'Ivoire* [Paris: Editions de Minuit, 1967], pp. 279-80). The indigenous administrative class, while clearly privileged, must be viewed in a larger context. The "rulers" do not act contrary to the interests of foreign capital to which their privileges are closely tied.

One need not fully accept this perspective to be concerned that Cohen's analysis does not come to terms adequately with the realities of neo-colonialism. His "proposals for change" in the final chapter reflect this limitation. Each of the proposals—for decentralization, for "Ivoirisation," for more employment- and welfare-oriented development projects—is thoughtful and "reformist," but taken together they run contrary to the neo-colonial foundations of the country. There is no indication in the prescriptions that external constraints might limit the reformist impulses of the ruling class or that quite radical changes might be prerequisite to such self-reliant, democratic, and egalitarian reforms.

*Urbanization as a Social Process: An Essay on Movement and Change in Contemporary Africa.* By Kenneth Little. London and Boston: Routledge & Kegan Paul, 1974. Pp. vii+153. \$10.00.

Paul Wheatley

*University of Chicago*

During the past three decades there has grown up a considerable body of literature dealing with the cities of Black Africa. Following pragmatic investigations of limited scope devised in response to the post-World War II needs of colonial administrations, these studies came to focus on a nexus of problems arising from the impact of colonial urban-industrial systems on the lives of tribal Africans. The authors of such works were concerned primarily to elicit theoretical orientations for the analysis of the city as a crucible for the fermentation of externally induced social change, including dissolution of kin-based institutions, the inception of social classes, and the emergence of new interest groups and associations. Urbanists working within this paradigm often viewed the urban systems of sub-Saharan Africa virtually as a laboratory in which hypotheses of social change could be replicated and tested.

The book under review provides a summary and interpretation of this literature in terms of a concept which the author calls "urbanization." But "urbanization" in this context is shorn of its demographic and spatially concentrative implications. As defined by Kenneth Little, it connotes "the process whereby people acquire material and non-material elements of culture, behaviour patterns and ideas that originate in or are distinctive of the city" (p. 7). It would seem, therefore, to incorporate both a purely behavioral component dealing with adjustments of personal conduct and a structural component focusing attention on the patterned activities of subgroups as they are absorbed into larger and more complex societies. When the outcome of the urbanization process is discussed, it is in terms

of urbanism as a way of life and as a set of functional centers of societal control, to the exclusion of the city as a built form.

The exposition of this theme is qualitative in character, cast in essay form, and relatively brief, comprising just over a hundred pages of text apportioned among six chapters, an "Introduction," and a "Conclusion." The author bases his argument on a wide range of data drawn eclectically from territories with diverse histories and varying colonial experiences spread throughout the southern half of the African continent. There are also 18 pages of expository "Notes," an "Appendix" tabling urban population totals for sub-Saharan Africa, and an analytical "Index."

In chapter 1 Little traces the way in which European capitalist enterprise, and with it the introduction of a money economy, induced the widespread voluntary cityward migration that, for many younger Africans, has become a modern form of initiation rite. This migratory basis of urban growth has produced town populations which are, typically, ethnically heterogeneous and relatively transient, in which adult males preponderate numerically over adult females, and younger age groups over older. And, of course, in colonial times it was responsible for the classic policy dilemma in which capitalistic demands for labor conflicted with governmental desires to maintain the integrity of traditional agrarian systems.

Chapter 2 is a discussion of the opportunistic undertones of contemporary urbanization as manifested in the ability of individuals to move easily between, and to manipulate to advantage, two contrasting social systems, and the resulting high level of occupational mobility (which, I suspect, probably make a more marked impression on a British investigator than on an American). Chapter 3 develops the same theme in an analysis of attitudes to work and wage employment and shows why the economic advantages of remaining permanently in the city are only marginal, except for those possessing relatively high educational qualifications. Chapter 4 examines the mechanisms by which population growth at the present time is generating two apparently contradictory processes, one (the more readily recognizable) tending to consolidate class stratifications, the other (often unnoticed) tending to maintain ethnic ties. In chapter 5 it is demonstrated that, after the attainment of independence, racial exclusiveness within both black and white communities was often replaced by social exclusiveness, so that, for example, race relations (at least in Little's opinion, and he is almost certainly correct) now constitute neither a social nor a political problem in Anglophone West Africa. Elsewhere, of course, things are often different.

Chapter 6 is an evaluation of the relative effectiveness of institutions, associations, cults, and movements in establishing sufficient social control to permit cooperation across, as well as along, ethnic lines, thereby enabling African towns to adequately perform their often somewhat specialized functions. In probing below the atomistic appearance of their social life and "the apparent confusion of the urban scene," Little attaches particular importance to A. L. Epstein's conceptualization of effective and extended networks of social ties: "new norms and standards of behaviour will tend

to arise more frequently within the *effective* network of those who rank high on the prestige continuum, and . . . through the *extended* network they gradually filter down and percolate throughout the society" (p. 80, quoting Epstein). To the role of voluntary associations in this particular process Little attaches less significance than have some previous authors.

Little is right in emphasizing that relative age and rate of growth are inadequate criteria for discriminating the sociologically traditional from the sociologically modern among African towns. He also points out that the idea of tribal societies progressively and inevitably becoming more and more Westernized is not always either useful or, indeed, legitimate. The most one can say, he concludes, is that there is taking place "a movement of a radical order which, fundamentally, is characterized by the exchange of smaller for larger settlement patterns." Little also faces squarely what is, for the Western urbanist, an awkward and intractable fact, namely, that not all African cultures effectively distinguish between town and country (pp. 98-100). Finally, while reinforcing Aidan Southall's notion of the town as the pacemaker for a wider society, Little emphasizes that, typically, the city itself is less a generator of change than a transmitter to the countryside of innovations developed within a system that is often far more extensive than the African continent. It is not without interest that in fulfilling this function the city is often underpinning the rural economy and so rendering traditional institutions more viable than they might otherwise be.

All in all, this lucidly written interpretive essay achieves for the sociology of African urban population growth (which is what the author means by "urbanization") precisely what the "Library of Man" series as a whole has set itself to do: namely, provide original guides for readers seeking concise and easily understood introductions to topics in anthropology.

*The Winds of Tomorrow: Social Change in a Maya Town.* By Richard A. Thompson. Chicago: University of Chicago Press, 1974. Pp. x+182. \$12.50.

Kathleen Fordham Norr

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This slender volume combines two distinct endeavors. The first is an ethnographic report of economic and social change in a town of about 13,000 in the Yucatan. Richard Thompson then uses survey data and mathematical models to assess current patterns of mobility and to project future change. The description of the town and its changing economic and social structure will interest students of economic organization, stratification, and ethnicity; the model and predictions, although ingenious, are marred by methodological and theoretical flaws sufficient to question their presentation at all.

Until the 1940s, Ticul was a sleepy rural town of corn-growing, Maya-speaking mestizos and a small Hispanic elite of merchants, pro-

professionals, bureaucrats, and urban craftsmen called *catrines*. Although the Yucatan region as a whole suffered economic depression following World War II due to falling hemp prices, Ticul had few hemp plantations and was little affected. Wartime shortages stimulated the growth of minor local crafts, especially shoe making, so that by 1968 these craft activities employed about one-third of the labor force. Accompanying this substantial economic change was a corresponding decline in the agricultural labor force, increased upward mobility, and adoption of Hispanic customs, dress, and language by upwardly mobile mestizos. The formerly elite status of *catrin* can now be attained by any mestizo with an urban job, fluent Spanish, and a willingness to follow Spanish rather than Mayan customs. Economic prosperity has thus brought a decline in local Mayan customs and an increased integration into the national Mexican culture.

I found the description of Ticul's economic and social change highly interesting and informative. Medium-sized towns play an important role in economic development and have been much neglected. Thompson's use of sample survey procedures with traditional ethnographic observation provides an appropriate methodology for studying these middle-sized communities. The discussions of ethnicity and stratification, essentially based upon Lloyd Warner in both methodology and perspective, are refreshing to readers of stratification literature. Because of Thompson's familiarity with the townspeople, the categories used are socially relevant, with concrete implications for life styles and social ties, not mere abstract statistical categories.

Thompson's findings will inevitably be compared to Manning Nash's classic study of the impact of a textile factory in a Guatemala Mayan community 15 years ago (*Machine Age Maya* [Chicago: University of Chicago Press, 1967]). Although that factory also employed about a third of the labor force and was certainly more "modern" than the crafts of Ticul, Nash found little social change, and greater economic prosperity did not lead factory workers to imitate Hispanic customs. Two factors that facilitate Ladinization in Ticul include a larger and more visible Hispanic elite and greater education for mestizos, giving them the necessary facility in Spanish to become *catrines*.

When Thompson turns from the current social structure to the use of intergenerational mobility data to predict future changes, he encounters both methodological and theoretical difficulties. His basic data come from a random sample of 111 married men. Thompson uses their statuses and their reports of their fathers' statuses in a Markov chain model to project that status distribution of the next "generation." Thompson then attempts to link his projection with real time: "it projects . . . no more than two or three decades into the future, the span of a single generation" (p. 169). Unfortunately, this assumption fails to distinguish between the concepts of generation and cohort. His random sample represents the current status distribution; by necessity, the statuses of the sample's fathers (or hypothetical sons) cannot represent the status structure of



the community at any real point in time. The men sampled presumably range at least from 20 to 60 years of age. Thus, their fathers were born between 40 and 100 years ago, and a substantial portion of the fathers are still part of the current status system. Many men have more than one son, while some have none. If there is differential fertility by status, those strata with higher fertility will be overrepresented in the father's generation and underrepresented in the hypothetical third generation. All the problems of interpretation of generational data as a cross-section are increased by projection into the future (cf. O. D. Duncan, "Methodological Issues in the Analysis of Social Mobility," pp. 51-97 in *Social Structure and Mobility in Economic Development*, edited by N. Smelser and S. M. Lipset [Chicago: Aldine Publications, 1964]). For these reasons, it seems that a conventional projection based on cohort analysis would be more meaningful, given the aim of predicting the actual cross-sectional status distribution in the near future.

More important, the basic assumption of Thompson's projection—that the rate of change of the last 30 years will remain constant over the next 30—seems unwarranted. The basic cause of the status transformation in Ticul is the expansion of craft industries. Changes in the occupational structure, especially in smaller units, are normally discontinuous, with periods of rapid expansion followed by consolidation or even depression. The future of the craft industries of Ticul seems especially problematic and depends primarily upon the changing economy of the Yucatan and Mexico as a whole, not upon forces within the town itself. The town could continue expansion into more industrialized and more diverse activities, with status improvements or at exceeding current rates. Equally possible, a modern shoe factory located elsewhere could virtually wipe out the local crafts, with the more skilled Ticulenos migrating to larger factories and the less skilled returning to corn farming and the old Mayan ways.

*Portrait of a Greek Mountain Village.* By Juliet du Boulay. Oxford: Clarendon Press, 1974. Pp. viii+296. \$19.25.

Scott G. McNall

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It was with some eagerness that I began reading this book, but I finished it in considerable disappointment. In her preface, du Boulay indicates that we will receive a glimpse of the world of the women in Greek villages. Had this implied promise been fulfilled the book would have filled a void in the anthropological literature on Greece. Instead it presents an incomplete view of both the world of the female and the world of the male. Du Boulay, I think, wants to demonstrate the validity of her mentors' theories, but she represents a school of anthropology that cannot produce fruitful descriptions of the human condition.

The book is about a dying village in Euboea with the pseudonym of

Ambéli. As the author notes (p. 5), Ambéli is an anomaly because, having no road, it is isolated from other villages. This means, among other things, that farm machinery cannot be used to till the fields or harvest the crops; many traditional forms of agriculture are still maintained. This, however, is the type of village that the author was seeking because she wanted to study "the more traditional aspects of village society" (p. 4). Her overall goal was to identify those values that unified the community, while delineating the strains that those values imposed on village life. So, at the most, what we have is the framework for more salvage anthropology—that is, the recording, and usually lamenting, of values and ways of life that are passing.

The author adequately describes the pattern of values in Greece that contributes to secrecy, lying, patronage, a lack of cooperation, and general suspicion of everyone outside of the immediate family. The unequal roles of males and females are clearly portrayed, and the inherent conflict between the sexes is traced. One wishes, though, for more detailed description of actual relations between males and females, at least from the female's point of view. With the images that both have of one another, how *do* they manage?

There is an overabundance of information which demonstrates that villagers do not get along with one another. In fact, the bulk of the book seems to be made up of information on gossiping and hostility. The author suggests that gossiping and certain forms of hostility may be functional. The argument is essentially the same as that the labeling theorists make when they note that deviance helps define the boundaries of a society. I would not argue with this position, but I would suggest that deviance is not sufficient to keep a society together or to provide the moral integration that allows people to deal with their existential despair. And here there is good reason for despair, as the author herself notes. The village is dying, and children migrate and/or marry out of the village as soon as the opportunity presents itself. Urban values have come to the village, even though urbanism itself has not, and the villagers feel the inadequacy of their life style.

We do not have an adequate description of the village's tie to the larger society. This is crucial because it may be that much of what keeps the village together is imposed from the outside. Support for the price of resin, loans for fertilizer, and so forth, should have been examined as one type of reason for "integration." (Perhaps du Boulay's decision to view the village from the perspective of the women led to a discounting of the importance of the external political system.)

Another characteristic of this work, typical of the work done in this fashion, is the ignoring of the social and historical forces that shaped the values of the people in the village. There is a discussion of the impact of the wars of the 1940s on the village, but not in terms of how this might have contributed to fatalistic attitudes. There is apparently no recognition of the fact that the people of Ambéli may behave the way they do because they are poor, at the bottom of the class ladder, and feel trapped there.

Du Boulay views as irrational the villager's refusal to maximize his economic gain by tapping the pine trees for resin instead of engaging in agriculture (p. 35). But it is not irrational, if one understands that farmers have previously tried single crops, or nonagricultural occupations, and lost their source of bread. There is nothing necessarily mystical, as the author suggests there is, in wanting to grow your own food so that no matter what happens you can at least eat.

One of the reasons why the author probably doesn't consider alternative explanations is that she doesn't make use of the anthropological and sociological literature on peasant societies. Perhaps she wishes to reject Foster's ideas ("Peasant Society and the Image of the Limited Good," *American Anthropologist* 67 [April 1965]: 293-315), or those of Banfield (*The Moral Basis of a Backward Society* [Glencoe, Ill.: Free Press, 1958]), but she should at least have referred to them, as they seem to fit her data so well. Her bibliography contains only 14 items, and these provide the framework of the book. Included are Andrews, Campbell, and Peristiany. Now the works of these three authors are certainly classics for anyone interested in studying Greece and Mediterranean culture, but to examine Ambéli solely through the viewpoints of these works means that one gets the same explanations and the same behavior recorded. This relates to the book's lack of a unifying theoretical framework. We do not need to go on ad infinitum recording information about dying villages. Too much detail produces both sterility and triviality. There needs to be a blend of ethnographic description and theory, to produce works such as Wylie's *Village in the Vacluse* (New York: Harper & Row, 1964).

But even as a traditional ethnography this book leaves a great deal to be desired. We have no real discussion of early socialization or child rearing. This is strange, to say the least, in a book about women. We have no discussion of how the village links up with the larger society, other than through outmigration. After reading this book a person would be hard pressed to describe what the women of Ambéli thought about the world.

On a more specific level, du Boulay's discussion of the division of property is somewhat confused and could have been clarified by the simple expedient of referring to Greek law. There is a puzzling use of Greek words and phrases throughout the book. One can understand the need for providing the Greek term or phrase when the idiomatic meaning is different from the literal translation, but when the translation is an exact rendering, why bother? Nor is there any apparent logic in the decision to give the Greek for some words and phrases, and not for others.

I think this book does provide some useful information. It adequately covers the type of value system found in many of the rural areas of Greece. The section describing the symbolic and social significance of the house is excellent. This work is certainly as good as most of the ethnographies that have been written about Mediterranean societies. As a final note, I think it would be good experience for those who are going to do ethnographies on semi-industrialized societies to live on a farm in their

own country before they go off to earn their stripes. Then, much of what happens would not be such a shock to them, and economic and rationalistic explanations might be given for behavior. A vision of the peasant family as a reflection of the holy family is not an explanation.

*Women of the Forest.* By Yolanda Murphy and Robert F. Murphy. New York: Columbia University Press, 1974. Pp. xii+236. \$10.00 (cloth); \$3.45 (paper).

Judith Shapiro

*Bryn Mawr College*

*Women of the Forest* is a study of sex roles and sex identity in a tropical forest society of central Brazil, based on field research carried out in the early 1950s among the Mundurucú Indians of the upper Tapajós River region. The authors first analyze the patterns that still prevailed in the few surviving traditional Mundurucú villages and then go on to consider the effects of the colonial situation.

The central theme of the Murphys' book is the disparity between the Mundurucú ideology of male dominance and the actual social positions of women and men. According to the Murphys, what is most essential about the relationship between the sexes is their independence from one another. In traditional Mundurucú villages, men and women work separately, eat separately, and even sleep separately. Groups of related women and their children reside in extended family dwellings; adolescent and adult men live together in a men's house. The major subsistence activities are carried out by groups composed of members of the same sex. The men of the village hunt together and women cooperate in the processing of manioc flour, the most important and time consuming of female tasks. Each village has a shed where much of the work of manioc processing is done. This serves as a social gathering place for women, a counterpart to the men's house.

This pattern of sexual separation in Mundurucú society is such that men exercise little influence over the activities of women in the course of daily life. Marriage for the Mundurucú woman means neither isolation within a nuclear family nor a particularly close bond with an individual man; her life continues to revolve primarily around the other women with whom she lives and works. The sexes relate to one another less in terms of dyadic bonds than as collectivities.

In the light of this form of social organization, the emphasis on male control over women in Mundurucú ideology is seen by the Murphys as a kind of just-so story with which men beguile themselves into feeling superior. The Murphys claim that women do not share men's ideas about the relative position of the sexes and are generally unimpressed with the paraphernalia of male ritual activity. This approach, while perhaps oversimplifying the situation, is nonetheless a refreshing departure from the common anthropological tendency to refer collective representations to one kind of collectivity: the society as a whole.

The image of the Mundurucú man that emerges from this book is one that Sigmund Freud and Erving Goffman might have worked out together: a figure conflicted and insecure about his own sexual identity, struggling to manage impressions for an audience that can easily see backstage. Women, on the other hand, are represented as truly strong. They are, as the Murphys put it, "firmly grounded in social reality—they are masters of the practical" (p. 112). Their relationships with each other, though narrower in scope than those of the men, appear to be more cohesive and enduring. Above all, like women in any society, they play the major role in the only human activity that the Murphys see as having intrinsic meaning—the creation of new life.

In their attempt to correct a stereotype of male dominance and female subordination, the Murphys go rather too far in the other direction. The result is, at times, too close for comfort to our own culture's ideologies of abject males and all-powerful females. I sympathize with the authors' interest in seeing whether a comparative consideration of two very different societies—our own and the Mundurucú—can shed light on universal dimensions of male and female experience. I also appreciate their willingness to go out on a limb and say the kinds of things that anthropologists will express informally but are unwilling to commit themselves to in print. But some of their generalizations—those concerning women's reproductive role, for example—are open to question. Evaluating the relative "meaningfulness" of various human activities is not an easy task. Meaningful to whom? and within what kind of perspective? We are told that Mundurucú women themselves "are resentful of the continued cycle of pregnancy and birth, regarding it as an encumbrance and physical handicap" (p. 161). It is important to keep in mind that the type of status a woman gains by virtue of being a mother and the personal experience that motherhood represents vary considerably from one society to another.

I would agree with the Murphys that there are certain universal problems in the establishment of male identity that can be related to the fact that women both bear children and generally play the predominant role in their early socialization. I am, however, uncomfortable on the path that leads from this point to the notion that culture is "a sort of collective fetishism" (p. 232) required primarily by men, since it is they who are justified by symbols alone. I do not think that differences in world view between men and women should be put in terms of an opposition between culture and practical reason.

The Murphys present an interesting and sensitive account of how Mundurucú involvement in rubber tapping has affected the relative positions of the sexes. They show that women, far from being passive and conservative, as they are often described in studies of culture change, have played a crucial and very active role in the coming of the new order. This new order, based on a highly individualized pattern of labor and of patron-client relationships, involves a closer bond between husband and wife and the emergence of the nuclear family as the central residential

and economic unit. Traditional patterns of intrasexual cooperation have dissolved and the men's house has disappeared.

Is it possible to determine in which system the Mundurucú woman is better off? The problem here, as the Murphys note, is that "we really do not possess the criteria for evaluating the relative status of the sexes" (p. 201). One solution is to let the people spoken about speak for themselves. "Instead of asking ourselves who has it best, we should be asking Mundurucú women" (p. 201). The answer here is that Mundurucú women prefer the new way—they like having access to trade goods; they like having their husbands around to help them in domestic tasks.

The Murphys do not, however, find the actors' own perspectives a satisfactory stopping place—and in this I fully agree with them. They are concerned with aspects of the acculturated Mundurucú woman's position of which she may as yet be unaware—for example, the consequences of her increasing dependence on her husband.

Though the Murphys note the difficulties of speaking about the relative status of men and women objectively, pointing out quite rightly that subordination and dominance are far more intricate issues in regard to sex than in regard to class, they do make use of certain objective variables as indicators of status. Thus, given a situation in which the roles of the sexes are strongly differentiated, women are better off when they exercise control over an important sector of production (in the Mundurucú case, manioc processing) and participate in solidary relationships with members of their own sex. An additional factor that should be considered in this context is control over extradomestic exchange, which one anthropologist has recently judged to be the most important single indicator of status in hunting-gathering and horticultural societies (Ernestine Friedl, *Women and Men: An Anthropologist's View* [New York: Holt, Rinehart & Winston, 1975]).

There are several other interesting issues touched upon by the Murphys—for example, how hunting and warfare relate to male dominance—which cannot be discussed here. Suffice it to say, in conclusion, that *Women of the Forest* is a valuable and thought-provoking contribution to the comparative study of sex roles. It is also written by social scientists who do not leave behind their sense of style, humor, and irony when they go about their professional business.

*Uses of the Sociology of Education: The Seventy-third Yearbook of the National Society for the Study of Education, Part II.* Edited by C. Wayne Gordon. Chicago: National Society for the Study of Education, 1974. Pp. xviii+518. \$10.00.

Leo Rigsby

Temple University

We are told in the preface that the board of directors of the National Society for the Study of Education made the decision to prepare a volume

on the uses of sociology in education. In order to accomplish the task of preparing a yearbook, the board of directors decides on topics and appoints committees "calculated in their personnel to insure consideration of all significant points of view" (p. iv). The committee that was appointed to produce the present volume decided on the approach of "presenting sociological studies of a number of current problems confronting those engaged in the study and practice of sociology of education" (p. ix). The problem areas chosen by the committee to be emphasized in this volume are the following: (1) Youth culture: alienation, protest, and dissent; (2) equality of educational attainment; (3) race/ethnic desegregation, integration, and decentralization; (4) the role of the federal government in education. These areas "had high priority in the committee's preferences; they were also those for which contributors could be secured" (p. ix). The result of asking a number of researchers to write articles on given topics for a collection of this kind is that one then incurs an obligation to accept what is written. In this case that has produced four sets of articles that are not well integrated within sections and that are overlapping in some cases. Further, in no section except, perhaps, that on youth culture, does one find anything like exhaustive or even representative coverage of issues and perspectives.

The section on youth culture has seven articles. The final article of the section, by William Spady and Chaim Adler, aims at integrating the other six papers, though its success is limited by the lack of exhaustive coverage of relevant materials in those articles. Worthy of note among the articles in this section is one by William G. Spady which combines Weberian notions on the bases of legitimacy of authority, an analysis of ways that teachers legitimate their authority over students, ideas about performance norms for students, and strategies for evaluating students to provide a framework for understanding the forms and extent of student alienation. The other articles (by Joseph Katz, Ralph W. Larkin, Thomas J. La Belle, Carl Weinberg, and Chaim Adler) are competent but relatively narrow. Sociologists seeking an overall understanding of youth culture and youth protest have available materials that are more exhaustive and integrated than the articles in this section.

The second section consists of two articles, one by W. B. Brookover, R. J. Gigliatti, R. D. Henderson, B. E. Niles, and J. Schneider, and the other by William Speizman. The first challenges the political motives of the standardized testing tradition in American education. Its thesis is that the use of standardized tests for assessing the extent to which schools are doing their jobs is reasonable but that the use of tests to differentiate among and to sort children is inappropriate. The other is an ethnomethodological critique of evaluation research. The dispassionate will find it provocative without agreeing with all that it says. A quote from the conclusion will illustrate the style: "Evaluation is activism. It is purposively directed at the manipulation of the institutions of society. But most of those engaged in evaluation are not prepared for the activist role.

They have little or no experience with politics and are unable to comprehend what political involvement entails" (p. 209).

The four articles in the third section of the book are equally disparate. Three are chronologies of the (lack of) progress in desegregation. The first is authored by Robert L. Green; the second, by J. R. Mercer, M. Coleman, and J. Harloe; and the third, by C. U. Smith and C. M. Grigg. In one of these, "metropolitanization" of urban school districts is explicitly advocated to provide a sufficient base for school integration. The final article in the section, by Jay D. Scribner and David O'Shea, deals with the decentralization movement in New York and Los Angeles. It argues strongly for community control. One wishes that the authors in this section had engaged in dialogue over bigger-and-more-integrated versus smaller-and-more-homogeneous school districts.

The last section of the book consists of three articles (by John C. Hogan, Michael W. Kirst, and Samuel D. Sieber) dealing with the impact of federal legal activities, federal aid, and federal research and development activities on schools, respectively. These articles are, for the most part, chronologies of events. In this vein they are informative for those needing up-to-date (as of about 1972) information in these areas. Most sociologists will not have an overriding professional need for such information.

In summary, there is useful information in this book. Because the articles are studies rather than syntheses, readers seeking state-of-the-art pieces on these general topics will have to look elsewhere.

*Varieties of Work Experience: The Social Control of Occupational Groups and Roles.* Edited by Phyllis L. Stewart and Muriel G. Cantor. New York: Halsted Press, 1974. Pp. ix+407. \$12.00 (cloth); \$5.95 (paper).

Raymond L. Gold

*University of Montana, Missoula*

"The basic effort [of this reader] is to bring together a variety of descriptive contemporary studies representing a broad spectrum of occupations and to determine what factors or processes of social control are common to these occupations" (p. 1). To focus on social control and its relation to occupational autonomy, the editors bring together 19 essays on occupations which are meant to reveal "the impact of the social context on the occupation in regulating, modifying, or controlling role relationships" (p. 1). In this effort they achieve only moderate success at best because the essays they use are too short to tell much about social context. Moreover, the investigators who did these essays concern themselves with few features of the larger work organizations in which the occupations are practiced and with even fewer features of the communities in which these organizations are situated. To say that "the occupations in this book are related to one analytical framework: the impact of the social context on occupational roles" (p. 3) is therefore to utter a wish which the editors



have no way of fulfilling after having selected or commissioned report extracts which deal almost solely with the microcontext. The reader is then left to wonder about the relation of these occupations to other work organizations and roles in their communities.

Although the editors claim to bring together "a broad spectrum of occupations," the 19 which they actually present are principally middle- and upper-status occupations. They note that they contacted about 100 occupational sociologists but found that only a few were prepared to write about recent research on low-status occupations. I find this most curious because (1) I know that quite a few low-status occupations have recently been studied, and (2) apparently the editors and the people they know have been ignoring Hughes's profound discovery, reported in *Men and Their Work* about 17 years ago, that comparative studies of work are greatly facilitated by beginning, whenever feasible, with low-status settings and roles which contain empirically accessible instances of whatever the investigator wishes to study. Hughes correctly pointed out that people in lower-status occupations are much more likely to tell occupational sociologists about aspects of their work (e.g., about processes and procedures of social control) which members of higher-status occupations are apt to gloss over, conceal, and otherwise hold back information on because of their feeling that such matters should not be shared with any but their inner circle of bona fide professional colleagues and fellow workers. A regrettable flaw in this book is the editors' failure to include enough low-status occupations to present a balanced picture of the work scene and to strengthen their otherwise limited ability to make comparisons.

The editors indicate that "this book is derived from an awareness of the paucity of systematic comparative studies of craftsmen, artists, professionals, and other occupational roles" (p. 1). It is true that, as the editors suggest, there is a paucity of such comparative studies, but it is equally true that they do very little to correct this lamentable state of affairs. To their credit, the editors make systematic efforts throughout the book to introduce, compare, and explain, but their efforts to compare (and therefore to explain) seem strained or forced. In addition to their aforementioned failure to adequately cover low-status work, two other shortcomings need to be noted in order to account for their difficulties in handling comparisons. First, the editors, along with a great many other sociologists who attempt to do comparisons, simply do not utilize formal sociological analysis, which is almost indispensable for clearly and certainly distinguishing between social interactional matters which are context-bound and those which are context-free. Second, even if they had tried to enlist Georg Simmel's assistance, they would have been greatly handicapped by the book's lack of substantive coverage of the 19 occupations. The book presents too little about any of them to facilitate doing the systematic comparisons the editors set out to do.

Overall, I found this book to be much less enjoyable to read than Terkel's *Working*, its thin essays much less amenable to comparisons than the five relatively substantial ones in Berger's *Human Shape of Work*, and

the whole much less useful in teaching than Hughes's out-of-print *Men and Their Work*. While some of the essays in *Varieties* are extraordinarily well done (especially Robert Faulkner's ethnographically sound, masterfully presented report on interaction in symphony orchestras), as a collection of readings they just do not fit together very well. Still needed for courses in the sociology of work is a book which combines the depth of substantive coverage offered by Berger, the conceptual insight and breadth given by Hughes, the sophisticated writing style displayed by Faulkner, and the sheer reading enjoyment provided by Terkel.

*Man's Work and Leisure*. By Nels Anderson. Leiden: E. J. Brill, 1974. Pp. x+146. \$23.22.

Jiri Zuzanek

*University of Western Ontario*

Nels Anderson's interest in sociology of leisure dates back to 1954 when he worked as director of the UNESCO Institute for Social Sciences in Cologne. At that time no more than 15 or 16 participants from a few European countries and the United States took part in his annual seminar on leisure. It was, however, out of this nucleus and the subsequent activities of the Chicago center for the study of leisure and the Committee on Leisure of the ISA that modern sociological study of leisure has evolved.

Anderson's pioneering role in leisure studies and his unique life experience provide the book under review with a special flavor. The following quotation from the publication may illustrate this point. "In 1908," Anderson states, "this writer worked in a smelter in Northern Michigan which produced pig iron from iron ore. . . . The pay was 18 cents an hour for shelving the heavy iron ore and 15 cents an hour for forking charcoal. Two shifts operated the furnace, each working 12 hours. Work was continuous, seven days a week. The shift should change, night to day, day to night, each for two weeks. On changeover days each shift worked 18 hours. This 84-hour week meant all 84 hours on the job" (p. 54). One finds, of course, similar data in almost any standard book on leisure discussing changes in the length of work during the last 60-70 years, but one is taken by surprise when the author is able to back this by such a colorful example from his own life.

There are numerous other connections between Anderson's life and his theoretical concerns as reflected in the book. Anderson was born to a family of Swedish immigrant farmers and, like Thorstein Veblen, who was born to a family of Norwegian immigrant farmers (the two countries were one at that time!), exhibits a positive attachment to the values of work and a somewhat scornful attitude toward those middle-class intellectuals who do not possess the knowledge and understanding of the "realities of the work which most people perform" (p. ix). Anderson openly admits that it was this remoteness of the discussions on leisure from the

problems of work which stimulated him into writing his first book on leisure, *Work and Leisure*, published in 1961 by Routledge and Kegan Paul. The relationship between work and leisure remains Anderson's major preoccupation in his latest book as well. Like many other authors, Anderson agrees that by the end of the 20th century less time will be spent at work and somewhat more in leisure, but socially and psychologically work will remain, he believes, of utmost importance. "There will be little shortage of jobs when it is recognized that there is endless public work waiting to be done to keep the earth, the waters and the air clean, to dispose of waste, rebuild the forests, and eliminate much of the ugliness in our cities" (p. x). In Anderson's view, work and leisure cannot be separated and both can be understood only in the larger context of social and technological change. "The future of work will obviously depend on a varied and changing set of change-inducing factors; population, natural resources, technology, movements of ideas, as well as other less obvious factors. The future of leisure depends on the future of work" (p. 139).

The book is divided into nine chapters which discuss the definition of work and leisure; the impact of technology upon work; class implications of industrialism; the coming of new leisure; time distributions between work and leisure; the role of arts in modern societies; the role of education; the relationship among work, leisure, and language; the relationship among work, leisure, and welfare; the challenge of leisure; and social forecasting of future trends in work and leisure.

My critical comment about the book is that the author's scope is too broad. This forces him almost inevitably to become in some chapters (especially in the first part of the book) a reviewer and commentator on the literature rather than a social analyst or critic.

I would like to add a personal comment as well. Although the author makes some use of his personal experience in analyzing work and leisure, it is my feeling that the book could have benefited from an even greater "autobiographic" emphasis. A critical examination of the changing philosophies of work and leisure since 1908 based on Anderson's direct exposure to different periods and social milieus, would appear to me an extremely intriguing endeavor. Nobody else could have done it better for us than the author of *The Hobo*. But then, maybe I am asking for another book—a social autobiography rather than a sociological monograph.

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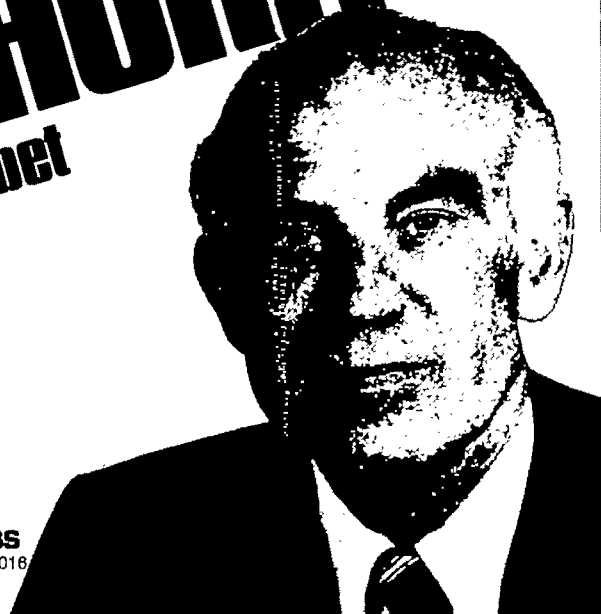
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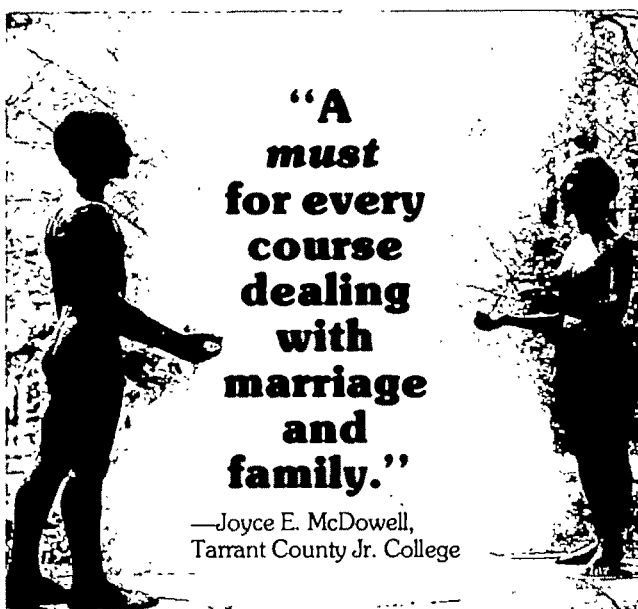
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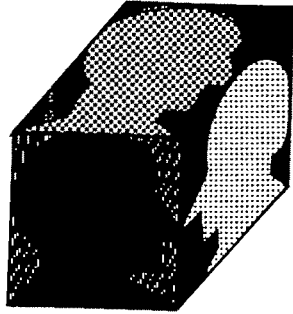
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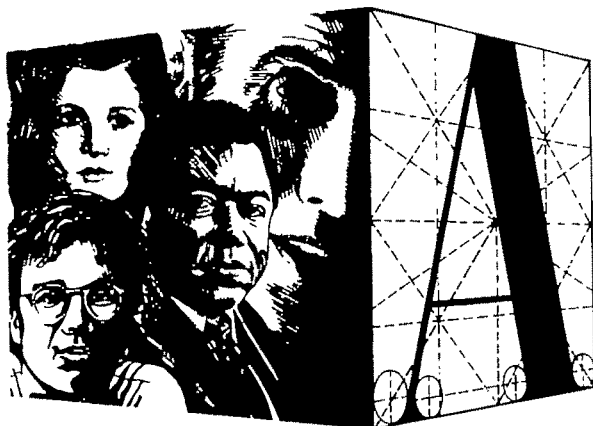
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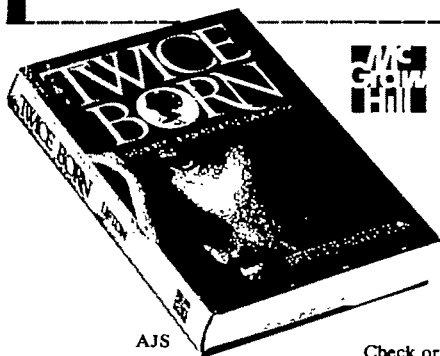
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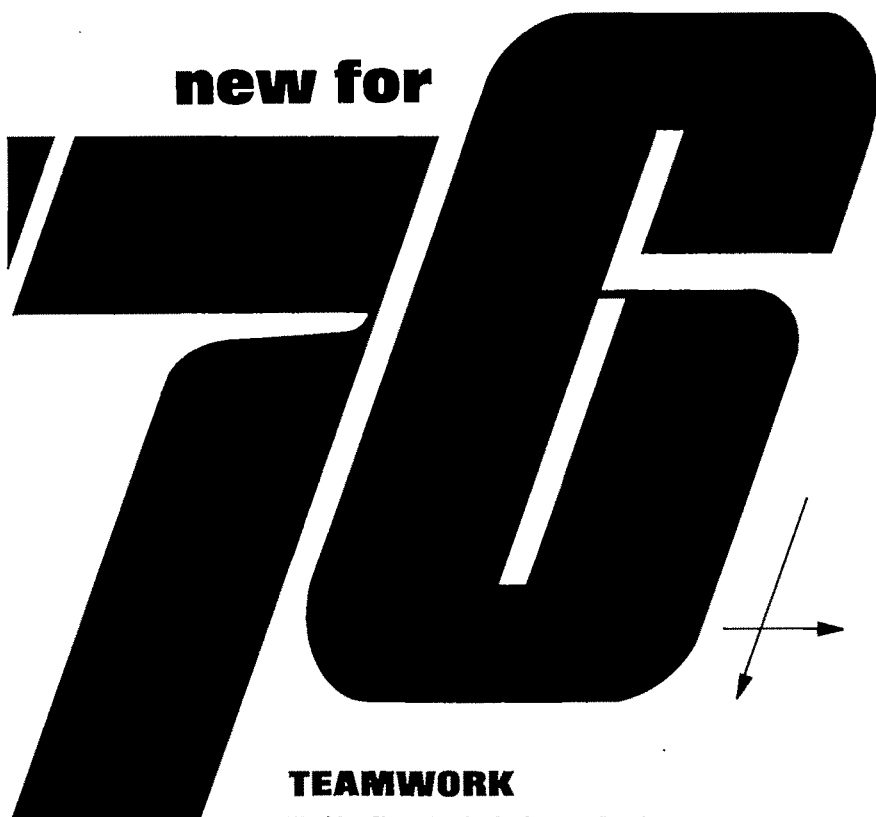
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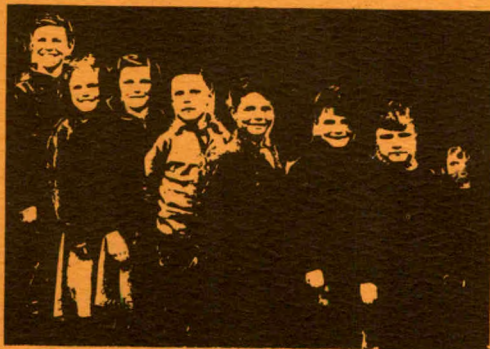
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## Apology

The biographical note for Nan Lin appearing in this *Journal* ("In This Issue," November 1975) mistakenly used feminine pronouns in referring to Professor Lin. We extend our apologies to him and promise to be more careful in the future.

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Weber, Max. (1921) 1968. "Society's Problems." Pp. 12–16 in *Economy and Society*, edited by Guenther Ross and Claus Wittich. New York: Bedminster.

# The Real Self: From Institution to Impulse<sup>1</sup>

Ralph H. Turner

*University of California, Los Angeles*

It is proposed that people variously recognize their real selves either in feelings and actions of an institutional and volitional nature, such as ambition, morality, and altruism, or in the experience of impulse, such as undisciplined desire and the wish to make intimate revelations to other people. A shift toward the impulse pole seems to be under way and might be plausibly explained by changing cultural definitions of reality, modified terms of social integration, shifting patterns of deprivation, or new opportunities and consequences. Many standard sociological assumptions about social control are incompatible with the new pattern of self-identification.

Except at the most macroscopic and demographic levels, there is no way to study dynamics and change in social systems without attending to the attitudes and conceptions held by their members. Subjective data are essential, because people are not just miniature reproductions of their societies (Bendix 1952; Wrong 1961; Etzioni 1968).

One important sociological tradition for bringing personal dynamics into the analysis of social structure began with Thomas and Znaniecki's (1918) concept of *life organization*, which Park (1931) translated into *conceptions of self* and Kuhn and McPartland (1954) converted into an easily applied set of empirical operations. The *self-conception* as an object arises in connection with self-process (Mead 1934). From early experience with the distinction between *mine* and *yours*, I learn to distinguish between *myself* and others (Cooley 1902).

The present discussion emphasizes a related point, that the idea of a self-as-object permits me to distinguish among the various feelings and actions that emanate from my person. Some emanations I recognize as expressions of my real self; others seem foreign to the real me. I take little credit and assume little blame for the sensations and actions that are peripheral to my real self (Turner 1968). Others are of great significance, because they embody my true self, good or bad. The articulation of *real selves* with social structure should be a major link in the functioning and change of societies. This approach to linking person and social structure

<sup>1</sup> Expanded version of a paper delivered at the meeting of the American Sociological Association in Denver, August 1971. The author gratefully acknowledges support from the National Institute of Mental Health, grant USPHS MH 16505, and comments on an earlier draft from Erving Goffman, David Heise, Rosabeth Kanter, Leo Kuper, Jerald Schutte, Melvin Seeman, Norma Shosid, Stephan Spitzer, Samuel Surace, and Cornelis Van Zeyl.



is especially compatible with symbolic interactionist and phenomenological perspectives which stress the ongoing creation of reality by each member of society.

The aim of this paper is to elaborate a dimension of self-conception that may have important implications for sociological theories of social control and other aspects of societal functioning. To varying degrees, people accept as evidence of their real selves either feelings and actions with an *institutional* focus or ones they identify as strictly *impulse*. There are suggestive signs that recent decades have witnessed a shift in the locus of self away from the institutional pole and toward that of impulse. This shift may have altered substantially the world of experience in which people orient themselves, setting it apart from the one that much established sociological theory describes. I describe these types, examine the hypothesized shift, suggest some theories that might explain it, and explore the implications for sociological thought.

In another publication I shall describe a modification of the "Who am I?" technique that I have developed specifically to gather data for analysis in these terms.

### Self as Object

Before presenting the thesis, I must briefly outline my assumptions concerning self-conception. Out of the distinctively human reflexive process emerges a sense or conception of self as an object, which, however, has no existence apart from the conceptions and attitudes by which one constitutes it (Blumer 1966, pp. 539-40). Identifying the self is not the same as identifying one's values, and self-conception is not to be confused with ideal self or ego ideal (Kuhn and McPartland 1954, p. 69). The self-conception identifies a person in qualitative and locational terms, not merely in evaluative ones such as self-esteem. The self is an object in relation to other objects, all of which are constantly modified in dynamic interrelationship (Berger 1966). Self-conception refers to the continuity—however imperfect—of an individual's experience of himself in a variety of situations. It is most usefully viewed as an intervening variable between some aspect of social structure and the working of the same or another aspect.

How the self can be so constituted as not to be coterminous with all the feelings and actions that emanate from a person has been a constant source of puzzlement. I suggest tentatively that the demarcation has three components. First, it relies on a more generalized discrimination between the real and the unreal in experience. We identify some experiences as fantasies, hallucinations, dreams, or other forms of the unreal. Under crisis conditions we sometimes go so far as to deceive ourselves concerning the reality of be-

havior and feelings that are not in accord with self (Spiegel 1969). More often we merely borrow the language of reality and unreality and employ its connotations. Second, bounding the self incorporates the general distinction between *attributions* to person and those to situation (Jones et al. 1971). Behaviors thought to reveal the true self are also ones whose causes are perceived as residing in the person rather than the situation. This distinction relies on a commonsense psychology held by the persons making attributions. The bases for bounding the self necessarily change whenever folk understandings of psychology change. The distinction also inextricably mixes normative conceptions of responsibility with naturalistic ones of cause. The emergence of self cannot be separated from the essentially moral process of establishing human accountability (Kilpatrick 1941). Third, the idea of self incorporates a further sense of a realm that is distinctly personal or *propriative* (Allport 1955). This realm has not been effectively defined; yet awareness of it is a crucial and almost irreducible intuition. Attribution theorists' evidence that self-attribution does not follow entirely the same rules as person attribution gives some clues to the proprium. But beyond the attribution of causation for behavior, the realm of self is characterized by possessiveness, privacy, and sacredness.

## INSTITUTION AND IMPULSE AS LOCI OF SELF

The self-conception is most frequently described sociologically by naming the roles that are preeminent in it. In a good example of this approach, Wellman (1971) finds that the self-conceptions of both black and white adolescents can be characterized on the basis of the same set of identities—namely, their age, gender, family, religion, race, and ethnic heritage, and their roles as students, athletes, and friends. Studies comparing the place of occupation and work in the life organizations of various groups of workers (Dubin 1956; Wilensky 1964) likewise relate the self-conception to particular roles in society.

Self-conceptions can also be compared on the basis of distinctions at a more abstract level. The relationship between self and social order is put in more comprehensive terms when we distinguish between self as anchored in *institutions* and self as anchored in *impulse*.

To one person, an angry outburst or the excitement of extramarital desire comes as an alien impetus that superficially beclouds or even dangerously threatens the true self. The experience is real enough and may even be persistent and gratifying, but it is still not felt as signifying the real self. The true self is recognized in acts of volition, in the pursuit of institutionalized goals, and not in the satisfaction of impulses outside institutional frameworks. To another person, the outburst or desire is recognized—fearfully or enthusiastically—as an indication that the real self is breaking

through a deceptive crust of institutional behavior. Institutional motivations are external, artificial constraints and superimpositions that bridle manifestations of the real self. One plays the institutional game when he must, but only at the expense of the true self. The true self consists of deep, unsocialized, inner impulses. Mad desire and errant fancy are exquisite expressions of the self.

Again, conscientious acceptance of group obligations and unswerving loyalty can mean that the real self has assumed firm control and overcome the alien forces. But for those who find out who they really are by listening to the voice of impulse, the same behavior is a meaningless submission to institutional regimens and authoritarianism. A mother's self-sacrifice for her child is the measure of her real self when seen through institutional eyes, and it is a senseless betrayal of the parent's true being to those who find personal reality in the world of impulse.

It is no accident that this polarity parallels Freud's classic distinction between id and superego. To Freud, the id was more truly the person and the superego merely an external imposition. As he turned to examinations of society, he expressed the same conviction when he wrote, "Our civilization is entirely based upon the suppression of instincts" (1931, p. 13), and when he proposed a relaxation of social norms and standards as a solution to the discontents of modern civilization (1930). This position sharply contrasts with a view shared by many writers and exemplified in Park's assertion that "the role we are striving to live up to—this mask is our truer self" (1927, p. 739). Although in other writings Park sometimes expressed a different conviction, his statement epitomized the institutional locus of self, while Freud located the self chiefly in the world of impulse—until his belated concessions to ego.

### The Key Differences

Several crucial differences between the two contrasting loci of self can be briefly stated.

1. Under the institution locus, the real self is revealed when an individual adheres to a high standard, especially in the face of serious temptation to fall away. A person shows his true mettle under fire. Under the impulse locus, the real self is revealed when a person does something solely because he wants to—not because it is good or bad or noble or courageous or self-sacrificing, but because he spontaneously wishes to do so.

2. To *impulsives*, the true self is something to be discovered. A young person drops out of school or out of the labor force in order to reflect upon and discover who he really is. To the *institutional*, waiting around for self-discovery to occur is ridiculous. The self is something attained, created, achieved, not something discovered. If vocational counseling to help the

individual find his peculiar niche has elements of the impulse conception of self, the idea that a person can make of himself what he will, that one chooses a task and then works at it, is the view of institutionals. The contrast is well stated in a contemporary prescription for effective living, written from the institutional perspective:

So if we reach a point of insight at which we become disgustedly aware of how we stage ourselves, play games, and ingratiate others, to say nothing of using defense mechanisms and strategies, and if at this point we want to enrich life by finding honest, deeply felt, loving interactions with others, it is tempting to believe that we can change simply by opening a door and letting out our "true" unsullied impulses. Change is never so simple. What is really involved is not the releasing of a true self but the making of a new self, one that gradually transcends the limitations and pettiness of the old. [White 1972, p. 387]

3. Under the institution locus, the real self is revealed only when the individual is in full control of his faculties and behaviors. Allport (1955) locates the self in planning and volition, in contrast to impulse. "When the individual is dominated by segmental drives, by compulsions, or by the winds of circumstances, he has lost the integrity that comes only from maintaining major directions of striving" (pp. 50-51). When control is impaired by fatigue, stress, alcohol, or drugs, an alien self displaces the true self. The danger of any of these conditions is that after repeated experiences the individual may lose the capacity to distinguish between the true self and the counterfeit and become progressively less able to resume control and reinstate the true self. If use of alcohol is viewed with favor, it is only on condition that the user is able to practice moderation or "hold his liquor," maintaining control in spite of alcohol.

But under the impulse locus, the true self is revealed only when inhibitions are lowered or abandoned. In a magnificent statement of an institutional perspective, Wordsworth (1807) called upon Duty, "stern daughter of the voice of God," for relief from the "weight of chance-desires" and for "a repose that ever is the same." But let the barest suspicion arise that a good deed has been motivated by a sense of duty, and it loses all value as a clue to self in the eyes of the impulsive. For some impulsives drugs and alcohol are aids—often indispensable—to the discovery of self, for without them socially instilled inhibitions irresistibly overpower the true self. A participant in a Los Angeles "love-in" in 1971 said: "It's a place where people can get out, get smashed, get stoned, or whatever. A love-in is a place to get away from the apartment. It's like being out and touching people for a change, rather than working with paper and working with inanimate objects. It's like being out in the real world for a change."

4. Hypocrisy is a concern of both types, but the word means different things to each. For the institutionals, hypocrisy consists of failing to live

up to one's standards. The remedy is not to lower standards but to make amends and adhere to the standards the next time. If one's failings persist, he ceases to represent himself as what he cannot be, so that he at least escapes the charge of hypocrisy by presenting himself only as what he is. For the impulsives, hypocrisy consists of asserting standards and adhering to them even if the behavior in question is not what the individual wants to do and enjoys doing. One who sets exacting standards for himself and by dint of dedicated effort succeeds in living up to them is still a hypocrite if he must suppress a desire to escape from these strict demands. Altruism, in the traditional sense of responding to duty and setting one's own interests aside, is a penultimate hypocrisy, compounded by the probability that it is a dissimulated self-seeking and manipulation. The institutional goal is correspondence between *prescription and behavior*; the goal of impulsives is correspondence between *impulse and behavior*: hypocrisy in either instance is a lack of the appropriate correspondence.

5. In the light of the foregoing differences, the qualities that make a performance admirable differ. The polished, error-free performance, in which the audience forgets the actor and sees only the role being played, is the most admired by institutionals. Whatever the task, perfection is both the goal and the means by which the real self finds expression. But impulsives find technical perfection repelling and admire instead a performance that reveals the actor's human frailties. They are in harmony with the motion picture star system, in which Gregory Peck, John Wayne, and Gina Lollobrigida, rather than the characters they play in a given picture, are the centers of attention. Ed Sullivan's popular appeal, generally attributed to his very awkwardness and ineptitude, is incomprehensible to the institutionals. Of course, the specific cues for spontaneity have changed, so a younger generation of impulsives no longer responds to these stars as did an older generation.

6. The difference between discovery and achievement also suggests a difference in time perspective. The self as impulse means a present time perspective, while the self as institution means a future time perspective. Institutionals, who build themselves a real world by making commitments, have difficulty retaining a vital sense of self when the future perspective is no longer tenable. The *malaise* of retirement is a common indication of this pattern. In contrast, freedom from past commitments is heralded poetically in the popular song "Gentle on My Mind," by John Hartford.

7. Just as hypocrisy takes on different meanings within the two patterns, individualism is found in both settings with different implications. The individualist is one who rejects some kind of social pressure that threatens his true identity. But there are different kinds of pressure. In one view, social pressures can divert a person from achievement, from adherence to ethical standards, and from other institutional goals. The rugged individ-

ualists of 19th-century America thought in these terms. Children were imbued with an individualistic ethic in order to protect them from peer group pressures toward mediocrity or compromise of principle, either of which meant failure to realize the potential that was the true self. But individualism can also be a repudiation of the institutional and inter-individual claims that compete with impulse. The individualist may be protecting himself against a conspiracy to force him into institutional molds, to make him do his duty, or to aspire. Both types would agree that one must resist the blandishments of friends and the threats of enemies in order to be true to himself. But the institutional individualist is most attentive to pernicious pressures on the side of mediocrity and abandonment of principle; the impulsive individualist sees clearly the social pressures in league with a system of arbitrary rules and false goals.

Both institution and impulse loci allow for individualistic and non-individualistic orientations. We have found it useful to employ a cross-cutting distinction between *individual* and *social* anchorages for the self. Institutionals stress either achievement, a relatively individual goal, or altruism, a social aim, as the road to self-discovery. Somewhere between the two lies adherence to an ethical code which will vary according to whether ethics is viewed as applied altruism or a forum for individual achievement. Impulsives may stress the simple disregard of duties and inhibitions in order to gratify spontaneous impulses; this is essentially an individual route to self-discovery. Or they may seek self-discovery through expressing potentially tabooed feelings to other persons and thereby attain a state of interpersonal intimacy that transcends the normal barriers between people.

### Related and Unrelated Distinctions

It is essential not to confuse these alternative anchorages with the question of whether people are preoccupied with maintaining appearances or conforming instead of "being themselves." Describing a mass gathering of youths, a student wrote, "People tend to forget how they would hope to come across, and instead act as their true selves." This is a terse statement of how participants felt in the situation and expresses the point of view of an impulse self-anchorage. But from an institutional perspective, the same youths appear to be tumbling over one another in their anxiety to comply with the latest youthful fad and to avoid any appearance of being square. The institutional hopes that after passing through this stage the youths will "find themselves," discovering their special niches in the institutional system. The self-anchorage determines which kinds of behavior seem genuine and which are concessions to appearances.

The polarity bears resemblances to several distinctions already advanced

by others. McPartland's "Category B" responses and Kuhn and McPartland's consensual responses (Spitzer, Couch, and Stratton 1973) to the Twenty Statements Test (TST) would all be institutional responses, but so would many responses in other categories. Institutionals have much in common with Riesman, Glazer, and Denney's (1950) inner-directed persons, but other-directed persons cannot be equated with impulsives. Sorokin's (1937-41) sensate types resemble the impulsives in their assignment of ultimate reality to the world of the senses. But Sorokin identifies striving for success with the sensate mind and altruism with the ideational, whereas I see these as alternative expressions of an institutional anchorage. Benedict's (1934) interpretation of Nietzsche's Dionysians suggests impulsives, who are unrestrained in acting on impulse and dream, while the Apollonians are more comfortable in an institutionally articulated system. But the dominating Apollonian theme of moderation certainly does not apply to heroic altruism or unrestrained striving for success.

Compared with the well-known dichotomies employed by Benedict, Sorokin, and Riesman et al., the institution-impulse distinction introduces a somewhat different dimension. It allows for the discovery of reality in either excess or moderation within both self-anchorages, and it allows in each orientation for both mystical and naturalistic realities and both individualizing and unifying realities. Goals of achievement, self-control, morality, and altruism lodge the self-conception more and more firmly in some institutional structure. The impulse release attained in encounter groups, expressive movements, and dropping out may in some forms promote a bond of intimacy with other individuals but always distinguishes the true self specifically from institutional values, norms, and goals.

This polarity has much in common with the widely discussed dimension of alienation. However, three important differences of approach make the concepts complementary rather than redundant. First, alienation is an intrinsically evaluative concept, incorporating a negative view of social trends and requiring that the search for explanation be directed toward disorganization in the social structure. Granted that each investigator's own self-locus will lead him to prefer one pole to the other, there are no a priori judgments that one locus is healthier than the other and no reasons to seek explanations for the shift in the pathologies of society. Second, while alienation implies a single continuum, self-locus implies two continua that may be loosely correlated but not identical. One is the continuum I have described. The other ranges from high to low *self-resolution*, according to whether the individual has a clear and stable self-conception or a vague and uncertain identity. Some scholars think of alienation as loss of self-resolution. But alienation from one's work can signify either an impulse locus or low self-resolution. And for some scholars in the Freudian tradition, self-estrangement *means* institutional anchorage.

A third distinction is that such concepts as alienation and anomia tell only where the self is *not*. The result is an effort to infer and explain self-estrangement without first exploring the possibility that the self may be securely lodged somewhere other than where the investigator has looked. Why *should* the self be lodged in work? Why should not work be carried out as a necessary but tolerable evil, its encroachment on the rest of life appropriately curtailed, as Seeman (1967) has suggested? Perhaps a decline of institutional self-anchorage is less a result of institutional failure than a consequence of the discovery of a new vitality in the world of impulse. Shils (1962) calls our attention to the new kinds of experience made possible for us by the mass society. The merit of examining these shifts in terms of self-loci rather than merely alienation is that we are sensitized to the possibility that the self has found new anchorages as well as losing the old.

Concerning this initial statement of the two loci of self, the reader should bear in mind that specifying polar types such as these is merely a way to start thinking about variation in the sense of self. Except on the fringes of society, we are unlikely to find the extremes. Elements of both anchorages probably coexist comfortably in the average person. Yet differences among groups of people in key facets of self may be of sufficient importance that their experience of each other is noncongruent, and little true communication can occur.

#### A CONTEMPORARY TREND

It is my speculative hypothesis that over the past several decades substantial shifts have occurred away from an institution and toward an impulse emphasis. Accounts of the "new sensibility" in American culture (Bell 1970, p. 59) or of "consciousness III" (Reich 1970) already associate many of the same features with the youthful protest of the 1960s. But it would be shortsighted not to see the shift in a more extended historical context or to overlook the possibility of rural-urban differences, class differences, and differences among national cultures, as well as generational differences. A revolutionary consciousness often unwittingly adopts perspectives that have been growing in established society, frees them from accommodation to other aspects of that society, and applies them to a contemporary crisis.

There is nothing novel in attending to changing values over the last few generations. But I suggest that the changes be viewed as a shift in what are conceived as valid indications of what is real about ourselves and our associates, telling us whether we really know a person or not. Distinguishing the real from the unreal is a matter of intuition, not of logic. Faultless logic that concerns unreal objects falls on deaf ears. A shifting



locus of self means that successive generations are talking about different worlds of reality. At the heart of each are the shared and socially produced intuitions through which people identify their true selves.

Literary themes often presage shifts in popular consciousness. Examining the writings of James Frazer, Friedrich Nietzsche, Joseph Conrad, Thomas Mann, and Sigmund Freud, Lionel Trilling (1961) traces the theme that we must accept the reality of those human impulses that were judged unacceptable by an artificial and unreal civilization. He identifies "a certain theme which appears frequently in modern literature—so frequently, indeed, and in so striking a manner, that it may be said to constitute one of the shaping and controlling ideas of our epoch. I can identify it by calling it the disenchantment of our culture with culture itself—it seems to me that the characteristic element of modern literature, or at least of the most highly developed modern literature, is the bitter line of hostility to civilization which runs through it" (p. 26).

I have already noted Freud's penchant for the impulse perspective. Perhaps the greatest impact Freud had on the modern world was to discredit normative behavior and conscience as manifestations of our true selves and to elevate impulses to that position. Under his aegis, guilt has ceased to be the redemptive experience through which the real self reasserts itself and has become an external impediment to personal autonomy. Lynd (1958) exemplifies this newer intuition of reality when she writes, "Living in terms of guilt and righteousness is living in terms of the sanctions and taboos of one immediate culture. To some extent such living is necessary for everyone. Living in terms of the confronting of shame—and allowing shame to become a revelation of oneself and one's society—makes way for living beyond the conventions of a particular culture. It makes possible the discovery of an integrity that is peculiarly one's own and of those characteristically human qualities that are at the same time most individualizing and most universal" (p. 257).

Concern with discovery of the true self, vaguely identified as a set of impulses that have been repressed or dissipated under institutional constraint, turns up as a novel element in the political process of recent years. It became a prominent theme in youth movements, minority movements, and women's movements during the 1960s (Turner 1969). Miller (1973) traces the "politics of the true self" back to the poet William Blake and shows that violence is conceived of as the ultimate form of self-expression and self-discovery in the writings of Fanon and Sartre.

The term "soul" has often been used in much the same sense as our term "true self." It can be found in the work of poets as different as Richard Lovelace and William Wordsworth. But its meaning has changed to suit prevailing conceptions of personal reality. A century ago the soul was essentially a moral force. As secular psychology brought the term into

disrepute, it disappeared, sank into obscurity, reemerging to describe a special quality attributed to blacks. It retains its character as a dynamic force, but a supposed lack of inhibition is a crucial criterion of "soul."

Miller and Swanson (1958) documented changing conceptions of child rearing as new middle-class parents evinced less concern about internalized controls and more about social adjustment than did parents from the old middle class. In studies of another stage in life, students, as they progressed through college or university, were found to look more favorably on the expression of impulses (Feldman and Newcomb 1969, p. 34). If the inner-directed person of Riesman et al. (1950) has much in common with our institutionals, the other-directed person may have been a transitional type, clinging to the institutional framework for his identity but finding a way to accept constant change. Perhaps the total repudiation of institutional identities is the product of a growing sense of unreality in *all* roles that comes from the other-directed person's efforts to *be* all his roles. In the world of business, the shift is from the view that human relations take care of themselves when tasks are effectively managed to the position that human-relations engineering is essential to effective production. In education the progressive movement promoted a conception of the child in terms of his impulses, and not merely his learning and conduct. Rieff's (1966) depiction of cultural change as "the shifting balance of controls and releases" (p. 233) and his account of the "triumph of the therapeutic" describe a historical change toward greater impulsiveness.

Recently Lifton (1970) has described a type of personality he believes is becoming much more common throughout the developed world. His "protean man" has no true shape of his own but assumes varied shapes according to circumstance. Except for the fact that Riesman et al. describe other-direction as a mode of conformity, "protean man" may be a new name for the same kind of person. But the idea that rapid social change makes fixed identities unworkable has also inspired Zurcher (1972, 1973) to identify the "mutable self" as a phenomenon unique to the present generation. Zurcher cites as evidence for the "mutable self" his discovery that students no longer answer Kuhn's TST as they used to. Early use of the procedure produced mostly "B mode" responses, meaning that the subject identified himself with various institutionalized roles and statuses. Now students give principally "C mode" responses which specify characteristic modes of acting, feeling, and responding.

"C mode" responses clearly attenuate the linkage between self and institutional anchorage. The real self is marked by characteristic orientations—attitudes, feelings, desires—rather than characteristic placement in social organization. Young people find self-realization in patterns that are viewed apart from their institutional settings. Consistent with this evidence is the contemporary view that, on meeting a stranger, it is inappropriate to ask

where he comes from, what he does, and whether he is married, or to categorize him in other ways. Instead, one seeks to know him through his tastes and his feelings.

#### THEORIES AND EXPLANATIONS

If we are to explain and predict the self-loci of different populations, we must combine insights about individual dynamics with understanding of social structure and culture. The problem of individual dynamics is to identify circumstances under which experiences seem real or unreal in relation to self. Large-scale differences and shifts must then be linked with the exposure of individuals to such experiences.

An acceptable explanation must also address the question of whether impulse and institutional self-anchorages are the poles of a unitary variable, two separate variables related in a loose but imperfect inverse correlation, or even unrelated variables. If the conditions that weaken one locus intensify the other, a single continuum is indicated. But if, as seems more likely, the conditions that weaken one locus do not necessarily intensify the other, a more complex conception will be required.

My procedure has been inductive rather than deductive, starting with the observation of a difference and an apparent shift and then searching for explanations. Hence I now offer a set of alternative explanations rather than advance a single theory of self-locus.

#### Cultural Definitions of Reality

The most parsimonious principle is that people experience as real what they are taught is real. Definitions of reality are crucial components in all cultures, and a cultural shift may have occurred in recent times. Conceptions of reality are buttressed by systems of belief, and the decline of religious belief and its replacement by materialistic or naturalistic belief assigns more reality to physiological and allied psychological impulses. Cultural shift can explain the increasing personal reality attached to impulse as well as the declining personal relevance of institutions.

In tracing the historical evolution of public establishments designed to segregate deviants from the community, Rosen (1969) suggests that a new view of human nature came to the fore in the late 17th century, bringing with it the new idea of a personal self: "Today the idea of a personal self appears as an indispensable assumption of existence. Actually, like other views of human nature, it is in large measure a cultural idea, a fact within history, the product of a given era. At any given period certain criteria are employed to establish normal human nature, as well as any deviation from it" (p. 164). If Rosen's evocative suggestion is correct, the locus of

self was probably institutional at first. As Soellner (1972) interprets the recurring theme of self-discovery in Shakespeare's plays, the true self is formed in self-mastery. Cultural changes that occurred principally in the 19th and 20th centuries turned attention increasingly toward impulse as the expression of self.

While economical, this explanation is somewhat question-begging, since we can do little with it except ask the revised question, Why has a cultural shift occurred? But as a starting point, the cultural explanation is useful. A related and well-established line of sociological explanation emphasizes uniformity and diversity of culture rather than its specific content. According to this view, certitudes come from unanimity—the vitality and reality of an experience come from the fact that it fits unmistakably into a consensual world view. But the intimate experience of cultural diversity undermines certitudes. Institutional forms are seen in relativistic, rather than absolute, terms. No longer the locus of *real* behavior because they cannot be taken for granted, institutional frameworks begin to seem arbitrary and artificial.

While the latter construction escapes the danger of tautology in explanations that rely on the content of culture, it accounts for a decline in the institutional locus of self without indicating the basis for increasing impulse locus. By itself, this explanation suggests a generally weakened sense of identity instead of a shift from one locus to another.

### The Terms of Social Integration

A second approach redirects our attention from culture to the fundamental terms of interpersonal relations. Early sociologists (Cooley 1902; Young 1940; Davis 1940) believed that human nature depends upon social interaction. The human meanings of experience come out of the sharing and exchange of experiences. We tend to believe in experiences we share with others and doubt those we cannot share (Blondel 1928; Halbwachs 1952). Events become real and vital when they become elements in the bonding of interpersonal and group relations. The integration of an individual into solidary groups is the ultimate source of any sense of reality concerning experience.

Festinger (1953) proposes that we often comply with group norms without accepting them as our own, but that, when we wish to be part of the group, our compliance is converted into private acceptance. Much earlier, Park's (1928) and Stonequist's (1937) depiction of the marginal man stressed the interplay between adherence to a particular culture and the desire for social incorporation. Social rejection led the marginal man to doubt his commitment to the group's values and images of reality. Warner (1941) found confirmation of the conclusion that the stratification

people experienced as real was the hierarchy of social identities and affiliations their economic assets helped them attain, not economic standing *per se*. Davie (1947) found that immigrants of relatively high status who came to the United States as refugees from nazism felt they had lost in the move, even while acknowledging that their economic conditions were as good as or better than before. The loss of social recognition was salient in their estimate of overall position.

From this perspective, the self-conception should incorporate those actions and feelings that are involved in exchange with others and, through exchange, contribute materially to the individual's integration into groups. We are led to search for shifts in kinds of social relations that have relatively lasting and significant effects. One key may be the shift from the organization of group life around production and mutual protection groups to organization around consumption groups. From Veblen (1899), to Riesman et al. (1950), to Miller and Swanson (1958), observers have remarked a trend in the Western world away from production orientation and toward consumption orientation. At an earlier period, the central problems of society revolved about how to produce efficiently and on a massive scale the goods that people wanted. The individual's round of life was concentrated in extended families, neighborhoods, small communities, and other social units, all of them organized about the aims of production. Disciplined work habits, aspirations that motivated work, and adherence to rules that facilitated collaboration for production were valued qualities that enhanced individual integration into stable and comprehensive social units. Since the same units were also vested with responsibility for mutual protection, altruism was the companion to productive contribution in making the individual an essential group member.

As production problems were increasingly resolved and production routinized and transferred to the domain of organizations with strictly segmental functions, the connections between production-related personal qualities and integration into primary groups became more tenuous and indirect. Likewise, protection became the routinized responsibility of experts who were external to the groups in which people lived. An altruistic act by an amateur could do more harm than good—witness warnings against amateurs moving automobile accident victims or trying to save drowning swimmers instead of waiting for paramedics or life guards! But of even greater significance, altruism toward a stranger has usually no significance for lasting social ties and identities in a highly segmented society. As their significance for interpersonal bonding declined, altruism, achievement, and righteousness became less credible clues to the real person.

As the consumption of goods, expressive activities, and pleasure became the aims about which the anchoring social groups were organized, a different set of qualities provided the interpersonal cement. The cultivation of

personal tastes, expressive styles, and distinctive psychological "needs" was at a premium in groups whose vitality and continuation depended on collaboration for consumption. The family was increasingly viewed as a contrast, rather than an adjunct, to the world of production. When the links to production could not be totally severed, some relegated the family to the same sphere of unreality. Ogburn (1933) and Burgess and Locke (1950) noted that interpersonal response was becoming more prominent as a source of family cohesion.

This approach is consistent with the hypothesis that the institutional self is less prevalent and the impulsive self more so among segments of the population for whom production activities are aimed at maintenance rather than expansion. Rural and small-town folk are often characterized as bumpkins because they do not seek to play such rituals as initiations and funerals to dramatic perfection and fail to play their vocational and community roles to the hilt. I suggest that these are groups in which production is routinized and hence constitutes a less salient social bond. Where a genuine spirit of entrepreneurship prevails, the expansive aim makes routinization impossible, and the institutional self seems more real.

#### Deprivation and Desire

From psychodynamics comes a third principle that might explain varying loci of self. Sustained denial of any goal or impulse causes a preoccupation with the blocked tendency which makes the latter seem more real and important. While this is a cardinal Freudian assumption, it has a venerable history and is entirely consistent with George Mead's conception of impulse and social act. Lee (1959) gives the same process a different slant when she attempts to explain why Americans conceive of psychodynamics in terms of basic human needs instead of values: "In maintaining our individual integrity and in passing on our value of individualism to the infant, we create needs for food, for security, for emotional response, phrasing these as distinct and separate. . . . We create needs in the infant by withholding affection and then presenting it as a series of approvals for an inventory of achievements or attributes" (pp. 74-75). By contrast, in Hopi, Tikopia, Kwoma, Arapesh, Ontong-Javanese, and many other societies, value is undivided and positive. There is no separate motive or need for food, security, or social response.

Whether we start from Freud's or Lee's contrasting assumptions about the ultimate character of human motivation, we find agreement that modern urban, industrial civilization requires a great deal of control and suppression of impulse. If these requirements have increased, the result should be enhancement of the sense of reality associated with impulse. If modern civilization has also frustrated mankind's need for interpersonal response

(Horney 1937), it is easy to understand that people may experience their longings for intimacy as manifestations of their real selves. We encounter more difficulty in developing a plausible account of lessened frustration in institutional spheres. Perhaps routinized social services and prepackaged solutions to all problems of production have eliminated the anxieties and potential frustrations in these areas and deprived such activities and concerns of their personal vitality.

Experience has shown how difficult it is for scholars to agree on such broad characterizations of societies as impulse repressive. A more plausible view may be that modern society is not so much repressive as it is contradictory, stimulating to an unparalleled degree the impulses whose expression is then inhibited. But if this is true of impulses Freud would have lodged in the id, it is equally true of achievement and service motivations that he would have placed in the superego. To some extent this entire approach may rest on a mythical conception of uninhibited life in preindustrial societies, based on selective reporting and disproportionate attention to premarital sexual play.

### Opportunities and Consequences

A final principle is pragmatic and in some ways antithetical to the deprivation-enhancement approach. The postulate is that lines of action with plainly perceived and significant consequences are experienced as real, while ones with undependable or unidentifiable consequences seem less real. Consequences cannot be experienced without opportunity, so the reality of particular goals and impulses is enhanced by either augmented consequences or increased opportunities.

The waning vitality of institutional dispositions can be traced to the emasculation of rites of passage and the permeability of role boundaries. Disappointment over rites of passage, leading to a generalized sense of unreality, is poignantly conveyed in the recently popular song "Is That All There Is?" Each of life's milestones turns out to be of little moment. A vital rite of passage initiates the individual into closely guarded secrets and skills and opens up new privileges, conditional upon his successful performance or endurance of the rite. When secrets, skills, and privileges are readily accessible to those who have not been through the rite, and when passage becomes pro forma, the entire sequence of institutional steps loses its reality. Likewise the potential of any institutional role for self-anchorage depends upon the degree to which it incorporates distinctive privileges, responsibilities, and skills. Such monopolies may have been progressively undermined in recent years.

There may be a key to the changing sense of what is real in the fate of

the once-popular belief in self-discovery through self-sacrifice. One side of institutional identity is incorporation into the self of a whole range of socially generated objects that augment the interest, richness, and possibilities of gratification in life. The obverse, however, is the relinquishment of those random sensations and impulses which—like Blondel's (1928) *cénesthésie*—offer a constant threat to the ordered world of the institutional self. From the poet Francis Thompson (1893), writing in religious terms, to the psychoanalyst Fromm (1941), students of human nature have understood the appeal of self-surrender. For Thompson, the errant soul flees the Hound of Heaven in desperation until in abject surrender he discovers the fulfillment of his deepest desires. For Fromm, identification with an omnipotent, charismatic leader and his movement resolves the problems of the troubled self.

But implicit in self-discovery through surrender is the assumption that one has been absorbed into a caring, gratifying entity, both powerful and dependable. When Ruth of the Old Testament found a true self in surrendering her life to Naomi, she did so in the firm knowledge that Naomi would care for her, protect her, and give her a meaningful and rewarding place in her household. Thompson assures us that to those who surrender, God will grant many times more than they have given up. It is this rich return for surrender that vests the institutional self-conception with a vital sense of reality. The discovery of self in love for another is made real by the reciprocated love, the dependability of the relationship, and the new opportunities for gratification that come from the relationship. The discovery of self through immersion in an institutional framework is real when the dependability of that framework makes the world predictable and the rich body of objects opens up a new world of gratifications.

But if reciprocation does not occur, neither does the vital sense of a real self. When the institutional framework is characterized by disorder and undependability, when it fails as an avenue to expanded opportunity for gratification, the true self cannot be found in institutional participation. Because *perceived* consequences are crucial, objective order and dependability may be less important than whether consequences are early or delayed and whether effects are intrinsic or extrinsic. Here the analysis converges with many treatments of alienation in the Marxian tradition. The institutional order may still be relatively efficient and predictable, but the increasing time span between action and consequence and the increasing dependence on extrinsic rewards may contribute to a sense of unreality in institutional activities.

On the other hand, as Shils (1962) has eloquently indicated, mass society opened up opportunities for an augmented range of gratifications. As existence became less precarious, people could afford to act on their im-



pulses. More of society's resources could be applied to creating avenues for the gratification of impulse:

The individual organism has become a seeker after experience, a repository of experience, an imaginative elaborator of experience. To a greater extent than in the past, the experience of the ordinary person, at least in youth, is admitted to consciousness and comes to form part of the core of the individual's outlook. There has come about a greater openness to experience, an efflorescence and intensification of sensibility. . . . In a crude, often grotesque way, the mass society has seen the growth, over wide areas of society, of an appreciation of the value of the experience of personal relationships, of the intrinsic value of a personal attachment. . . . [Pp. 58-59]

In accordance with this more positive view of mass society, the sense of unreality attending institutional activity may be a crisis phenomenon, related to a stage in life or restricted to a small but vocal segment of the population. On the other hand, a relative decline in institutional self-anchorage may have occurred just because of the newly discovered vitality in the world of impulse. Instead of alienation from our institutions, we may be witnessing a reestablishment of balance between institutional and impulsive loci of the self.

### Alternative Theories

I have by no means covered all plausible approaches to explaining the hypothesized shift in self-anchorage. For example, the massive amount of literature on patterns of deferred gratification should offer clues. Nor am I prepared to recommend a choice among theories. The major conflict is between deprivation theory, on the one hand, and opportunity theory and social integration theory, on the other. But even here the opposition may not be complete.

Evidence and experience suggest a curvilinear relationship between deprivation and the sense of reality. Severe and continued deprivation probably leads to a lessened sense of reality and sometimes even a complete divorcement between self and desire. "Narcotization" (Koos 1946, p. 48) and "analgesic" (Ball 1968) are terms sociologists have used to designate a pervading sense of unreality that stems from exceptional deprivation.

Furthermore, relative rather than absolute deprivation is probably the key. Hence, stimulation of desire without equivalent increase in gratification should also increase the sense of reality. With this modification, the deprivation approach need not be inconsistent with the others. Changing definitions of reality may be the source of, or bring an end to, relative deprivation. Modest relative deprivation may be the normal consequence when new opportunities for gratification become available. Just as com-

petition may increase the interest in work of both winners and losers (Myers 1962; Julian, Bishop, and Fiedler 1966) and a healthy anxiety enhance the excitement of love, the combination of accessible gratification and some degree of risk may create the most vital sense of reality. Individuals today may experience their impulses as more vital expressions of self than heretofore, because opportunities to gratify impulse have increased and norms against doing so have weakened, at the same time that lingering inhibitions from the past and contradictory cultural definitions add an increment of risk to the expression and pursuit of impulse.

Application of this approach to institutional activities suggests two phases. An early period of institutional innovation greatly elaborated the scope of potential human gratification, through family, work, community activity, and other institutional spheres; yet the growing and changing nature of the institutions maintained a universal element of risk. This combination of promise and risk led to a more profound personal investment in institutional pursuits than formerly, with the result that more people experienced their true selves as distinctively lodged in these spheres. As institutions came to perform more smoothly and routinely, the risk was reduced and the sense of reality declined. Thereafter ensued the second phase, already amply described.

#### IMPLICATIONS FOR SOCIAL STRUCTURE

Any massive shift in the locus of identity should have substantial consequences for social structure and may negate the implicit assumptions on which some sociological theories are founded. Theories deeply rooted in the past often take for granted an actor who locates his real self in an institutional setting. Conflict theories are no different from order theories in this respect, since they merely shift the locus of self to a class-bounded institutional framework. It is difficult to find sociological theories that locate bases for order rather than pathology when institutional participation is strictly instrumental and when only those impulses that seem unrelated to institutions are experienced as genuine and personal. I shall suggest how alternate loci of self bear on a few major concepts in sociology.

##### Role Distance

Few would deny that society exists only when members are enacting roles. Sartre (1956, p. 59) argues that society requires every person to play his roles to the hilt. Role theory assumes that role behavior is monitored and evaluated on scales of role adequacy. Evaluations by the actor and significant others influence subsequent role performance. When role theory is refined in accordance with self theory, evaluations of role adequacy are

understood to have the greatest impact for roles that are paramount in the self-conception. Whatever society may expect, the actor usually plays to the hilt only those roles in which his ego is strongly involved (Sherif and Cantril 1947). So long as the roles attached to institutionally *key statuses* (Hiller 1947) are also most salient in personal identities, institutional and personal dynamics are mutually supporting.

But Goffman (1961) has identified a pervasive phenomenon of *role distance*. The role-distancing antics of parents on the merry-go-round with their children plainly ward off identification with a role that does violence to their self-conceptions. But when the surgeon engages in role distancing with nurses and assistants during a difficult operation, he can hardly be guarding against inappropriate institutional placement and identification. The distance is between the real person and *any* formalized role. If role distancing in such situations is as prevalent and important as Goffman suggests, it is difficult to accept Sartre's assertion.

The apparent contradiction between these two sensitive observers may come from their having viewed different cultures at different moments in time. When the self is securely lodged in institutions, the individual will play at least the most crucial roles to the hilt. When he does so, he will appear genuine to his institutional auditors. When he does not, he will seem insincere, because role distancing behavior is inappropriate flippancy, a denial of respect to significant others in the encounter. Indeed, the behavior of Goffman's surgeon would be so regarded in many medical circles. But when the true self is lodged in impulse, role distancing shows that one is not really the uptight, false, or plastic person his conformity with institutional routine might suggest. Role distancing reminds significant others of a real self that is temporarily obscured by compliance with an institutional role.

On the one hand, organizational theorists should take account of role distancing as a crucial aspect of the meshing of person and institution. On the other hand, role distancing is probably not a universal feature of social behavior but a distinctive feature of societies like our own, in which the locus of self is widely found in noninstitutional impulses.

### Values and Norms

Sociologists distinguish between norms and values as part of the directing and regulating apparatus of society. Because of their preoccupation with distinguishing between these two components of social structure, sociologists often fail to notice that the difference is principally in the eye of the beholder: values and norms are largely the same phenomenon viewed in different ways. Honesty is a value; "thou shalt not lie" is a norm. But the two are inseparable, and, as Blake and Davis (1964) argue per-

suasively, neither can be derived from the other. But the object (value) and the rule (norm) are two constructions the individual can place on the same phenomenon. Which construction is foremost in his experience makes important differences in his relationship to social structure.

Honesty is a positively valued object, a goal to be achieved as a matter of self-respect and pride. One who values it does not feel that society is depriving him of the privilege of telling lies when he wishes, nor does he feel frustrated or constrained when he passes up the temptation to lie. Instead he has a positive sense of accomplishment and well-being from having lived up to his values. But one who thinks in terms of the norm experiences deprivation and frustration. Like a driver adhering to the speed limit on an open highway, he gains no warm sense of worth and satisfaction from adhering to the rule; even when complying he often feels resentful of the restraint imposed upon him. The practice of premarital and marital chastity has been variously experienced as an opportunity for self-development and as an external restraint on a fundamental human need. *Self-as-impulse tends to transform the institutional order into a set of norms, all cramping expression of the true self. Self-as-institution subordinates the normative sense to a set of values, such as integrity, piety, patriotism, considerateness, and many others.*

Sociologists frequently prejudge the constructions people place on the social order and reify them into features of an objective social structure. They emphasize the constraints of society by seeing principally norms, or they emphasize the creation of new opportunities for self-fulfillment by underlining social values. A more adequate sociological understanding can be reached if we stop reifying the value-norm distinction and seek instead to understand what it is in the social structure that fosters anchorage of the self in either institution or impulse, with the resultant predisposition to see either values or norms in the institutional order.

### Sentiments

Much of the spontaneous joy that lubricates the functioning of social orders resides in the social sentiments. Love is of paramount importance among the sentiments. Because sentiment seems to express the inner person, in contrast to external behavior that may be contrived, people seek agreement on signs by which to tell genuine from false sentiment. The choice of cues reflects the anchorage of self. Self-as-impulse can feel love as genuine, as a true reflection of self, only when it arises and persists as a spontaneous attachment, untrammelled by promises, covenants, and codes of behavior. Sentiment is not helped along by a facilitative social order: it erupts in spite of the order and threatens it. The less organization and preparation, the more easily can the individual discover his true sentiments.

Institutionals, on the other hand, understand love as something that requires effort to attain and preserve. The infatuation that explodes impulsively is undependable and unreal. The institutional seeks to learn how to achieve true love and turns for guidance to such documents as Paul's chapter on love in the New Testament (1 Cor. 13). The contrasting perspectives are represented in the analysis of popular sex manuals by Lewis and Brissett (1967). Manuals popular with married middle-class people in the past two or three decades are institutional in orientation. They offer readers an opportunity to enhance the vitality and mutuality of sexual experience, leading to a deeper union of the two selves. But Lewis and Brissett read the manuals from the impulsive perspective. Stripped of the institutional perspective, the quest for mutual self-attainment becomes sheer, meaningless "work." Thus, they write of "sex as work." To the extent to which the self-locus has moved away from institutions, the correlations found by Burgess and Cottrell (1939), Burgess and Wallin (1953), and Locke (1951) with persistence and love in marriage may become increasingly invalid, and new and different indicators may become relevant.

### The Meaning of Ritual

In 1930, an article entitled "Ritual the Conserver" (Cressman) appeared in the *American Journal of Sociology*. It elaborated the crucial part played by ritual in sustaining the Catholic church and its doctrines. To a contemporary reader, the paper seems peculiarly unconvincing. To those who find not only religious ritual but also marriage ceremonies, funerals and memorial services, initiation ceremonies, and graduation exercises devoid of meaning, it is unclear how ritual could add vitality and reality to anything. Yet plainly many people have been, and continue to be, moved deeply by participation in collective ritual, and for many people dedication to institutional goals and forms is strengthened in this way. The locus of self must be closely intertwined with the ability to gain a vital experience from engaging in collective rituals. It would be premature to label one cause and the other effect, but the impulsive's self-fulfilling prophecy that he will not experience his real self through participation in institutional ritual contrasts with the equally self-fulfilling prophecy from the institutional.

But the matter cannot be reduced to a differential receptivity to ritual. Writing on the "collective search for identity," Klapp (1969) describes the contemporary poverty of ritual, then insists that "ritual is the prime symbolic vehicle for experiencing emotions and mystiques together with others—including a sense of oneself as sharing such emotions . . ." (p. 118). In the place of traditional forms, there have arisen new rituals that participants experience as spontaneous outpourings instead of institutional

routines. Sitting on the floor in a circle and singing to the accompaniment of guitars takes the place of sitting in rows on pews and listening to an organ. Rock festivals and love-ins are only the more dramatic rituals, for even the conventional partying rituals of middle-class establishmentarians are experienced as a welcome contrast to institutional routine. Here, then, is another set of rituals that have meaning and vitality as opportunities for experiencing a self that contains more impulse than institution.

Ritual is commonly viewed as a support of the institutional order, and Klapp's "poverty of ritual" does indeed characterize many of the forms that have been employed to strengthen a collective sense of institutional commitment. But it is doubtful that there is any poverty of ritual today in those forms that increase the vitality of an impulsive view of self.

### General Implications

Each of the foregoing points bears on the theory of social control. Concern with the prestige of one's role and the esteem that goes with high role adequacy buttresses the institutional structure. A sense of value eases the pathos of conformity with social norms. Social sentiments domesticate potentially disruptive emotions yet preserve their sensed vitality and spontaneity. And through collective ritual, group solidarity and dedication to the institutional structure are continually renewed. But all of this depends upon the individual's feeling that his real self is engaged in these experiences. If he finds that self elsewhere, control can only be instrumental.

One side of what I have described sounds much like a condition of anomie. But if people recognize their real selves in impulse, they become susceptible to social control from that quarter. And if impulses are generated in unrecognized ways by the social order and follow unsensed but consistent patterns, the impulsive self is as much a vehicle for social control as the institutional self. However, the relationship to recognized institutional structures will be complex and indirect. We need a theory of social control that relies more extensively on the creation and manipulation of situations and on symbiosis than on the internalization and enforcement of norms and values.

### AFTERTHOUGHTS

My chain of speculation has consisted of five links. First, I have assumed that each person develops at least a vague conception by which he recognizes some of his feelings and actions as more truly indicative of his real self than other feelings and actions. He does so in part on the basis of unique experience and in part in accordance with the guidelines shared by members of his society, subsocieties, and groups. Second, I have identified

a distinction between locating the real self in sentiments and activities of an institutional and volitional nature, such as ambition, morality, and altruism, and recognizing the real self in the experience of impulse—for example, in apparently undisciplined desires and fancies and the wish to make intimate revelations in the presence of others. This appears to be a significant polarity in the contemporary relationship between individual and society. Third, I have assembled a number of suggestive indications to support the intuition that there has been a long-term shift in the direction of impulse, intensified in the last decade or so. Fourth, I have drawn from analyses of contemporary society to suggest four theoretical approaches that might explain the shift in self-loci and differences in self-anchorage among diverse population segments. And fifth, I have assumed that many sociological theories, especially those having to do with social control, take for granted a population with institutional self-anchorages and need revision to allow for the more hidden ways in which societies control members who find their true selves in impulse.

I stress that there is no objectively, but only a subjectively, true self. Likewise institutional versus impulsive self is a subjective distinction and not a statement of psychogenesis. Institutional motivations are effective vehicles for a multitude of private impulses. And few of the instigations and sensations that people experience as impulse are not institutionally conditioned and generated. We are like the Plains Indians who sought purely personal visions to establish the basis of each individual's authority but faithfully replicated their culture in the form and content of their visions (Benedict 1934, p. 84).

One might suppose on the basis of attribution research that the self should necessarily be anchored overwhelmingly in impulse rather than institution. When an external cause is present, "the behavior is discounted, so to speak, as an indicator of personal disposition" (Kelley, in Jones et al. 1971, p. 155). But there are two important differences. First, although we understand achievement, ethical conformity, and altruism as rooted in institutions, the pattern is internalized, so there need be no sense of an external cause. Second, when we ask people to describe circumstances that reveal their true selves, the institutional replies usually involve going beyond requirements of a role or adhering to goals or norms in the face of obstacles that constitute acceptable excuses for failing to do so. Thus the behavior in question is unlikely to be ascribed to external causes.

As we explore the polarity of institution and impulse, we must ask whether choice between the two is of universal significance or peculiar to certain cultures and social structures. Thomas and Znaniecki (1918) discovered philistines and bohemians, but the fact that the society they studied tended to dichotomize people thus and force each person to choose between those polar types was probably more important than the objective typing

of individuals. Mannheim ([1928] 1952) described the distinctive socializing experience that sets one historical generation apart from others in terms of the *issues* over which people divide into rival camps rather than the association of a point of view with a generation. We must not, then, assume that a distinction between institutional and impulse self-anchorages found to be significant in contemporary American society should necessarily be significant in other societies.

We may indeed assume that because the polarity is important, most individuals will seek both kinds of anchorages. For much of life, these alternate anchorages will coexist in fairly easy accommodation. But at crucial transitions in the life cycle the coexistence will be interrupted. The latent opposition between institutional and impulsive selves then becomes manifest, figuring strongly in the turmoil of choice. The point of choice may be passed and the conflict recede into dormancy without the individual's self-conception being firmly anchored at one pole to the exclusion of the other.

Perhaps the feature of the distinction that warrants greatest stress and is most heavily freighted with implications for examining social structure is the hypothesized correlation between self-locus and a disposition to perceive either values or norms. Sociologists writing from a structural perspective often reify the distinction and speak as if values and norms existed as separate entities in society. To a large degree, however, it is the nature of the self-conception, in the way it identifies the individual's relationship to social structure, that leads one to perceive principally values or norms. Perceiving values and discovering the true self in the achievement of high institutional-role adequacy facilitates the operation of social control systems as they are commonly described in sociological theory. Perceiving norms and recognizing the true self specifically in the experience of impulses that are thought not to have an institutional basis subjects one to quite a different system of social control, one that is much less well described in sociological theory.

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# On the Emergence of Oligarchy in Human Interaction<sup>1</sup>

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The elementary conditions of human interaction generate Michels's (1910) "iron law of oligarchy" and Mosca's (1896) major proposition on system size and ruling elites, the latter holding without regard to the definitional restrictions recently examined by Mayhew (1973). Specifically, the structure of focused face-to-face interaction constrains chance expectations for power relations. Power is constrained to equalize (by chance alone) as interaction sequences increase in length. Power is constrained to polarize (by chance alone) as group size increases. Since interaction sequences falling within the operative range of human perception as specified by Miller (1956) and Simon (1974) are relatively short, power will be increasingly concentrated as a function of group size in most situations, yielding Michels's law and Mosca's proposition. As these regularities hold at much smaller sizes than originally specified by Michels (1911), their implications for macro-level control structures are examined in light of the results. It is concluded that control structures at the macro level are likely to be affected by considerations above and beyond those imposed by face-to-face (micro-level) interaction.

Recent social science literature has redirected attention to the effects of group size on power structure (e.g., Niemi and Weisberg 1972; Coleman 1973; Dahl and Tufte 1973; Noell 1974). There is considerable precedent for this concern in the classical literature.<sup>2</sup> In fact, Michels's "iron law of oligarchy" was originally hypothesized to hold only after a critical point in size had been reached: a point somewhere in the range 1,000–10,000 (Michels 1915, pp. 26–27).<sup>3</sup>

<sup>1</sup> The senior author thanks Sybil Segraves for assistance in typing the manuscript.

<sup>2</sup> Overviews of the classical literature are provided by Bottomore (1964), Parry (1969), Pateman (1970), and Dahl and Tufte (1973).

<sup>3</sup> Although Michels attributed the insight of a critical size range to Rousseau (1762, p. 97), the correlation between size and form of control structure was noted earlier by Montesquieu (1748, p. 199). Furthermore, Michels based much of his theoretical statement on the works of Mosca (1884, 1896) and Pareto (1896–97), especially the former. The most complete statement of Pareto's theory appeared after Michels had posited his

The objective of the present inquiry is to show that group size not only generates Michels's law but that it does so far below the size range he specified. Incidental to this analysis, we show that group size generates Mosca's (1896) proposition on the relation between the relative size of a controlling component and the size of the system it governs without recourse to the definitional restrictions examined by Mayhew (1973).<sup>4</sup>

Specifically, we show that the elementary conditions under which human interaction generally occurs constrain power (1) to equalize as the length of the interaction sequence increases and (2) to polarize as size of the group increases. Michel's law is generated for two tipping points of size, one beyond which it holds probabilistically and the other beyond which it holds deterministically, that is, becomes the "iron" law of oligarchy. The same conditions yield Mosca's proposition but without definitional restrictions which require the controlling component to be an elite; that is, his proposition holds across the full range of size without regard to whether the controlling component is a minority. The result of our analysis is a series of propositions indicating how variation in the form of power structure will be a function of group size, by chance alone.

#### THE CONDITIONS OF FOCUSED INTERACTION

In situations of face-to-face interaction, human communication occurs as a sequence of objective events ordered in time. This fact is so well known that it is more often implicitly assumed than explicitly stated. The reasons for this serial order in communication are essentially the same as those which compel temporal sequences in the behavior of the individual organism (Lashley 1951). The limitations of the human nervous system are such that the organism cannot operationally assimilate a variety of simultaneous messages received in the same sensory mode (Broadbent 1971). If two or more persons are speaking at the same time, communication fails to occur: the listeners receive only noise. Shouting matches, collective laughter, and similar outbursts disrupt the interaction process because they destroy the thread of ongoing communication, even if only temporarily (Lindsay and Norman 1972).<sup>5</sup>

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"iron law" (see Pareto 1916). Of course, all the Italian theorists were familiar with Machiavelli (1532).

<sup>4</sup> As regards Mosca's proposition, it should be noted that the recent analysis by Freeman and Kronenfeld (1973) does not bear upon the question. Their results are due to an assumption they make (1973, p. 109), not to definitional dependency, as their title suggests.

<sup>5</sup> This is not to say that all instances of simultaneous transmission are devoid of communicative content. They can be quite meaningful in collective singing, chanting, and other ceremonial enterprises, but only because signals are coordinated to ensure harmonious patterns decipherable by the human nervous system (see Geldard 1972).

The noise-generating effects of simultaneous transmission yield what is probably the only universal norm in all human societies: interrupting an ongoing conversation is taboo. In casual groups it is merely impolite; in task-oriented groups it may be strictly forbidden. As the maintenance of adequate communication has survival value for groups, the stringency of the sanction is probably a direct function of the degree of environmental uncertainty they face (Cohen 1968). At a cocktail party simultaneous transmission is no more than a *faux pas*, but in a war party it can be fatal (Radin 1927).

We shall use the term "focused interaction" to refer to situations in which face-to-face communication proceeds without interruption from either simultaneous transmission or conversational asides.<sup>6</sup> In focused interaction, all group members attend to ongoing communication regardless of whether particular statements are addressed to them individually. For purposes of the task at hand, we consider only groups operating under the conditions of focused interaction. This is not as much of a limitation as it might seem. The conditions of focused interaction identify the vast majority of all human groups: if interaction ceases to focus, the group ceases to exist as an interaction system, however temporarily.<sup>7</sup>

#### INTERACTION SEQUENCE AND POWER STRUCTURE

An interaction sequence is an ordered set of elements. While such sequences may be conceived in a variety of ways (see Collins 1967), our concern is only with actor sequences. In actor sequences the occurrence of an element in the sequence indicates that a particular actor is associated with the type of interactive event under consideration. In the present case, the actor's appearance in the sequence will indicate that he has achieved an instance of effective control over the action of the group as a whole. This selection is not arbitrary: it is required for evaluation of the theories formulated by Michels and Mosca. The classical elite theorists were not concerned with such side issues as whether George controls John. They dealt with the operational control of the system itself. Accordingly, assessment of their theories in situations of focused interaction restricts our analysis to this distinctive class of interactive events.

<sup>6</sup> Conversational asides not only break focused interaction, they constitute instances of differentiation into subgroups (Goffman 1959). To the extent that interaction ceases to focus—to the extent that all group members do not attend to ongoing interaction—subgroups are forming. Thus, in an identifiable aggregate of persons, such as one sees at a cocktail party, the number of people talking at the same time is a measure of the number of groups that have formed, bearing in mind that lone individuals are not groups.

<sup>7</sup> Although we repeatedly make reference to the "face-to-face" aspect of focused interaction, it clearly is not necessary. A telephone conference can exhibit the serial order of communication observed in focused interaction.

Sequence Structure<sup>8</sup>

However, we shall return to questions of power. For immediate purposes, we are concerned with a person's appearance, in sequences, defined by the event of having achieved an instance of system control. For example, for a triad comprised of individuals A, B, and C, various series of these letters, such as AAA, BCB, CCB, ABB, ABC, etc., indicate interaction sequences and the order in which participants achieve instances of system control. In the example AAA, actor A has achieved such control three times in succession; in the example ABC, each of the participants has achieved an instance of system control in the order indicated.

Our concern is with inequality in sequence structure. Following Mayhew, Gray, and Mayhew (1971), we define a *coefficient of sequence inequality* ( $K$ ):

$$K = \frac{1/2 \sum_{i=1}^N \sum_{j=1}^N |p_i - p_j|}{N - 1}, \quad (1)$$

where  $p_i$  = the proportion of "instances of system control" achieved by individual  $i$  out of all the instances defining the sequence;  $p_j$  = the proportion of "instances of system control" achieved by individual  $j$  out of all the instances defining the sequence; and  $N$  = the number of individuals in the group, that is, group size. Clearly,  $K = 1$  for any sequence where a single actor dominates interaction, as in the sequences AA, AAA, AAAA, AAAAA. On the other hand, we have  $K = 0$ , indicating complete equality in interaction, only when each actor can appear the same number of times in the sequence. For example, in triads, the sequences ABC, AABCC, and ABACBC all have  $K = 0$ , or complete equality. Intermediate values of  $K$ , between 0 and 1, are determined by the length of the interaction sequence, the number of persons in the group, and the specific sequence configuration observed. Thus,  $K$  provides a measure of inequality in sequence structure (power structure) ranging from 0, or complete equality, to 1, or complete inequality.<sup>9</sup>

<sup>8</sup> An extensive treatment of interaction sequences, defined for communication in general, appears in Mayhew et al. (1971). The conception of interaction sequence employed in the present paper follows that formulation and should not be confused with sequences that are defined across group boundaries (e.g., Blain 1971).

<sup>9</sup> The classical elite theorists considered power to be a purely structural phenomenon, as do most contemporary students of the subject (e.g., Wrong 1968; Rogers 1974). The  $K$  measure adopted here, developed by Mayhew et al. (1969) for the study of structural relations in small groups, fits our purposes exactly. It calculates the *relative* positions of all individuals simultaneously, providing a characterization of power as a purely structural phenomenon. Some other measures of inequality in interaction, devised for more specific conceptions of power, are reviewed in Bartos (1967).

Of course, a basic consideration is the identification of a sequence. If we are to be able to study structural stability and change over time, we must know when one sequence ends and another begins. As a matter of fact, the propositions we derive relating power structure to group size hold independent of sequence length: they are true for any sequence length. Nevertheless, there are several reasons for identifying an appropriate range of sequence lengths.

All human interaction sequences are finite in length, even though some of them may be quite long. But, if we always take the maximum length which occurs, we never capture the dynamics of interaction and can never study structural change over time. To summarize all interactive events over several sessions of interaction is static and deceptive. For example, in a triad, a sequence like AAAAAAABBBBBBBBCCCCCCC yields complete equality ( $K = 0$ ) in the overall summary configuration, but if each session of interaction lasted for length seven, then power was completely polarized ( $K = 1$ ) at any one session. As sociological analysis is concerned with the forms assumed by power structure over time, we must decide, at least tentatively, upon a range of sequence lengths that are operationally relevant to human interaction. Fortunately, this decision does not have to be arbitrary.

Just as they constrain human interaction to sequential order, the limits of human information processing place an upper bound on the length of operationally relevant interaction sequences. Summarizing a vast array of experimental evidence on human perception, Miller (1956) finds that the operative range of human "primary memory" is somewhere in the neighborhood of seven, plus or minus two, items of information. He suggests that the average range of events which can be simultaneously retained in human primary memory is approximately seven, and in any case never exceeds about 10 (1956, pp. 90-91). More recently, Simon (1974), also basing his conclusions upon extensive experimental data, suggests the "chunk" as a basic unit of information in assessing the capacity of human primary memory. He finds (1974, p. 487) that the number of "chunks" (of information) that can be held in human primary memory is five, which he proposes to replace Miller's seven. These findings have clear implications for the study of human interaction. On the basis of existing evidence, which is quite extensive, it appears that humans cannot simultaneously conceptualize an array of more than five to nine significant events from recent experience. Accordingly, their operating mental images of transpired interaction would not permit them to react to the development of structure in interaction beyond what has occurred in sequences of this length.<sup>10</sup>

<sup>10</sup> As some academicians have tended to emphasize the "unlimited potential" of the human mind, skepticism concerning such short memory spans and their relevance to



Bearing in mind that future evidence may revise present estimates, we tentatively adopt sequence length nine as the upper bound for sequences that are operationally relevant to human interaction. This will not constrain the applicability of propositions we derive relating power structure to group size: as indicated previously, they will hold for interaction sequences of any length. However, it provides a limit for relevant illustrations, as well as a guide for researchers who may wish to explore the implications of our results.

### Power Structure<sup>11</sup>

The classical elite theorists conceived of power as an objective phenomenon: they were concerned with the actual control which occurs in social systems. Nothing could be more alien to their view than the conception of power as a "potential," as when it is defined in terms of resources or dependency relations (e.g., French 1956; Emerson 1962; Rogers 1974). They were concerned with what happened, not with what might happen. Accordingly, at the level of social interaction in face-to-face groups, our use of the term "power" or "control" must be confined to what Mayhew, Gray, and Richardson (1969, p. 475) call "operating power structures," that is, to "the actual control achieved in terms of behavior exchanged by persons and groups."<sup>12</sup>

Since focused interaction proceeds as a series of events ordered in time, we might consider cases in which individuals simply dominate the interaction process itself: control may then reduce to relative participation in communication, so that whoever talks the most controls the interaction

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human interaction is understandable. Insofar as our own inquiry is concerned, it should be noted that the events which define power structure may be embedded in a longer series of interactive events. Thus, an interaction sequence of length five or seven as conceived here with respect to power structure may represent much longer sequences with respect to communication in general. Accordingly, the time span and range of events over which human primary memory must operate to visualize power structure may be quite long, representing hours, or even days, of interaction. In the one empirical study which bears upon this question, Riecken (1958) finds that even in small groups, individuals are generally unable to identify the overall form of the power structure, except in extreme cases. More generally, the frailty of the human nervous system as an information-processing instrument has long been recognized (Ibn Khaldun [1377] 1967, pp. 414-15). Strong theoretical arguments (Wallace 1961) and a myriad of empirical evidence (Broadbent 1971; Lindsay and Norman 1972) attest to the limited capacity of the human nervous system. Indeed, its capacity for errors in the transmission of information is notorious (Campbell 1958; DeFleur 1962).

<sup>11</sup> Overviews of the literature bearing upon the distinctions we maintain here between operating power structure as actual control achieved and latent-force definitions for potential control appear in Cartwright (1965), Rose (1967), and Wrong (1968).

<sup>12</sup> Empirical analyses of operating power structures include Mills (1953), Hamblin (1958), Kaufman (1960), Walter (1966), Gray, Richardson, and Mayhew (1968), Richardson, Mayhew, and Gray (1969), and Richardson et al. (1973).

process most. This conception might be appropriate for groups whose *raison d'être* is nothing more than interaction, for what have been called "expressive," "ceremonial," or "primary" groups (Cooley 1909). However, to align our inquiry with Michels and Mosca, it is more appropriate to consider what are called "instrumental," "task-oriented," or "secondary" groups (Olmsted 1959).<sup>18</sup> The type of interaction situation we may consider for illustrative purposes is described by Mayhew and Gray (1971, p. 171) as being

... one of the most widespread types of encounter situations occurring in human social interaction. Task-oriented groups that require decision-making as a prerequisite to task accomplishment provide a general case which is of considerable importance both for understanding small groups in general and the internal dynamics of organizations in particular. These situations are characterized by an interdependency among group members such that suggestions for the direction of task activity must be made and either accepted or rejected; otherwise, activity on the task cannot proceed. In such situations, two or more individuals operate on the task by making suggestions for courses of action (concrete proposals for alternatives in task activity) and by deciding on the course of action (accepting some proposals while rejecting others). An individual who makes a suggestion is attempting to direct task activity. If his suggestion is accepted, he dominates the interaction (or other persons) by guiding the course of group activity.

This suits our purposes and can actually be extended to "primary" or "expressive" groups as well. George can suggest that the group go on a picnic. If, in response to his suggestion, the group actually does go on a picnic, then George may be considered to have achieved an instance of effective control over the activity of the group as a whole. Clearly, the sorts of directive acts considered by Mayhew and Gray may range from imperative commands issued by a formal superior to suggestions offered by peers, so that we do not unduly restrict the scope of our inquiry by adopting that view.

It is therefore possible to examine a wide variety of interaction situations, identifying, in sequential order, instances of effective control over the action of the group, that is, instances of system control.

#### RANDOM INTERACTION

For an arbitrary point in time  $t$  and actors  $i$  and  $j$ , we define

$$P(i_t | j_{t-1}) = 1/N, \quad (2)$$

where  $i, j = 1, 2, 3, \dots, N$ ;  $i_t$  = actor  $i$ 's being associated with the interactive event which defines time  $t$ ;  $j_{t-1}$  = actor  $j$ 's being associated with

<sup>18</sup> Admittedly, the distinction is somewhat arbitrary. Kinship groups in traditional societies tend to be both task oriented and expressive (Stephens 1963; Goode 1970).

the interactive event which defines time  $t - 1$ ; and  $P$  = the probability operator. The definition given in equation (2) specifies a model of random interaction with respect to the transmission or receipt of communication or for any other event defining interaction sequences.<sup>14</sup> If all interactive events are randomly distributed in time, it follows that any subset of events we choose to examine is also randomly distributed in time. Accordingly, as we wish to identify actor sequences defined by the event that an actor has achieved an instance of effective control over the group as a whole, it logically follows from equation (2) that such events are also randomly distributed in time according to the equation

$$P(i_t \cdot j_{t-1}) = 1/N, \quad (3)$$

where  $i, j = 1, 2, 3, \dots, N$ ;  $i_t$  = actor  $i$ 's having achieved an instance of effective control over the action of the group as a whole at time  $t$ ; and  $j_{t-1}$  = actor  $j$ 's having achieved an instance of effective control over the action of the group as a whole at time  $t - 1$ .

#### POLARIZATION OF POWER

As a consequence of equation (3), our  $K$  measure, defined in equation (1), is calculated for power events over sequence lengths and numbers of actors to be specified. For each sequence length and group size, we may derive an expected value of  $K$ ,  $E(K)$ , which measures the degree of inequality in power structure that occurs by chance alone:

$$E(K) = \sum_{i=1}^S P(s_i) K_i, \quad (4)$$

where  $i = 1, 2, 3, \dots, S$ ;  $s_i$  = the  $i$ th sequence; and  $K_i$  = the value of  $K$  associated with the  $i$ th sequence. For example, with sequence length three in a group of size three, the sequences AAA, BBB, and CCC each have  $K = 1$ ; the sequences ABB, ACC, BAA, AAB, AAC, BBA, BBC, CCA, CCB, ABA, ACA, BAB, BCB, CAC, CBC, BCC, CAA, and CBB each have  $K = 2/3$ ; while the sequences ABC, ACB, BAC, BCA, CAB, and CBA each have  $K = 0$ , yielding an expected value of  $E(K) = 5/9$ .

For sequence length  $L$  and group size  $N$ , table 1 shows  $E(K)$  calculated for integer values in the range  $2 \leq L \leq 9$  and  $2 \leq N \leq 21$ . These results indicate two distinct trends. Power tends to equalize as a function of sequence length and to polarize as a function of group size.<sup>15</sup> However, we

<sup>14</sup> Eq. (2) specifies a discrete-state, discrete-time, zero-order Markov chain with stationary transition probabilities and a doubly stochastic matrix (see Markov 1912). These same properties carry over to eq. (3).

<sup>15</sup> When examining expected values of a random variable, it is important to consider whether this or some alternative value is more appropriate to represent the underlying distribution. In the present case, the median value of  $K$  behaves in exactly the same

TABLE 1

EXPECTED CONCENTRATION OF POWER,  $E(K)$ , UNDER CONDITIONS OF RANDOM INTERACTION FOR SELECTED VALUES OF SIZE AND SEQUENCE LENGTH

GROUP SIZE	SEQUENCE LENGTH							
	2	3	4	5	6	7	8	9
2	.500	.500	.375	.375	.313	.313	.273	.273
3	.667	.556	.444	.432	.395	.366	.343	.324
4	.750	.625	.547	.492	.451	.419	.409	.371
5	.800	.680	.600	.542	.499	.464	.435	.412
6	.833	.728	.644	.585	.539	.503	.473	.447
7	.857	.755	.679	.621	.575	.537	.506	.481
8	.875	.781	.709	.652	.605	.568	.535	.508
9	.889	.802	.734	.678	.633	.595	.562	.534
10	.900	.820	.755	.701	.657	.619	.586	.557
11	.909	.835	.773	.721	.678	.640	.607	.581
12	.917	.847	.789	.739	.696	.660	.619	.599
13	.923	.859	.802	.755	.713	.677	.645	.617
14	.929	.867	.814	.769	.728	.693	.662	.634
15	.933	.876	.825	.781	.742	.708	.678	.649
16	.938	.883	.835	.792	.754	.721	.691	.663
17	.941	.889	.843	.802	.766	.733	.703	.676
18	.944	.892	.851	.812	.776	.744	.715	.688
19	.947	.900	.858	.820	.785	.754	.726	.699
20	.950	.905	.864	.828	.794	.764	.736	.710
21	.952	.909	.870	.834	.802	.773	.745	.720

are not confined to the illustrations in table 1; both trends hold across the full ranges of their respective variables.

In the case of the trend toward equality with increasing sequence length,  $E(K)$  is constrained to equal 1 for the vacuous instance of sequence length one (i.e., for  $L = 1$ ). And, since the definition in equation (3) yields equality of participation among actors for large values of  $L$ , it will be apparent from the structure of equation (1) that  $E(K)$  is forced to approach 0 as sequence length increases. Thus, it is necessarily the case that, for a given size of group:

*Theorem 1: The expected concentration of power is a decreasing function of interaction sequence length.*

This trend toward equality in the relative frequency of elements in long sequences is well known (see Feller 1968) and need not concern us further. However, it is appropriate to note that the very knowledge of this trend toward equality within groups has probably prevented students of human interaction from fully realizing the profound effects of group size

fashion as  $E(K)$ , so that no misrepresentation occurs in spite of the fact that underlying distributions of  $K$  are not always symmetric about the mean. Copies of the underlying distributions for the values presented in table 1 and subsequent tables are available upon request from the authors.

evident in comparisons across groups, the primary subject of the present inquiry.

For any size of group or interaction sequence length, the  $K$  measure can take on its maximum value of 1. However, the minimum value of  $K$ , symbolized  $K_{\min}$ , is constrained by both  $N$  and  $L$ . For  $N > 1$  and  $L > 1$ , with  $L \leq N$ , we have

$$K_{\min} = \frac{N - L}{N - 1}, \quad (5)$$

so that, for  $L$  constant with  $N$  increasing,

$$\lim_{N \rightarrow \infty} K_{\min} = 1. \quad (6)$$

Since  $E(K)$  must fall between its maximum and minimum values, these considerations require that, for a given length of interaction sequence:

*Theorem 2: The expected concentration of power is an increasing function of group size.*

*Theorem 3: As group size increases without bound, the expected concentration of power approaches its maximum.*

In other words, by chance alone, power will be increasingly concentrated as size increases and, in the higher ranges of size, will tend toward maximum polarization. The broad implications of these theorems are illustrated in figure 1, which graphs  $E(K)$  as a function of size for Simon's value of sequence length five.

These results are significant, but it is their consequences for oligarchic structure which occupy our attention here.

#### THE LAW OF OLIGARCHY

At an arbitrary point in time, let  $n$  be the number of actors who have achieved instances of system control. We may then define the relative size of the controlling component as  $R = n/N$ . Under the condition that power events are randomly distributed in time as specified in equation (3), we may calculate an expected relative size of the controlling component,  $E(R)$ , the proportion of actors who will have participated in power events by chance alone:

$$E(R) = \sum_{i=1}^S P(s_i) R_i, \quad (7)$$

where  $i = 1, 2, 3, \dots, S$ ;  $s_i$  = the  $i$ th sequence;  $R_i$  = the value of  $R$  associated with the  $i$ th sequence; and  $P$  = the probability operator.

Mosca's proposition on the relation between the relative size of a ruling elite and system size was restated by Mayhew: "*the relative size of a ruling elite is a decreasing function of the size of the system it governs*"

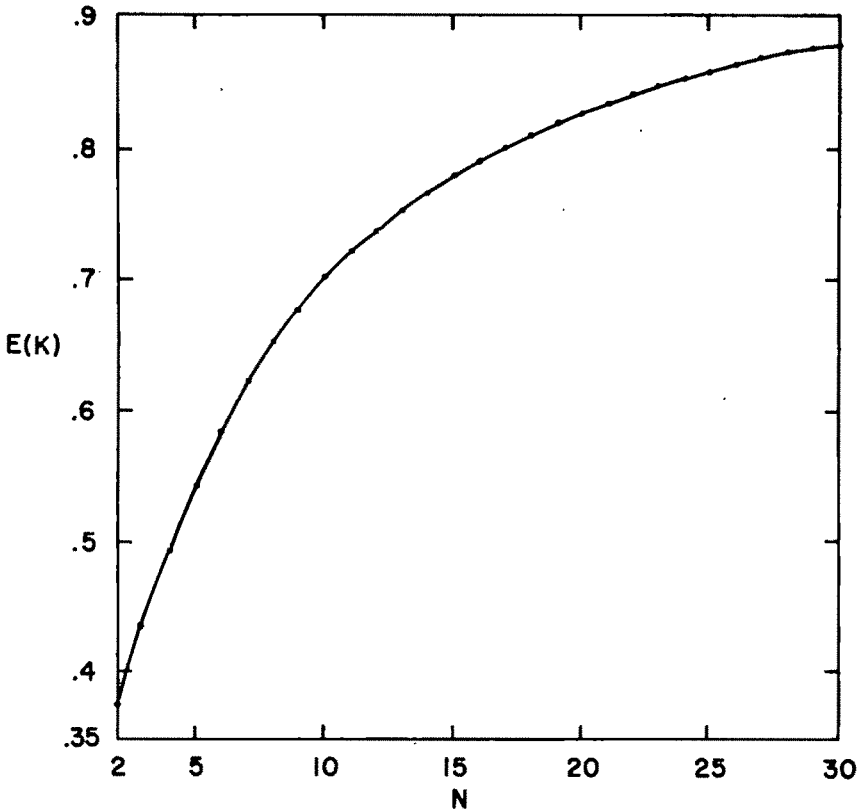


FIG. 1.—Expected concentration of power,  $E(K)$ , as a function of size,  $N$ , for sequence length five.

(1973, p. 468; italics in original). This relation is generated by our conditions of random interaction, but it holds without the restriction that the controlling component constitute an elite, that is, a minority of the system's population. Rather, it holds for any value of  $E(R)$ . Table 2 shows  $E(R)$  calculated for selected values of sequence length and group size. Figure 2 graphs  $E(R)$  as a function of size in the range  $2 \leq N \leq 30$  for Simon's value of sequence length five. As these results clearly show, not only does Mosca's proposition hold for the controlling component, it also generates minority rule at two tipping points of size. The critical values of size beyond which Michels's law holds, first probabilistically and then deterministically, are shown in table 3 for the size and sequence ranges illustrated in table 2. It will be apparent that the difference between the two types of tipping points is an increasing function of size. However, regardless of how great this difference may become, it is clear that, under conditions of random interaction, Michels's law holds far below the size range he specified.

TABLE 2  
 EXPECTED RELATIVE SIZE OF THE CONTROLLING COMPONENT,  $E(R)$ , UNDER  
 CONDITIONS OF RANDOM INTERACTION FOR SELECTED VALUES OF  
 SEQUENCE LENGTH AND GROUP SIZE

GROUP SIZE	SEQUENCE LENGTH								
	2	3	4	5	6	7	8	9	10
2	.750	.875	.938	.969	.984	.992	.996	.998	.999
3	.556	.704	.802	.868	.912	.941	.961	.974	.983
4	.438	.578	.684	.763	.822	.867	.910	.925	.944
5	.360	.488	.590	.672	.738	.790	.832	.866	.893
6	.306	.421	.518	.598	.665	.721	.767	.806	.838
7	.265	.370	.460	.537	.603	.660	.709	.750	.786
8	.234	.330	.414	.487	.551	.607	.656	.699	.737
9	.210	.298	.376	.445	.507	.561	.610	.653	.692
10	.190	.271	.344	.410	.469	.522	.570	.613	.651
11	.174	.249	.317	.379	.436	.487	.533	.576	.614
12	.160	.230	.294	.353	.407	.456	.501	.543	.581
13	.148	.213	.274	.330	.381	.429	.473	.513	.551
14	.138	.200	.257	.310	.359	.405	.447	.487	.523
15	.129	.187	.241	.292	.339	.383	.424	.463	.498
16	.121	.176	.228	.276	.321	.363	.405	.441	.482
17	.114	.166	.215	.261	.305	.346	.384	.421	.455
18	.108	.158	.204	.249	.290	.330	.367	.402	.435
19	.102	.150	.194	.237	.277	.315	.351	.385	.418
20	.098	.143	.185	.226	.265	.302	.337	.370	.401
21	.093	.136	.177	.216	.254	.289	.323	.355	.386

That is, the law of oligarchy will hold under conditions of focused face-to-face interaction by chance alone.

Again, the results presented in table 2 and illustrated in figure 2 hold beyond the size and sequence ranges shown. If  $R_{\max}$  is the maximum value of  $R$ , it is clear from the definition of a ruling elite as a minority of the system's population that oligarchy will occur with certainty in any instance where  $R_{\max} < 1/2$ . For a given value of sequence length,  $L$ , and beyond the point where  $N = L$ , oligarchy will occur with certainty in the size range  $(2L + 1) \leq N \leq \infty$ . Furthermore, for  $L$  constant in the range of  $N$  so specified,

$$R_{\max} = L/N; \quad (8)$$

under these circumstances, since  $L$  is a finite constant, it is necessarily the case that the rate of change in  $R_{\max}$  with respect to  $N$  is negative:

$$\frac{dR_{\max}}{dN} < 0. \quad (9)$$

Since  $E(R)$  must fall between the maximum and minimum values of  $R$ , it is therefore determined that, for a given sequence length:

Theorem 4: *The expected relative size of the controlling component is a decreasing junction of group size.*

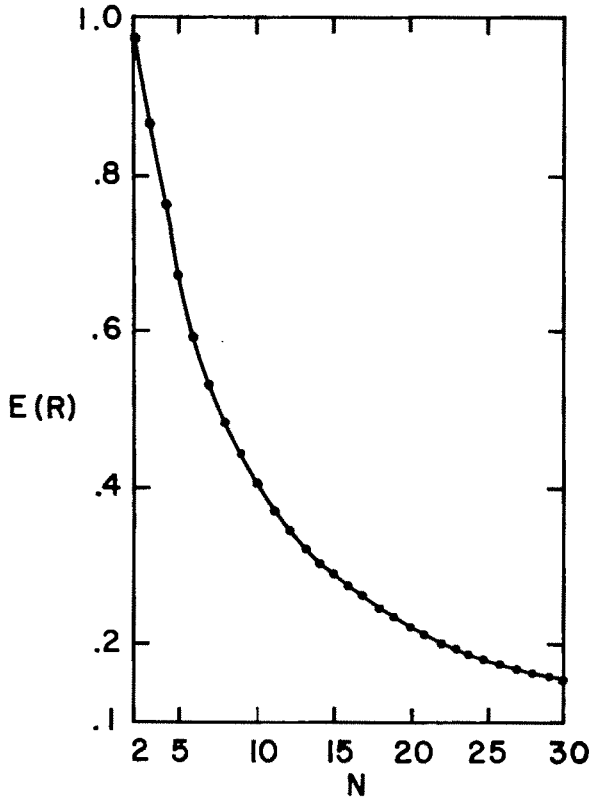


FIG. 2.—Expected relative size of the controlling component,  $E(R)$ , as a function of size,  $N$ , for sequence length five.

Since a ruling elite is a special case of controlling components in general, its behavior will conform to this more general theorem. Under conditions of focused interaction, Mosca's proposition is generalized to all controlling components and holds across the full range of size by chance alone.

#### INSIDE THE CONTROLLING COMPONENT

Shifting attention to the activity of the controlling component itself, we discover some striking consequences of random interaction. Since, by the very definition of random interaction, actors proceed independently of one another, "conspiracy" theories of elite formation are not required to account for the occurrence of oligarchy in conditions of focused, face-to-face interaction. Oligarchic structures will develop, as we have seen, in the absence of concerted efforts by coalitions.

An equally important result is a trend toward equality among the members of the controlling component as a function of increasing size. For an



TABLE 3

SIZE TIPPING POINTS FOR THE LAW OF OLIGARCHY UNDER CONDITIONS OF  
RANDOM INTERACTION, BY INTERACTION SEQUENCE LENGTH

SEQUENCE LENGTH	CRITICAL SIZE BEYOND WHICH THE LAW HOLDS:	
	Probabilistically	Deterministically
2 .....	3	4
3 .....	4	6
4 .....	6	8
5 .....	7	10
6 .....	9	12
7 .....	10	14
8 .....	12	16
9 .....	13	18
10 .....	14	20

arbitrary point in time, let  $n$  be the number of actors in the controlling component. Given the condition of random power events specified in equation (3), we may define an expected absolute size of the controlling component,  $E(n)$ , the number of actors who will have participated in power events by chance alone:

$$E(n) = \sum_{i=1}^S P(s_i) n_i, \quad (10)$$

where  $i = 1, 2, 3, \dots, S$ ;  $s_i$  = the  $i$ th sequence;  $n_i$  = the value of  $n$  associated with the  $i$ th sequence; and  $P$  = the probability operator. Our calculations of  $E(n)$  for group sizes and sequence lengths in the ranges used in table 2 show that, at any given sequence length,  $E(n)$  is an increasing function of size. Due to considerations of space, we illustrate this relation only in figure 3, which graphs  $E(n)$  as a function of size in the range  $2 \leq N \leq 30$  for Simon's value of sequence length five. It is clear from this illustration that participation within the controlling component itself broadens as a function of size by chance alone.

For a given value of sequence length,  $L$ , the maximum value of  $n$ , symbolized  $n_{\max}$ , is  $L$  itself. Thus, while  $E(n)$  is an increasing function of size, it will approach  $L$  as an upper bound:

$$\lim_{N \rightarrow \infty} E(n) = L, \quad (11)$$

so that  $n$  is confined to a finite range, while  $N$  expands without bound. This creates the same kind of situation we considered earlier in theorem 1. Just as the number of elements in long sequences tends toward equality, it is also well known that an expanding number of elements randomly placed in a finite sequence length will tend toward equality in the relative frequency of their occurrence (see Feller 1968). As may be seen from equa-

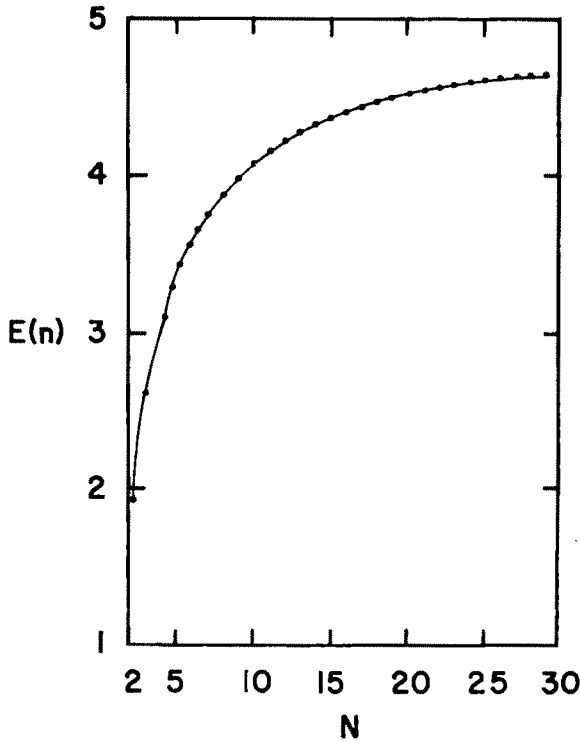


FIG. 3.—Expected size of the controlling component,  $E(n)$ , as a function of group size,  $N$ , for sequence length five.

tion (11),  $E(n)$  can approach its upper bound only if the relative frequency of actors within the controlling component approaches  $1/L$ , a completely flat distribution of actors in the sequence.

Accordingly, if we define a new coefficient of sequence inequality,  $\tilde{K}$ , to be calculated only for actors within the controlling component itself, then its expected value under conditions of random interaction,  $E(\tilde{K})$ ,<sup>16</sup> must fall toward zero with increasing size, as illustrated in figure 4, so that for a given sequence length:

**Theorem 5:** *The expected concentration of power within the controlling component is a decreasing function of group size.*

This result may have important implications for elite studies. At least at the level of face-to-face interaction, it suggests that, as size increases, outside observers would tend to perceive a lowering of power differentials within elite groups and would be led to hypothesize a plurality of elites

<sup>16</sup> The value of  $E(\tilde{K})$  is calculated in exactly the same fashion as that of  $E(K)$  shown in eq. (1), except that inequality is measured only among actors within the controlling component itself.

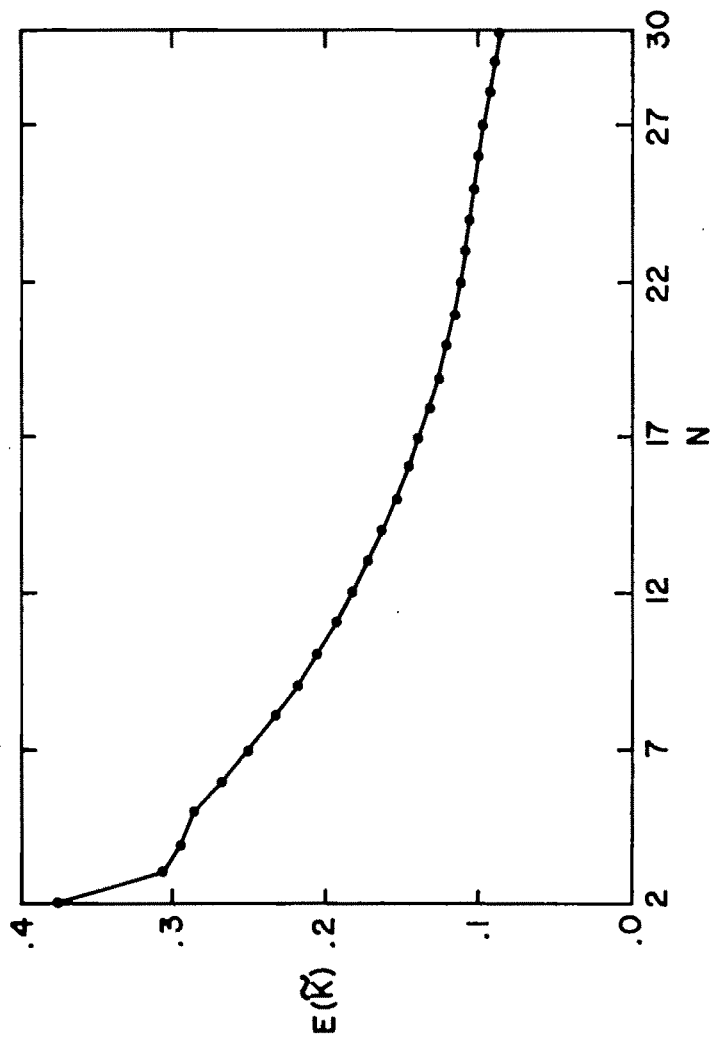


FIG. 4.—Expected concentration of power within the controlling component,  $E(\tilde{K})$ , as a function of size,  $N$ , for sequence length five.

engaged in competition rather than a unified, monolithic cadre which controls the system through a "conspiratorial" format.

#### IMPLICATIONS FOR MICROSTRUCTURE

Even at the level of face-to-face interaction, the structural implications of increasing group size have long been recognized (Simmel 1902*a*, 1902*b*) and continue to play a role in the theoretical repertoire of small-group analysis (see Weik and Penner 1966). Indeed, it has been claimed that large size cannot even develop unless it is accompanied by an increase in structural properties which hold the aggregate together (Simmel 1902*a*, pp. 2-3); that every step in size has its concomitant structural configuration (Simmel 1902*a*, p. 34); and that it is possible to identify ranges of size which contain tipping points for transition from one form to another (Simmel 1902*a*, pp. 32-35).<sup>17</sup>

Insofar as power structure is concerned, empirical studies have shown that inequality is an increasing function of size (summaries appear in Hare 1962; Thomas and Fink 1963; Bales and Borgatta 1965; Kadane and Lewis 1969), and some writers have indicated their awareness that this increased polarization is to some degree constrained by the interaction situation itself (Hare 1962, p. 231; Mayhew et al. 1971, p. 16).

The empirical observation that inequality is an increasing function of group size is predicted, and therefore explained (Hempel and Oppenheim 1948), by our model of random interaction. The empirical generalization is logically deducible from theorem 2. However, this does not mean that the relationship is a statistical artifact; it only means that it might be.

As Mayhew (1973, p. 471) observes, there are at least two alternative interpretations for a set of data which conform to the predictions of a baseline model. One is that, given the conditions of interaction specified, the phenomena do occur as a simple random process and are best understood in that light. The other is that the empirical result is a consequence of a variety of real-world forces playing upon the dependent variable in such a way as to produce conformity to the random expectation. Only when this second possibility has been ruled out can the first be given realistic consideration. That is, only after we have exhausted all other contingencies would we conclude that inequality in power structure, like molecular motion, is best described by a purely random process.

Neither can we assume in advance that we will even be confronted with such a dilemma: empirical results may not correspond to the random ex-

<sup>17</sup> We have singled out Simmel's observations because he deals as much with microlevel as with macrolevel phenomena, the latter having received more attention from other theorists. Simmel's insistence that odd and even sizes have special structural properties accounts for our illustrations of values up to size 21 in tables 1 and 2. These illustrations provide 10 points of comparison for both odd and even values of size.

pectation. If substantive theories about the conditions generating inequality in power structure have merit, then we might observe sharp deflections away from the random baseline. In fact, our model provides a gauge against which it is possible to compare empirical trends and thereby ascertain whether variables other than size affect inequality in power structure, or whether size itself has an effect above and beyond that given by the conditions of random interaction.

Comparisons may be made by constructing a new variable to express the difference between observed and expected values. If  $K_0$  is the observed value of  $K$  in a group of specified size and sequence length, then the variable  $D$ , defined as

$$D = |E(K) - K_0|, \quad (12)$$

might exhibit a functional relationship to size, indicating a departure from the null hypothesis. Taking the signed value of  $D$  permits determination of whether values of  $K_0$  are greater or less than the chance expectation. Even if  $D$  shows no relationship to size, it might still be systematically predicted by the values of some other variable hypothesized by substantive theory, for instance, the division of labor.<sup>18</sup>

Of course, unless comparisons are made in this fashion, it would never be possible to evaluate a substantive theory. For this reason, perhaps the primary contribution of the present paper to the study of power at the level of face-to-face interaction is that of providing the requisite baseline. Hopefully, our model provides not a dilemma but a starting point for building sound theory.

Turning to a related problem area, it might be thought that random interaction provides a sufficient answer to the problem of order in human groups. This is not the case. Rather, the random generation of power structure is but one example of Krippendorff's law of structural generation that "*any communication process, once initiated and maintained, leads to the development of social structure*" (1971, p. 171; italics in original). In this general form, the law provides a partial solution to the Hobbesian problem of order (see Hobbes 1651; Ellis 1971). The solution is not complete because Krippendorff's law does not show how structure stabilizes over time. Our model exhibits the same feature. Power does not have to assume one concrete pattern under conditions of random interaction. That is, stability in concrete interaction patterns is not required to yield a particular value of  $E(K)$  or minority rule. Consequently, although our model

<sup>18</sup> Some candidates for the status of independent variable may themselves be constrained by their definitional relation to size, i.e., may also be generated by a random model based upon size. Structural differentiation is such a variable (Mayhew et al. 1972), and the division of labor is one type of structural differentiation. In this situation, one constructs a variable like  $D$  for both the independent and dependent variable and examines their correlation.

is a special case of Krippendorff's law, it does not solve the Hobbesian problem of order with respect to power structure. While a positive achievement of our model is to show how social structure can be generated from interaction, additional information is required to explain the stabilization of social structure over time.<sup>10</sup>

With these troublesome points now out of the way, we may examine other substantive implications of our model. Perhaps one of the most interesting possibilities has to do with the size distribution of freely forming groups, a topic broached by the original work of James (1951, 1953). His most general finding is that the frequency of occurrence of groups is inversely related to their size.

Numerous interpretations of this phenomenon have been offered (see Cohen 1971). It is probable that the explanation will ultimately involve several factors, each contributing in some degree to the observed outcome. For example, James (1953) suggested the hypothesis that group size is inversely related to the duration of interaction, that is, to the life span of the group. This is plausible if we consider that, especially for primary groups, the object of the group's formation is interaction itself. To the extent that individuals are attracted to groups for participation in interaction (Simmel 1908; Toch 1965), it is clear that the satisfaction derived therefrom would be a decreasing function of size. This is because, in a finite time period, there is less opportunity for each person to participate in interaction. To the degree that the locus of interaction is shifted to smaller and smaller groups, individuals have (1) more time for (2) a greater share in interaction (Castore 1962). Thus, as interaction begins to develop in large groups, individuals more quickly become dissatisfied and more quickly break off interaction for preferential participation in smaller systems. The result is that total time in interaction (life span) for a group is inversely related to its size. For complex societies where time pressure is great (Simmel 1903; Meier 1962; Milgram 1970), this form of explanation seems promising.

However, our model of the relation between group size and power concentration suggests yet another source of pressure upon the locus of interaction. Whereas individuals may derive satisfaction not merely from participation in, but also from effecting a degree of control over, the interaction process, their opportunities for doing so decrease with group size. Relatively few individuals can have any degree of control over the interaction process in large groups, as our model shows, and a preference for

<sup>10</sup> For situations of focused, face-to-face interaction, some recent studies describe and offer explanations for the stabilization of power structure over time (von Broembsen, Mayhew, and Gray 1969; Gray and Mayhew 1970). The latter solution to the problem—a learning paradigm—needs to be tested over a wider range of group sizes before it can be given tentative acceptance.

freedom from such control might also incline individuals to disperse into smaller groups. While we do not think this explanation is sufficient by itself, it may well be one of several conditions which shift the principal locus of interaction down the size scale, creating the inverse size-frequency relation.

Studies of role differentiation among leaders of face-to-face groups (Bales and Slater 1955) have themselves differentiated (Burke 1967; Lewis 1972) and created controversy (Burke 1973; Lewis 1973). We do not wish to enter the debate but only to note that our model of size and randomly generated power structure supplies an expectation relevant to this area of analysis. Specifically, the absolute number of persons in the controlling component will increase with group size, by chance alone, providing an enlarged basis for differentiation in leadership roles. Whether empirical results will bear out this expectation, or departures from it, remains to be seen. Our model shows that increased differentiation among leaders has a greater probability of occurrence in larger groups and provides a baseline for examination of such questions.

Finally, since our model of random interaction assumes independence among actors, it shows that power concentration can occur in the absence of coalitions. While coalitions may be sufficient to concentrate power in some instances (Caplow 1956; Gamson 1961), they clearly are not necessary in large groups. On the other hand, theorem 5 shows that the distribution of participation among the members of the controlling component itself shifts toward relative equality with increasing size, by chance alone, providing less relative influence for any one leader or subset of leaders. Should this chance expectation be empirically realized, it would create an atmosphere conducive to conflict and coalition formation among subsets of leaders. The suggestion is that as size increases, the arena for power struggles will shift inside the controlling component itself. Again, our model provides a baseline for examination of these empirical questions.

#### IMPLICATIONS FOR MACROSTRUCTURE

The fundamental prediction of the classical elite theorists is that all forms of social organization which attain marked degrees of size and complexity will be controlled by a minority: the few will govern the many. Michels's law states that, regardless of the particular form of political organization a system contrives at the outset of its existence, it will drift into oligarchy: rule by a minority. This view has had a profound impact upon leading students of political organization, who have affirmed that it is so (Bryce 1924; Weber 1925; Schumpeter 1943; Selznick 1949; Duverger 1951; Mills 1956; Lasswell 1958). As evidence has accumulated, it has become clear that Mosca and Michels were right (Dodd 1953; Cassinelli 1953;

Ostergaard 1961; Bottomore 1964; Bourricaud 1966; Lipset 1966; Prewitt 1969; Zeigler and Dye 1969; Parry 1969; Pateman 1970; Dahl and Tufte 1973; Field and Higley 1973; Schmidt 1973).<sup>20</sup> In the words of Zeigler and Dye: "In all societies, under all forms of government, the few govern the many. This is true in democracies as well as dictatorships" (1969, p. 167).<sup>21</sup>

Having seen in the present paper that oligarchy will occur by chance alone far below Michels's range of 1,000–10,000, we are confronted with the question of whether our model bears upon the validity of his law at the macrostructural level of organizations and nations. Providing an answer requires that we clearly distinguish between micro- and macrostructure.

By macrostructure we mean structure generated in the relations among the elements of social systems where the elements themselves are not individuals. Interaction (communication) among groups, organizations, communities, etc., is interaction at the macrostructural level. Of course, we might identify several macrolevels. Within an organization, interaction across work-group boundaries would be one level, but since departments within organizations are comprised of work groups, interaction across departmental boundaries is yet another level. Such vertical telescoping can continue on up to interaction among organizations, communities, or even societies. By microstructure, on the other hand, we mean structure generated by focused, face-to-face interaction among persons at the level we have been considering in our model.

At the first emergent level of macrostructure, from which all further levels ultimately derive, the system elements are face-to-face groups, with the new structural relations being generated in the interstices between groups (Bates and Bacon 1972). Our model clearly applies to the internal structure of the elements themselves. But the nature of activity in the interstices cannot be assumed to consist of serially ordered interactions. Unless it does, the model does not apply.

Consider a population of interacting groups, say 100 in number. In the interstices between these elements we might periodically observe instances of sequentially ordered communication occurring for clusters of three, five, eight, or even 20 groups. However, in the absence of specific empirical constraints, episodes of serial action among identifiable clusters of groups

<sup>20</sup> This is not to say that all theorists agree that oligarchic control is inevitable in complex systems (e.g., Pateman [1970] does not). While the majority view is that control by elites is built into the structure of complexity, there are many areas of disagreement about other aspects of elite systems (see Pateman 1970; Parry 1969; Dahl and Tufte 1973; Field and Higley 1973).

<sup>21</sup> This observation may occasion surprise due to definitional ambiguities that have blurred the distinction between "participatory" and "representative" democracy and to unfamiliarity with statistics on and legal structures of contemporary political systems. Part 1 of the Appendix discusses these ambiguities in detail.



seem very improbable. If this is so, the conditions specified in our model have little or no bearing upon interstitial interaction and would be likely to apply, if at all, less and less with increasing size of the element aggregate.

The same considerations apply to interstitial interaction among communities, organizations, and nations. Accordingly, it is not possible to claim that macrostructural power relations are governed by an extension of the features employed in our model.<sup>22</sup> Whether or not this is so can be determined only empirically.

Neither do we endorse a specific alternative theoretical format (e.g., Olson 1965; Kasarda 1974). There are many available, and each merits unbiased empirical examination. But we should at least attempt to give a brief overview of the conditions that have been hypothesized to create oligarchy at the macrostructural level, as specified by the classical elite theorists and those of their predecessors who are, in the main, in agreement with them. This overview is given with hopes that it will dispel misunderstandings and provide a firmer basis for future research on the problems of elite formation at the macrostructural level of analysis.

The basic argument is evolutionary. All social systems exist in environments to which they must adapt; otherwise they perish (Duncan 1964). Emphasis is on the social environment, on the political struggle between organizations and societies (Service 1962; Eisenstadt 1963). The originating condition for oligarchy is therefore environmental pressure. This pressure, in the form of political struggle, has tended to yield two highly adaptive system properties. Since the beginning of recorded history, competition has generated increased size and complexity in its surviving contestants (Lenski 1970).<sup>23</sup> Large size is required to process high volumes of activity in dealing with the environment, and increased complexity in the division of labor enhances the efficiency of all system operations, both

<sup>22</sup> Some aspects of human systems discussed in the initial formulation of our model may have direct relevance to macrostructural power relations along quite different routes. For example, Graicunas (1933) preceded both Miller and Simon in estimating an upper limit on human primary memory. He suggested that the human "span of attention" could not exceed six—a strikingly good estimate considering the evidence provided by Miller and Simon. Furthermore, he noted that this span has implications for power structure, because it limits the effective "span of control" in formal organizations. However, Graicunas was not the first to suggest that the range of human memory has implications for power structure. Hitler, in the course of enunciating a list of propaganda principles, observed that "the receptivity of the great masses is limited, their power of forgetting is enormous" (1925, p. 190; translation supplied).

<sup>23</sup> Most social systems are relatively fragile and do not persist under environmental pressure. The vast majority of all societies that have ever existed have vanished (Lenski 1970, p. 91). And they have not merely languished: they have been crushed or absorbed by other societies. While major societal patterns unfold over many generations (e.g., Boserup 1965; Carneiro 1970), collapse tends to be both abrupt and externally induced.

internal and external (Blau 1972). It is important to realize that the environmental conditions which created these properties are not just past history: they are ever-present contingencies. They exist today just as they did in the past, and they are as critical for organizations (Terryberry 1968) as they are for societies (Dahl and Tufte 1973). The bureaucratization of the world is a historical trend (Weber 1925), and it continues apace (Jacoby 1969).<sup>24</sup>

However, as the debris of empires attests, complex systems are not exempt from destruction. Social systems in competition must exhibit additional features if they are to maintain operations under environmental pressure. Specifically, in responding to environmental turbulence, they must display unitary action and short reaction time. These conditions are fundamental; a system incapable of either is destined for the tar pits.<sup>25</sup> And no system can achieve either unless it is able to mobilize for action without undue internal interference. This fact guarantees that large, complex systems will either develop elites or perish.

A relatively small corps of professional leaders—an elite—can more

<sup>24</sup> The perspective developed here harks back to Comte (1851) and Spencer (1876–82), who defined sociology as the study of the structure and evolution of societies. Evolutionary theory persisted in anthropology (White 1949, 1959; Steward 1955; Sahlins and Service 1960) and remains dominant (Flannery 1972; Segraves 1974). Within sociology the theory retained adherents in human ecology (e.g., Hawley 1950) and continues to hold sway (Duncan 1964; Hawley 1971). It is reemerging not only at the level of societal analysis (Eisenstadt 1964; Lenski 1970) but also within the area of formal organizations, in which it is now the primary style of explanation (e.g., Thompson 1967; Blau 1972).

<sup>25</sup> The inability of Gallic and British tribes to exhibit unitary action in the face of Roman expansion contributed to their conquest and incorporation into the empire (Tacitus [ca. 98] 1970, p. 48; cf. Caesar [ca. 51 B.C.] 1917). Sahlins (1961) notes that the ability of the Nuer of the Upper Nile to act in unison and the corresponding inability of the Dinka to do likewise led to the expansion of the former at the expense of the latter. The inability of the kingdom of Judaea to maintain unitary action in the face of military pressure contributed to its destruction and dispersion (Tacitus [ca. 109] 1931, p. 196; cf. Josephus [ca. 77] 1927–28). Internal dissension in Spanish cities during Roman civil wars contributed to their collapse under military pressure (Caesar [n.d.] 1955, p. 374). Delayed reaction resulted in the collapse of the Abyssinian Empire under military pressure (Del Boca 1965). Because these examples deal with crisis conditions, they are apt to be misinterpreted. Crisis is merely a high point of environmental pressure, and what constitutes crisis in one system might not in another (cf. Hamblin 1958). Discussing work organizations, Blumberg (1968, p. 132) notes that crises cannot be handled by “participatory democracy,” but Pateman (1970, p. 66) fails to appreciate his point because she assumes that crises are unusual. Actually, crises are the *raison d’être* for many kinds of organizations, such as hospitals, fire departments, police departments, and armies. The failure to realize that most organizations operate under high-pressure (crisis) conditions misleads many students of democratic theory into adopting rather utopian views (in Mannheim’s [1929] sense of the term). Dahl and Tufte (1973) have recently cautioned that environmental pressure is one of the principal obstacles to the operation of democratic systems at the national level as well. De Tocqueville (1836, pp. 103–4) made the same point long ago.

easily achieve unitary action than can a large mass.<sup>26</sup> And the relatively short communication circuit connecting their small number permits shorter reaction time than does the vast network encompassing the whole system. Directed by an elite, a system can exhibit both of these properties in dealing with its environment (e.g., Okamoto 1970).

In complex systems, elites are created by the division of labor as an extension of the division of labor itself. The operational vicissitudes attendant upon increased division of labor put the system under pressure to coordinate its activity through further division of labor with respect to coordination itself (Blau 1972). If a complex system is to survive in the political struggle with its environment, its direction cannot be left "to a fickle swarm of dilettantes" (Lasswell 1958, p. 59), untrained in the management of complexity under conditions of environmental uncertainty (Thompson 1967).

But Lasswell's "fickle swarm" will be lacking in any case. What Michels called "apathy" is a structurally induced condition due less to disinterest than to the constraints of complexity. As systems grow in size and complexity, the routine matters of policy and implementation cease to concern the majority (Field and Higley 1973, p. 1), not just due to their "apathy" but by virtue of the limitations of the human nervous system. Beyond a certain point, complexity overloads the human nervous system (Wallace 1961; Miller 1962). The majority will not even attempt to interfere in most elite decisions, simply because they do not have time.<sup>27</sup> Accordingly, in complex systems, elites will be able to exhibit unitary action and short reaction time in the absence of undue internal interference.<sup>28</sup>

The preceding sketch, although oversimplified, gives an outline of the conditions that are considered sufficient to generate elites, at least in large,

<sup>26</sup> In Schumpeter's view, the "mass is incapable of action other than a stampede" (1943, p. 283).

<sup>27</sup> Even individual members of the elite who devote full time to such matters cannot deal with all the problems of system operation. The result is that there must be a division of labor within the elite itself. And if the absolute size of and degree of complexity in the elite corps grow beyond a certain point, elites will emerge within elites, creating the multilevel hierarchy characteristic of bureaucracies. The evolution of macrostructure occurs by connecting subgroups through hierarchy (Spencer 1876-82; Simon 1973).

<sup>28</sup> Or will they? If an organized minority can direct the system, cannot another organized minority hamstring their efforts? Competition among elite subgroups can paralyze a system, and often has (Huntington 1968). The conditions under which a plurality of elites will be unified or disunified have recently been examined by Field and Higley (1973) and warrant continued theoretical and empirical attention. These considerations aside, how do we account for the fact that elites attempt to mobilize the system's population for "political participation" if the intervention of the masses in decision making can prevent unitary action and short reaction time? An adequate answer to this question would take us far afield. A brief interpretation of existing evidence is offered in part 2 of the Appendix.

complex systems.<sup>20</sup> It in no way addresses other aspects of elite structure and dynamics which continue to pose current research problems (see Parry 1969). In fact, Zeitlin's (1974) analysis reveals that social scientists know much less about the activity of elites than received opinion would suggest.

## CONCLUSION

Under conditions of focused, face-to-face interaction, human communication proceeds in sequential order. For this reason, even random interaction will generate increasing concentration of power as a function of group size. Constructing a model of this process has permitted us to see that Michels's law of oligarchy and Mosca's proposition on size and ruling elites will hold by chance alone over size ranges much lower than those hypothesized by Michels. In addition, random interaction expands participation within the controlling component itself as a function of size, creating an increased likelihood of equality among leaders.

The implications for microlevel structure are many but problematic. Our model indicates that minority control can occur in the absence of coalitions, offers an added factor to the size-frequency relation for freely forming groups, and creates expectations for differentiation in leadership roles at larger sizes. Whether these relations are merely artifacts of size can only be empirically determined. Investigation of these and related questions is facilitated by our model, which provides the requisite baseline for comparisons.

We have not been able to offer a rationale for extending our model's implications to macrolevel control structures, because sequential order in

<sup>20</sup> From the outline we also see the conditions that permit social systems to operate without elites. Clearly, if a system exists in a placid, munificent environment unruffled by either political or physical contingencies, it will not be under pressure to adopt a specific form of control structure in order to survive. The "iron law" of oligarchy is no more than a metaphor for a probabilistic prediction: if its predictive power is high, this is because the conditions which generate minority control are relatively ubiquitous. Whether additional elite generating conditions would come into play in the absence of environmental pressure is an open question.

Under conditions of *relatively low* ecological pressure (political, if not physical), it seems likely that social systems would articulate their internal structures without regard to the possibility of environmental turbulence. However, when the political environment closes in, as eventually it will (Terryberry 1968), the structure of control will either shift toward elite formation or collapse. One example of this process is found in the rotating election system among the Ait Atta of the Central High Atlas and Moroccan Sahara (Guenoun 1929). Their system of countervailing power guarantees structural paralysis vis-à-vis the larger political environment. It ensures that "leaders" will have no real power beyond mediation functions (Gellner 1969). As long as outside political pressure is low, such factional quagmires may last indefinitely. Even a shift in the larger political milieu need not be fatal. If environmental pressure builds up slowly, the system may exhibit learning and shift its structure to a more adaptive form. But if the political environment closes in rapidly, as it did on the Ait Atta (Spillman 1936), the option of shifting to a new form may not be available.

communication is not necessarily required for interstitial interaction. Rather, there are substantive theories which purport to explain minority control at the macrolevel, and they appear to be well supported by empirical research, although many questions about elite structure and dynamics remain unanswered.

## APPENDIX

### I. PARTICIPATORY AND REPRESENTATIVE DEMOCRACY

Participatory democracy, or full participation, is defined by Pateman (1970, p. 71) as "a process where each individual member of a decision making body has equal power to determine the outcomes of decisions." With regard to this view, Lipset (1966, p. 34) says: "Democracy in the sense of a system of decision-making in which all members or citizens play an active role in the continuous process is inherently impossible. . . . Michels clearly demonstrated the technical impossibility of terminating the structural division between rulers and ruled in a complex society." Lipset's observation is important because it is a retraction of an earlier statement to the contrary (Lipset 1952, p. 47). Lipset and his colleagues (Lipset, Trow, and Coleman 1956) managed to convey the impression that Michels was wrong. However, their study, *Union Democracy*, does not even address the question (cf. Schmidt 1973).

A "representative democracy" is a system in which leaders are elected by all the adult members of the population, with the outcome of elections being determined by simple majority vote. But, as Steffen (1911, p. 59) has observed, this has no bearing upon whether or not an elite exists. The elections merely determine who the "official" elite will be, not whether there will be one. Furthermore, even if we decide to consider the electorate as politically relevant, we would still conclude that most systems are ruled by elites. For example, the United States has never been a democracy in either of the senses considered here. But this has not prevented influential writers from claiming the contrary. According to de Tocqueville (1836, p. 2), in America "the people elect their representatives *directly*, and for the most part *annually*, in order to insure their dependence. The people are therefore the real directing power. In the United States the majority governs in the name of the people, as is the case in all countries in which the people are supreme" (1945, p. 173, italics in original). These statements are false. At any time prior to 1920 half the citizens in the United States could not even vote: women were not enfranchised. After 1920, as statistics summarized by McCarthy and Zald (1973, p. 5) show, the deciding vote in national elections has always been cast by a minority of the citizens legally qualified to vote. Thus, even if we consider the electorate

politically relevant, the United States has been a *de jure* oligarchy throughout most of its history and a *de facto* oligarchy throughout all of its history.

However, the classical elite theorists rejected the view that the formal trappings of "democracy" alter the structure of power relations. Michels (1915) asserted that "democracy" always transforms into oligarchy. He saw electoral procedures as weapons in the hands of competing elites, who manipulate the masses to gain office (Michels 1935). Neither did the classical elite theorists suggest that struggles among elites stop at mere election fraud (e.g., Whyte 1943). Indirect strategies were seen as even more important. In commenting upon the "democratic facade" of the United States, Mosca observes that "it does not prevent elections from being carried on to the tune of clinking dollars" (1939, p. 58). He notes with Jannet (1891) that the outcomes of elections in the United States are financially determined and that powerful economic cartels influence legislatures as well.

Retreating before the law of oligarchy, democratic political theorists attempt to rescue "democracy" by continually redefining it, with the result that it comes to mean "republic" (see Schumpeter 1943, p. 269) or even "faction interaction," as Lipset concludes: "In essence democracy in modern society may be viewed as involving the conflict of organized groups competing for support" (1966, p. 36). Since this does not distinguish the United States from the Soviet Union, or the political process in either from the internecine warfare of Albanian clans (Cozzi 1910), it is difficult to avoid the impression that the elite theorists may well have been right about the propaganda aspects of democratic institutions. Perhaps the strongest evidence favoring this interpretation is the recent demonstration by Jackman (1974) that Lenski's (1966) hypothesis on the relation between political and social democracy is unfounded. If contemporary democratic institutions have no effect on other aspects of society, perhaps they are no more than ceremonial epiphenomena. Pateman (1970, p. 71) concurs.

## II. THE DEMOCRATIC MIRAGE

To distill the observations of the classical elite theorists in one maxim: "*democracy*" is not a form of political organization; it is a form of propaganda. Evidence supporting this interpretation is extensive. As Pateman (1970, p. 73) concludes, the most striking effects of participation in decision making, however trivial the decisions may be, are psychological. People believe that they have a degree of control over the system, whether they do or not. If individuals believe that they have an influence on decision making, they are more likely to accept the decisions that are made. They come to believe in their own political efficacy and to *accept the system*. Pateman (1970, pp. 45-66) summarizes extensive research from both organizational and national studies which confirm that this is so (cf. French, Israel, and

Aas 1960; Verba 1961; Almond and Verba 1963; Blumberg 1968). Thus it is possible, by encouraging mass participation in trivial decisions, to gain acceptance of the existing order without sacrificing elite control of significant decisions. Mulder (1971) provides insight into the process through which management in organizations can introduce "participatory democracy" even with respect to more important decisions and still retain control over the outcomes of decisions. The basic premise in Mulder's argument has received strong empirical support (Richardson et al. 1973). Pateman (1970, pp. 64-65) notes that the "participation" ploy has been so successful that it is likely to be increasingly adopted as a management strategy in the future.

The political significance of these findings should not be underestimated. Lazarsfeld and Merton (1948) concluded that propaganda is most successful when it is used to support the existing order. Huntington (1968, p. 275) observes that "perhaps the most important and obvious but also most neglected fact about successful great revolutions is that they do not occur in democratic political systems." De Tocqueville (1836, p. 39) appears to have perceived the basis for this insight: "perhaps the most powerful of the causes that tend to mitigate the violence of political associations . . . is universal suffrage" (1945, p. 197). If people believe that they rule, they can hardly revolt, for they could only revolt against themselves. This may provide a partial answer to Goode's (1972) question about the relative decline of violence and the concomitant increase in governmental control in complex societies. A more complete answer would undoubtedly include Simmel's (1908) observation that complexity mitigates extreme conflict through the interdependence it creates.

Huntington's (1968) theme that the extension of political participation increases the degree of governmental control in both totalitarian and "democratic" polities is consistent with the view that democracy is more adequately understood as a propaganda strategy than as an operative form of political organization. And the conception of democracy as propaganda squares well with Jackman's (1974) finding that "political democracy" has no effect on other aspects of the social order.

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# **The Functions and Reproduction of Migrant Labor: Comparative Material from Southern Africa and the United States<sup>1</sup>**

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For a capitalist economy to function, its labor force must be maintained; that is, workers must receive a historically determined minimal day-to-day subsistence. It must also be renewed; that is, vacancies must be filled. A system of migrant labor is characterized by the institutional differentiation and physical separation of the processes of renewal and maintenance. Accordingly, migrant labor entails a dual dependence upon employment in one place and an alternate economy and/or state in another. In addition, the separation of migrant workers from their families is implied. It is enforced through specific legal and political mechanisms which regulate geographical mobility and impose restrictions on the occupational mobility of migrants. These mechanisms in turn are made possible by the migrant workers' powerlessness in the place of employment, in the labor market, and under the legal and political systems where they are employed. One consequence of a system of migrant labor is the externalization, to an alternate economy and/or state, of certain costs of labor-force renewal—costs normally borne by the employer and/or state of employment. This framework is developed and applied to migrant farm workers in California and migrant mine workers in South Africa. The differences between the two systems are highlighted and analyzed in terms of the broader features of the respective social structures. Finally, the implications of the theoretical scheme are discussed and extended to an interpretation of race relations.

Traditionally, studies of labor migration have adopted the perspective of the individual migrant. This has involved the examination of two questions: the reasons for migration and its consequences at the level of the individual or group. In answer to the first question, it has generally been assumed that individuals respond to the "push" and "pull" factors associated with the market. With regard to the second question, attention is directed to prob-

<sup>1</sup> This paper originated with the ideas and work of Jaap van Velsen and was later stimulated by Harold Wolpe. I owe the greatest debt to Adam Przeworski, who read and criticized successive drafts and helped in placing the analysis in a broad theoretical framework. I am also grateful to a number of persons who were kind enough to comment on various versions of the paper, forcing me to reformulate many aspects of the problem: Manuel Castells, William K. Cummings, Terence Halliday, Ida Susser, William J. Wilson, and two anonymous referees.

lems of adaptation, assimilation, and acculturation of the newly arrived migrant. In each instance, individuals are conceptualized as actively responding to environmental forces, maximizing their individual interests, and in this sense exercising control over their own destiny. Although these formulations are important, they are too restricted to clarify the functioning of a *system* of migrant labor either in its broader social, political, and economic contexts or where the flow of labor is regulated to a greater or lesser extent to suit dominant political and economic interests.

The shortcomings of conventional analysis are particularly apparent with regard to migrant laborers in the fields of California or the gold mines of South Africa, where specific political mechanisms regulate their movement between industry and "home." Accordingly, what is of interest is not how migrants adapt to their new environment but how structural, particularly political and legal, constraints make permanent "integration" impossible. The issues are not ones of assimilation and acculturation but of enforced segregation through such "total" institutions as the compound and labor camp and the corresponding persistence of race and ethnic differentiation. The individual cannot be conceived of as a rational actor maximizing interests under market forces. Instead, the flow of labor is directed by supramarket institutions beyond the control of an individual or even a group of migrants. For these reasons, the analysis of such systems of migrant labor requires a different perspective: one focusing on the nature of external coercive institutions and their mode of organization. The development of such an approach is the purpose of this paper.

### THEORETICAL INTRODUCTION

For an economy to function, a labor force has to be maintained and renewed. In other words, workers have to subsist from day to day, and vacancies created by their departure from or by the expansion of the labor force must be filled by new recruits. Under capitalism the distinction between these two elements of the reproduction of labor power is normally concealed.<sup>2</sup> The same institutions simultaneously perform both renewal and

<sup>2</sup> The concept of "reproduction" is central to this paper and will be used in two empirical contexts, those of labor power and systems of migrant labor. In each context, reproduction presupposes the existence of what is being reproduced. It expresses a preoccupation with continuity, persistence, and above all repetition. The concept is motivated by the view "that nothing which exists is natural (in the nonhabitual sense of the word), but rather exists because of the existence of certain conditions, whose disappearance cannot remain without consequences" (Gramsci 1971, p. 158). Social relations, labor power, systems of migrant labor, etc., do not merely exist but have to be produced again and again—that is, *reproduced*. Analysis of the conditions of reproduction entails examining how different levels or regions of the social structure interconnect so as to ensure the repetition of the particular process of "producing" labor power,

maintenance functions. For example, the distinction between the rearing of children and the day-to-day sustenance of the productive worker is not normally inscribed in the organization of the family. On the contrary, domestic work simultaneously provides for both maintenance and renewal requirements of the labor force. Equally, in the provision of welfare, housing, and urban amenities such as transportation, the state simultaneously performs both maintenance and renewal. Only in a few institutions such as the school are renewal processes clearly separated from those of maintenance. As indicated in the distribution of family welfare benefits, even the industrial enterprise tends to treat the day-to-day maintenance of the labor force and the creation of a future one as though they constituted a single process.

By contrast, the organization of migrant labor not only makes the distinction apparent but is even defined by the separation of the processes of maintenance from those of renewal. How does this separation manifest itself? First, the two processes take place in geographically separate locations. Second, at the level of the institutions of reproduction, the institutions of maintenance may be very different from those of renewal, or a single institution may continue to engage in both processes. To take the family as an example of the latter possibility, geographical separation of the two processes is reflected in a corresponding division of labor and internal differentiation of the family unit. Thus, for Mexican migrants, processes of renewal are organized under the Mexican state in the Mexican economy and those of maintenance in the United States. Yet the kinship group remains a single cohesive unit despite its internal differentiation. What is important for this paper is that the activities of maintenance and renewal are separated.

They remain, however, indissolubly interdependent, as reflected in the oscillatory movement of migrants between work and home. Under capitalism the binding of production and reproduction is achieved through economic necessity: for the laboring population, work is necessary for survival; under feudalism the unification is achieved through coercive regulation. A system of migrant labor contains elements of each. On the one hand, renewal processes are dependent on income left over from maintenance, which is remitted home by the productive worker. On the other hand, productive workers require continued support from their families engaged in renewal

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systems of migrant labor, etc. While what is being reproduced has invariant characteristics, the conditions for its reproduction may have to be modified. Thus, as we shall see, while the definition of a system of migrant labor entails the specification of a set of invariant features, the conditions of reproduction will vary between societies and over time. In addition to Marx's treatment in *Capital*, an important discussion of reproduction is to be found in Balibar (1970, chap. 3).

at home, because they have no permanent legal or political status at the place of work. In other words, the state<sup>3</sup> organizes the dependence of the productive worker on the reproductive worker, while the economy organizes the dependence of the reproductive worker on the productive worker. The interdependence establishes the cohesion of the family. Similar ties link the state supplying labor and the state employing labor: the former requires revenue and employment for its population, the latter requires labor at low wage rates.

In the following sections, I explore two implications of the separation of the maintenance and renewal processes. The first concerns the functions of migrant labor. Under such a system costs of renewal, normally borne by the employing state and economy, are to a considerable degree borne by another economy or another state or a combination of the two. Furthermore, the employer of migrant labor is neither responsible politically nor accountable financially to the external political and economic systems. In other words, a proportion of the costs of renewal is externalized to an alternate economy and/or state. The second implication concerns the conditions for the reproduction of a system of migrant labor—namely, the reproduction of its defining characteristic, the separation of maintenance and renewal processes.

The two systems of migrant labor were chosen not for their similarity but for their contrast, with a view to highlighting the invariant characteristics they share. Furthermore, I hope to show that an analysis founded on examination of the conditions for the reproduction of the invariant characteristics leads to a better understanding of the peculiarities of the individual system; that is, differences arise out of the interplay between the under-

<sup>3</sup> The notion of the capitalist state used here derives from Poulantzas (1973) and Przeworski. For our purposes, the capitalist state is constituted of formal institutions, each with an internal coherence and relative autonomy, which in combination possess the monopoly of universally binding rules to which force may be applied. Throughout the paper I associate the state with the organization of the reproduction of systems of migrant labor, thus unavoidably conferring upon it a monolithic quality it does not in practice possess. Although this is no place to elaborate on a theory of the capitalist state, two points are in order. First, I do not regard it as necessary that the capitalist state be an "instrument" of the economically dominant class. Though there are instances, particularly in the case of migrant labor in California, in which the state does indeed appear to be an instrument of the economically dominant class, this is not *implied* by our definition of the capitalist state. On the contrary, it is the relative autonomy of the capitalist state which is central to its operation. Indeed, the discussion of migrant labor in South Africa mentions significant occasions when the state clearly acted in opposition to the interests of the mine owners. Therefore, I propose a tentative formulation of the function of the capitalist state: in normal times it preserves the cohesion of a society as a whole. Second, it is a state in which a society is divided into classes; therefore, it reflects to some extent the interests and struggles of *all* classes, if only to preserve the stability of the whole.



lying structure and the particular political and economic context. More specifically, I argue that observed patterns of relations among races, classes, and fractions of classes emerge out of an interaction between the organization of the separation of maintenance from renewal processes and certain features of the particular industry and the particular state.

#### MINE WORKERS IN SOUTH AFRICA

The South African gold mines, first commercially exploited in the last decade of the 19th century, have relied on two types of migrant labor. On the one hand, unskilled tasks have been and continue to be performed by African labor recruited from the rural hinterland and surrounding territories (see Wilson 1972a, p. 70, for exact distribution by geographic location). Once cajoled into selling their labor power by expropriation of land, imposition of taxation, and similar nonmarket inducements, African workers became attracted to wage employment as a way of making up or supplementing their means of subsistence (Horwitz 1967, chap. 2). On the other hand, white labor, initially recruited from Britain, was employed in skilled and supervisory positions. Just as craft unions at that time had a powerful monopoly of a sector of the British labor market, so the white workers of South Africa, in part influenced by their experience at home, formed a union to protect their positions from competition from black labor (Simons and Simons 1969, chap. 3). Although mine owners wished to advance blacks into more skilled occupations, their efforts were obstructed as early as 1893 by the legal enforcement of the color bar which reserved a range of jobs for white workers. As the mining industry expanded, the color bar became an entrenched feature of the occupational structure, barring blacks from advancement into skilled and even semiskilled jobs and stipulating an upper limit to the employment ratio of blacks to whites (Wilson 1972a, pp. 110–19). On a number of occasions, most notably during the Rand Revolt of 1922, management attempted to breach the color bar, but the power and determination of white workers to protect their monopolistic position proved insurmountable.

Once the color bar was accepted as irrevocable, management sought to offset the costly protection of white labor by externalizing the costs of renewal of a black labor force. This process was made possible by the reproduction of the system of migrant labor. Initially a response to the insecurity of employment in industry and the lack of provision for permanent settlement near the place of work under colonial rule, migrant labor continues to be an institutionalized feature of the mining industry. Just how the system has been perpetuated and how certain labor costs are reduced under it will be examined in subsequent sections.

## The Economic Functions of Migrant Labor

Earlier I was careful to define a system of migrant labor in institutional terms. Others have defined it in economic terms, and I now propose to consider some of the difficulties of these formulations. Wolpe (1972), Castells (1975), and, with some qualifications, Castles and Kosack (1973, chap. 9) all assert that a system of migrant labor lowers the cost of the reproduction of labor power; Wolpe goes so far as to claim that it constitutes a system of cheap labor power. However, the assessment of the costs and benefits of migrant labor and of its effects on the rate of profit is far more complicated than even Castles and Kosack (pp. 374–75, 422) indicate and requires considerably more substantiation than any of the writers provide.<sup>4</sup> First, they fail to distinguish among the institutions whose costs are reduced, preeminently between the state and the employer—though, of course, the two sets of costs are not unrelated. In other words, they do not address the question, Cheap for whom? (Castles and Kosack, however, do discuss the implications for domestic labor.) Second, they do not adequately examine which aspects of the costs of the reproduction of labor power—maintenance or renewal—are reduced. Third, while it is true that migrant labor does lead to some economic savings for employer and state, the reproduction of a *system* of migrant labor in itself represents a cost which may outweigh the economic benefits based on the externalization of renewal. None of these writers considers the costs (political as well as economic) of the reproduction of a system of migrant labor.

What evidence is there to suggest that black mine workers receive little more than the costs of maintaining themselves from day to day? Virtually all black workers in the gold mines migrate between the reserves<sup>5</sup> or surrounding black countries and their places of employment. In other industries, migrant labor is less prevalent, and Wilson writes, “The mining industry would have to double the wages if they hoped to compete with the manufacturing sector for labor” (1972a, p. 153). His calculations appear to be based on the average earnings of Africans in different sectors, and while they do not take into consideration skill differential and pay-

<sup>4</sup> When migrant labor is referred to as “cheap,” the issue is not whether a single migrant worker costs less to hire than a single domestic worker in any specific context, although that meaning is frequently conferred upon the term. Like the other writers cited here, I am considering the cost of the *system* of migrant labor. I am concerned with an institutional rather than a marginal economic analysis.

<sup>5</sup> The reserves are those geographical areas where the black three-quarters of the South African population are allowed to acquire permanent domicile and landed property. They represent approximately one-tenth of the area of South Africa. Bantustans are the theoretically self-governing reserves. In practice, however, the degree of self-government is so limited by their poverty that, except concerning some internal matters, Bantustans depend upon the South African government in Pretoria.

ments in kind, these factors alone cannot account for the observed difference. The mining industry pays less by virtue of its access to isolated labor supplies, because some proportion of the costs of renewal is assumed to be borne by a subsistence economy. But it should be noted that people in these subsistence economies (particularly the reserves) may be so poor that they are largely dependent on income remitted by kinsmen working in urban areas. Through a frugal existence in town, savings are made from what are essentially maintenance wages.

While the extraction of produce from a precapitalist mode of production redounds to the benefit of the employer of migrant labor, reliance on an alternate state or its functional equivalent redounds to the benefit of the South African state. Functions normally performed by the state, such as provision of welfare facilities, education, and social security, are transferred to the communal context of the precapitalist economy. The provision of urban amenities is therefore limited to those necessary for the single productive worker.

But pointing to the existence of "excessive exploitation" and the externalization of costs of labor-force renewal is not the same as demonstrating the existence of cheap labor. In one sense all labor is cheap simply because it is exploited.<sup>6</sup> In another sense, it is a more difficult concept to grapple with. *Cheap with respect to what?* It is conceivable, for example, that the reduction in the costs of reproducing labor power through access to a subsistence economy would be outweighed by the latter's replacement by a capital-intensive agricultural economy. (In fact, given the state of soil erosion in the reserves, this is unlikely, but it is the sort of question involved in examining whether a particular system of labor is cheap.) So far, I have highlighted the *economic* benefits for state and capital of a system of migrant labor, but there are also political benefits. A series of political costs are externalized to the reserves, costs associated with the residence of a large, stable black population under a white supremacist state. Indeed, the system of migrant labor is often perceived in political terms.

We cannot, however, ignore the *costs* associated with migrant labor, such as high rates of turnover, recruitment expenses, and the more general set of costs experienced by the state and arising from the political and legal conditions for the reproduction of a system of migrant labor. When all these are introduced, many of them intangible, the balance sheet becomes so complex that the notion of cheap labor, in practice if not in principle, may become impossible to handle.

<sup>6</sup> "Exploitation" refers to the existence of a "surplus value" remaining when the value of a worker's wage and the value of the means of production consumed by a worker are subtracted from the value of a worker's produce. The rate of exploitation is the ratio of this surplus value to the value of the wage.

One way of circumventing the problem is to dispense with the notion of cheap labor altogether. An alternative approach is to adopt the tautological argument that migrant labor exists because it is cheap and it is cheap because it exists. This is not as unenlightening as it might appear, for tautologies are useful if they lead to the formulation of important questions. For example, we may be led to ask what is cheap about migrant labor and thus to generate new insights.

Finally, it may be that "Cheap with respect to what?" is less appropriate than "*Cheap for whom?*" While migrant labor may be cheap for industries that rely extensively on unskilled labor and have facilities for the recruitment of migrant laborers, the smaller industry which uses skilled labor and has little access to isolated labor supplies finds a system of migrant labor more expensive. If industry bears a small minority and the state the majority of the costs of organizing a system of migrant labor, the former may find it cheap compared with other systems of labor, while the latter may find it more expensive than systems relying more on market institutions for the regulation of labor supplies. Yet at the same time, one must not forget that the state does not finance itself but relies on industry to support its activities. Thus the question of whether migrant labor is cheap for a particular industry involves not only an examination of the direct costs experienced by that industry but also secondary costs, such as taxation appropriated by the state. While "*Cheap for whom?*" may appear to simplify the problem, it still remains inordinately complex, and the problems of comparison—that is, Cheap for whom with respect to what and under what conditions?—are still with us.

I have argued elsewhere (Burawoy 1974) that the appearance of migrant labor in South Africa must be sought, not in its specific or general cheapness, but in the historically concrete circumstances of the articulation of different modes of production and the corresponding superstructures. At the level of function, there is nothing necessary about the system of migrant labor. It is not what Castells (1975) refers to as an "organic" part of capitalism at a particular stage in its development. Instead, it is a conjunctural feature which acts as a functional substitute for other modes of organizing labor under capitalism.

### Dependence on a Capitalist Economy

I turn now to examine the conditions for the reproduction of a system of migrant labor. They naturally revolve around the separation of the means of renewal from the means of maintenance of a labor force. Two aspects of the reproduction of this separation can be delineated. First, there is the reproduction of a twin dependency upon the capitalist economy on the one hand, and upon a subsistence economy and/or alternate state on the other.

Second, there is the (coercive) separation of the family from the worker (in such a manner as to preserve their mutual dependence) through a series of legal and political measures and institutions.

What is the basis of a dependence on the capitalist economy, and how is it reproduced? Originally, the imposition of taxes upon the Africans living in the rural areas dislocated them from their subsistence livelihood and required them to seek employment in the emerging extractive industries. This was so, for example, for Malawians who trekked to the South African gold mines and the Zambian copper mines. In South Africa, the movement of blacks to the towns was further compounded by the state's expropriation of land, making subsistence existence increasingly difficult and reliance on an additional source of income increasingly necessary. With regard to Mozambique, Harris (1959) shows how the colonial administration forced able-bodied males into the system of migrant labor by conscription, where necessary. Even where subsistence livelihood could still be eked out, Africans have supplemented it with income from employment in the urban areas. Arrighi (1973) shows how Africans who began to respond to the demand for agricultural produce with the development of Southern Rhodesia at the beginning of this century were priced out of the market through discriminatory subsidies favoring the European farmer. Accordingly, the rewards of remaining in the rural areas and accumulating surplus produce were arranged to be less than those of entering wage employment. In this way, the colonial administration managed to generate a commitment to and, to a certain degree (taxes still had to be paid in cash), a dependence on the capitalist economy. In all these cases, Africans who engaged in productive activities in the towns were able to send home a portion of their income out of which taxes could be paid and on occasion "luxury" items bought. The broadening commitment to the South African wage economy and in particular the gold mines stems largely from the inability of the reserves in South Africa and, to a lesser extent, the rural areas of Malawi and Mozambique to support the reproduction of a labor force.

#### Dependence on a Subsistence Economy

Wages earned by African mine workers on the Rand are calculated on the assumption that they supplement the produce of a subsistence economy (Bettison 1960; Harris 1959). To provide some material basis for such an assumption and to ensure continuing dependence on a subsistence economy, the economy must be capable of providing for some needs. It must be continually recreated in the face of the eroding tendency of capitalism (Lenin 1960, pp. 40-41). It is necessary in this discussion, there-

fore, to examine the impact of an industrial economy upon the subsistence economy in the surrounding rural areas.

4. The rural economy in the South African reserves has been under continual decay, as soil erosion and overpopulation make the extraction of a viable existence there increasingly difficult. The South African government's recognition of this fact and its desire to prevent the further decline of the rural black economy lead Wolpe (1972) to interpret the policy of "separate development" as an endeavor to re-create the subsistence base of the migrant labor force. So far, the actual resources invested in the Bantustans are meager compared with what will be necessary to reverse the trend. One factor in the slowing down of the accumulation of land in the hands of a few Africans in the reserves and the dispossession of the majority has been the government's active policy of reproducing a system of communal land tenure and the corresponding precapitalist relations of production. How much the reserves are able to produce is a matter for  
7. some debate. In any event, the numerous prosecutions under the pass laws suggest that the dependence is more an artifact of the legal and political institutions forcing Africans back into the reserves than a result of a commitment to a viable economy.

The situation in surrounding black territories like Malawi is somewhat different. While the impact of migrant labor in some areas has contributed to the erosion of the subsistence economy, in others it has reinforced that economy. The crucial variable would appear to be the reliance of the subsistence economy on able-bodied males. Where the economy is such that the absence of males does not prevent the cultivation of crops, the earnings remitted by those absent serve to bolster the rural political economy (van Velsen 1961; Harris 1959; Watson 1958). By contrast, those economies relying on male labor for cultivation, as in "slash and burn" techniques, have tended to be adversely affected by the system of migrant labor (Richards 1939).

### The Regulation of Circulation

2. The twin dependency on two modes of production does not reproduce itself without recourse to noneconomic institutions. We have already noted how attachment to the capitalist economy was generated by the intervention of colonial administrations in the subsistence economies and how dependency on the latter is perpetuated by preventing their erosion through supramarket intervention. The thesis I am about to outline is that the twin  
3. dependency can be better seen as a *reflection* of a set of political and legal arrangements designed to separate the means of renewal from those of maintenance and at the same time to ensure a continued connection between the two.

The separation of family from worker is organized through a set of laws restricting urban residence, with few exceptions, to those who are gainfully employed. The enforcement of pass laws externalizes the supplies of unemployed labor and the processes of labor-force renewal to areas where those not gainfully employed are legally permitted to reside—namely, the reserves or Bantustans and the surrounding black territories. Influx control and pass laws also ensure that, on termination of a contract with an employer, a worker returns to the “home” area before being allowed to gain further employment in the urban area. Should a worker become unemployed owing to retirement, physical disability, or simply scarcity of opportunity, he can have no legal residence outside the reserves or wherever his home may be. Such arrangements compel the worker to maintain close ties with the remainder of the family in the reserves or surrounding territories. Equally, these measures lead to the remittance of a proportion of wages earned in town and thereby supply the domestic unit with necessary commodities for the renewal of the labor force. In other words, influx control and pass laws preserve the separation of renewal and maintenance functions, prevent the stabilization of families in the urban areas and the surrender of subsistence existence in the reserves, uphold the continued interdependence of worker and family, and, finally, regulate the circulation of labor between the place of work and “home.”

### Restrictions on Occupational Mobility

Participation in a system of migrant labor has tended to be incompatible with employment in skilled positions (Arrighi 1973, pp. 216–18) for at least two reasons. First, for jobs requiring both training and experience for their effective performance, high rates of labor turnover could be prohibitively costly.<sup>7</sup> Second, entry into the more skilled occupations in any considerable numbers could result in the development of power based on the possession of a relatively scarce resource. We may conclude, therefore, that the preservation of the color bar is not merely a matter of safeguarding the interests of white workers but also represents a major factor in the reproduction of a system of migrant labor.

In this condition, we have the possible seeds of the erosion of a system of migrant labor. With its superior recruitment facilities and extensive use of unskilled labor, the mining industry has successfully adapted itself to the exigencies of a system of migrant labor. However, as manufacturing assumes an increasingly important role in the South African economy, and as the color bar is removed from increasingly higher skill levels, a greater

<sup>7</sup> This need not be so when the turnover arises from fluctuations in the level of employment, e.g., in the case of migrant coke workers in England at the turn of the present century (Hobsbawm 1964, chap. 9).

number of Africans will be engaged in skilled and supervisory positions. This is perhaps the major contradiction between the reproduction of a system of migrant labor and the development of the South African economy.<sup>8</sup>

### Migrant Labor Powerlessness

The reproduction of a system of migrant labor hinges on the inability of the migrants, as individuals or as a group, to influence the institutions that subordinate them to the other fractions of the labor force as well as to the employer. Domination of the migrant labor force takes place in three arenas: the labor market, the industrial organization, and the state.

I shall deal with the state first. Under the capitalist state, the migrant is treated as an alien without rights of citizenship. In the South African colonial superstructure, the differential incorporation of races leaves the subordinate race with no formal power to modify fundamental institutions. The migrant has no significant political rights and only limited legal rights in the urban areas. Only in the Bantustans or reserves can Africans exercise rights of citizenship, and because of their very limited resources such participation is unable to affect their lives materially. Protest by blacks directed at the South African state has been dealt with violently, and the rise of a police state makes combination almost impossible (see, e.g., Simons and Simons 1969; Roux 1964; Kuper 1957).

In cementing the system of migrant labor, the role of ideology is not unimportant. The coincidence of racial characteristics and participation in a system of migrant labor has a number of consequences. All dominant ideologies under capitalism tend to conceal the underlying class structure; if an ideology has a strong component of racial supremacy, class differentiation is masked by the prevailing racial perspectives. This remark applies equally to the consciousness of the dominant and the dominated classes. As a result, the dominant ideology pays little attention to the economic role of migrant labor and the manner in which its exploitation is organized. Behavioral characteristics due to participation in a system of migrant labor are portrayed by the dominant ideology as racial characteristics. Migrant labor is seen as a voluntaristic form of participation in the South African economy, upholding the integrity and indigenous culture of the African people. It is considered the natural and

<sup>8</sup> In this connection it is interesting to note the emergence of the border industry program (Bell 1973). The South African government has tried to promote the movement of capital to labor, rather than the reverse. Industries established near the borders of the reserves enable black laborers to commute to work. Mayer (1971, "Postscript") describes the development of a township in the reserve near East London and the government's attempt to reunify productive worker and family. The Border Industry Program promoted by the Mexican government (Baerresen 1971; Briggs 1973, pp. 44-47) represents a similar movement of American capital to an external labor reservoir.



inevitable form of black labor. It purportedly reflects the strength of tradition pulling the African from the foreign and corrosive urban area to his natural environment and thereby solidifying his so-called tribal allegiances. Instead of there being an inherent conflict between the dominant ideology and the system of migrant labor, the former reinforces and legitimates the coexistence of two structurally different modes of organization of labor distributed according to racial characteristics.

Domination within industry is enforced with the cooperation of the state, as when strike action is suppressed. Though not actually illegal, trade union organization among blacks has been thwarted through "racial discrimination in the law and in labor practices; government obstruction and intimidation; and colour prejudice among white workers" (Hepple 1971, p. 72). Only 2% of black workers in South Africa are organized into trade unions. The structural conflict between migrant labor and organized nonmigrant white workers redounds to the advantage of the employer. The conflict is based on competition over the distribution of income *within* the working class. Concessions extracted from the mine owners by one group are granted, to a considerable extent, at the expense of the other group. For example, the restrictive practices and development of a strong white trade union led to the institutionalization of a system of migrant labor incorporating an ever-increasing earnings gap between black and white workers (Wilson 1972a, p. 46). Not surprisingly, white workers have assisted management in the subordination of black workers within industry, for example, through the breaking of strikes. Equally, white workers are ever conscious of management's interest in breaching the color bar and advancing black workers into more skilled positions. This reinforces divisions within the working class. In addition, the black labor force has been the victim of collusion among the different mining companies in wage fixing. With the development of the Chamber of Mines to coordinate policies of the industry in areas of common interest to the various companies, there arose a common wage policy based on the principle of "maximum average" (Horwitz 1967, p. 27). Such industrywide policies prevented competition for labor from redounding to the advantage of the black migrant workers.

Finally, I turn to the domination of black workers in the labor market. The superior recruitment organizations of the mining industry give it monopolistic access to such labor reservoirs as Malawi and Mozambique and even more distant territories. In 1973 foreign labor accounted for 80% of the blacks employed. Since pass laws preclude the development of a labor reservoir within the urban areas, they favor industries with effective recruitment organizations which employ black labor in primarily unskilled occupations. With a weaker recruitment capacity, manufacturing industry has to restrict itself to a labor supply from the reserves, for which

it competes with all other employers of black labor. Overpopulation in the reserves and diminishing subsistence levels have led to increases in earnings necessary to supplement rural incomes. Being less dependent on South African labor and drawing extensively on foreign labor reservoirs where subsistence levels have not declined, the mining industry has managed to maintain the real earnings of its black workers at approximately the same level over the past 60 years (Wilson 1972a, table 5).

### A System of Migrant Labor Which Failed to Reproduce Itself

So far I have argued that the distinguishing feature of a system of migrant labor is the separation of processes of renewal from those of maintenance. Further, this separation is not a natural or voluntaristic phenomenon but must be enforced through a set of political and legal mechanisms which presuppose that the migrant is without citizenship rights and has only limited power in the state of employment. Therefore, when the specific mechanisms that enforce the circulation of labor, restrict its upward mobility, and establish the migrant's powerlessness are relaxed or disappear, if my thesis is correct, we should then expect the system to fail to reproduce itself. In this context the decline of migrant labor in Zambia is pertinent.<sup>9</sup>

Prior to the Labour Government's assumption of power in Britain after the Second World War, the pattern of migrant labor between the Northern Rhodesian (now Zambian) copper mines and the rural hinterland followed that just described for South Africa. Until the postwar period, the colonial administration actively organized the political and legal mechanisms that separated the worker from his family. Subsequently, the administration retreated from the performance of these functions for reasons related to Zambia's status as a British protectorate. First, Africans were not merely allowed to organize trade unions but in some instances were actively encouraged to do so. Later in the 1950s, political parties representing the African population began to appear. At the same time, the colonial government became less resolute in defending the color bar in industry (particularly the copper industry). Without support from the colonial administration, white workers were unable to prevent the removal of the color bar from jobs which they had previously monopolized. As restrictions on African advancement were being relaxed, regulations on the geographical movement of black workers began to disappear also. Significantly, in the early 1950s the mining companies began to dispense with their "pole-and-dagga" huts and to build family accommodations for their black employees.

<sup>9</sup> Epstein (1958) and Burawoy (1972, chap. 2) describe these changes as they occurred on the Northern Rhodesian copper belt.

Thus, the separation of renewal and maintenance functions was being slowly and even deliberately undermined. Finally, shifts in ideology from white supremacy to evolutionary movement to African self-determination further weakened the legitimacy of migrant labor and the regulatory mechanisms necessary for its reproduction. Therefore, we may tentatively conclude that, unless separated by a specific set of political and legal institutions, the processes of maintenance and renewal tend to coalesce. In other words, economic factors by themselves cannot enforce the separation of worker from family but must be supplemented by structures of coercion.

Systems of migrant labor, as they have existed or continue to exist in southern Africa, may be regarded as "pure" types. State organization of the separation of maintenance from renewal is transparent. Further, I have shown how a system of migrant labor dissolves when the state no longer performs this function. But the framework developed is of limited interest if it can be applied only to southern Africa. The question before us now is: Can the framework be extended to shed light on the nature of migrant labor in other, radically different countries?

#### FARM LABOR IN CALIFORNIA

The discussion here is complicated by the more variegated history of seasonal agricultural labor in California. I will endeavor to highlight the aspects most relevant to comparison with South African mine workers and to the development of a more general framework for the analysis of systems of migrant labor in capitalist societies.

Because California is the United States's largest agricultural producer, farm labor has assumed a critical role in its development. The history of farm labor is the history of a succession of labor reservoirs. Each group entered as a domestic migratory or alien migrant labor force, but, before stabilizing, voluntarily left agriculture for employment in other sectors of the economy or was removed forcibly and succeeded by a new group of migrants.

The Chinese were the first immigrant group to respond to the seasonal demands of California agriculture. They were rendered occupationally immobile by discriminatory practices, and their stabilization coincided with increasing demands for Chinese exclusion by domestic labor during the last two decades of the 19th century (McWilliams 1964, chap. 2). With the eclipse of Chinese labor, whites affected by the depression of the 1890s were recruited for work in the fields; but with the return of economic prosperity, a new reservoir was tapped—the Japanese (*ibid.*, chap. 4). By the end of the first decade of this century, the Japanese had superseded every other group, only to lose their dominance to Mexicans by 1915. After the First World War and increasingly until the Second, white domestic

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labor was again recruited for farm work. Although attempts to settle the dust bowl migratory workers of the 1930s into camps were made, they were never very successful (McWilliams 1971, chap. 16). Many laborers were recruited from the skid rows of California cities for temporary jobs in agriculture (Fisher 1953, pp. 51-57; Anderson 1923; Parker 1920).

The wartime demand for labor outside agriculture threatened the supply of domestic labor. The governments of Mexico and the United States signed an agreement providing for the use of Mexican labor under contract in farm employment. Known as the *bracero* program, this was the first governmentally administered system of migrant labor in agriculture. At the same time as *braceros* entered legally under contract, illegal migrants, referred to as "wetbacks," were also crossing the border from Mexico in search of employment. Their numbers have varied according to such factors as the state of the Mexican economy (Gamio 1930, chaps. 1, 12), the stringency of border controls (Samora 1971), and the availability of jobs in the United States (see Frisbie [1975] for a statistical analysis of economic push and pull factors). Although the actual number of illegal Mexican entrants is not known, the number apprehended annually rose steadily from the early 1940s to a peak of over a million in 1954 (Galarza 1964, chap. 8). Recent studies indicate that with the termination of the *bracero* program in 1965 the number of illegal entrants has again risen, while commuters who live in Mexico and work in the United States have assumed a new prominence in the border states (North 1970, chaps. 1, 3). Meanwhile, domestic labor has organized itself in an attempt to prevent competition from labor recruited legally or illegally from foreign labor pools.

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Castells (1975) indicates that, in addition to suffering excessive exploitation, migrant labor functions as a regulator of capitalist crises, cushioning the impact of the expansion and contraction of capital. When industry faces a recession, for example, migrant workers are particularly easy to lay off. The nature of agricultural production, rather than capitalist crises, gives rise to fluctuations in the demand for farm labor. Nonetheless, migrant labor performs the same "regulatory" function in California agribusiness, providing for seasonal labor requirements.

The basic dilemma faced by farm employers, particularly those with farm operations requiring seasonal hands in large numbers, is this: They want a labor supply which, on the one hand, is ready and willing to meet the short-term work requirements and which, on the other hand, will not impose social and economic problems on them or on the community when work is finished. This is what is expected of migratory workers. The de-

mand for migratory workers is thus twofold: To be ready to go to work when needed; to be gone when not needed. [U.S. President's Commission on Migratory Labor 1951, p. 16]

The more generic function of a system of migrant labor—namely, the externalization of the costs of labor force renewal and low wage labor—is complicated by the coexistence of three different labor systems in California. First, there are migrants who circulate between Mexico and California. They constitute a system of external migrant labor. Second, there are aliens who reside in California throughout the year. They constitute a system of internal migrant labor. Finally, there is a domestic labor force which migrates from place to place in search of employment. It does not constitute a system of migrant labor as defined here. I shall refer to this fraction of the labor force as migratory labor. At different periods in the history of California agriculture, different systems have been dominant.

The migrants from Mexico bear the closest resemblance to Africans from Malawi or the reserves working in the South African mining industry. In both cases the system of migrant labor facilitates the externalization of the costs of renewal and the provision of earnings at a level commensurable with the day-to-day existence of the farm laborer.<sup>10</sup> A system of internal migration has no obvious parallel with the South African situation. Japanese, Chinese, and many Mexican aliens who worked in the fields during the harvest period did not return "home" in the off-season but eked out an existence in California towns. As a result, they became a potential burden upon the state where they were employed. At the same time, because they were mainly single, able-bodied men, the processes of maintenance were separated from those of renewal, which took place in their country of origin (see, e.g., Fuller 1940, p. 19824).

Domestic migratory labor distinguishes itself from migrant labor by the fusion of the functions of labor-force renewal and maintenance. The employer and/or state must bear all the costs of reproducing labor power. Other techniques are adapted to compensate for the inability to externalize costs in the case of domestic migratory workers. The prevailing adaptation has been the exploitation of family labor in picking crops, so that earnings of the *individual* can be maintained at inordinately low levels. If we look

<sup>10</sup> According to the 1960 census, over half the Chicano families living in the rural areas had an income below \$3,000 and 14% received less than \$1,000 (Briggs 1973, p. 23). Another survey showed that commuters from Mexico were being paid average hourly rates of \$1.65 (if they were Mexican nationals, i.e., "green card" commuters [see n. 12]) and \$1.45 (if they were U.S. citizens) (North 1970, p. 114). The corresponding annual incomes were, respectively \$3,910 and \$2,984 (ibid., p. 117), both falling below the poverty line. Illegal migrants from Mexico were paid at rates between those of green card commuters and U.S. citizen commuters (ibid., p. 116). According to Samora (1971, pp. 98–102), in the El Paso region in 1969 the going wage for wetbacks was between \$0.75 and \$1.10 per hour—far below the national minimum wage of \$1.60.

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upon wages as the costs of maintaining and renewing the family, the greater the number employed within each family, the less each individual member has to be paid. In this way, the earnings of domestic labor are kept at the level paid to internal and external migrants. However, with domestic labor the state of employment has to bear a set of costs, such as welfare for the old and young and education, even though these may be small as compared with costs for other sectors of the national labor force.

Although there is evidence to suggest that growers prefer a system of migrant labor to a system of migratory labor (U.S. President's Commission on Migratory Labor 1951, p. 16), there have been periods in California history, particularly during economic depressions, when migrant labor barely existed. More recently, the organization of migratory labor in the United Farm Workers Union and the eclipse of the bracero program have led to an increase in the use of domestic labor. Clearly the interests of the state, as defined by such factors as the level of employment and the political power of domestic groups, interact with the interests of the growers to determine the relative importance of each system of labor.

The issue of cheap labor arises in the California context, just as it did for South Africa. The immediate economic gains to growers from the use of migrant labor may be more apparent in the case of farm labor. First, migrant labor is a common form of adaptation to seasonal fluctuations in the demand for unskilled work. For example, in the first half of the 19th century, Irish migrants traveled to England to work as farm laborers during harvest periods and returned to Ireland during the slack seasons (Redford 1926, pp. 122-29). They were also paid less than domestic labor. Second, the system of migrant labor is not such a "total" institution in California as it is in South Africa, and it may require fewer resources for its reproduction. It appears to be less a response to government intervention than a direct reflection of the economic interests of the growers. It may be argued that in South Africa political costs as well as economic costs are being externalized, whereas in California the economic costs are paramount. So long as migrant labor was readily available, the need for capital substitution was not urgently felt. But with moves in the direction of effective union organization of domestic migratory workers and the dissolution of the bracero program, growers have turned increasingly to picking by mechanization.

### Twin Dependency

In discussing South Africa, I noted that a system of migrant labor involved a twin dependency on two separated economies. This is also true, but in a weaker sense, in California. External migrants—essentially Mexicans—

depend on their own state and to a lesser extent on employment in the United States. In the case of internal migrants, there is an overriding dependency on employment in the United States; and, like external migrants, they have tended to be restricted to such marginal occupations as farm labor. In both instances, there is a separation of the processes of labor-force maintenance from those of renewal, but the connection between the two is stronger for external migrants.

With respect to the migrant's dependency on employment in the South African economy, I noted the deliberate policies of the colonial administration to force the African population off the land and into the labor market to create an industrial work force and also remove competition with white farmers in the commodity market for agricultural products. These goals were achieved through the expropriation of land and the levying of taxes. The dependency of Mexicans on the United States economy cannot be reduced to such terms. The availability of Mexican labor has been contingent upon such factors as the state of the Mexican economy and political change, as in the revolutionary period between 1910 and 1930 which led to the release of many Mexicans from peonage in the haciendas.

However, at a more general level the proximity of the United States has been a factor in the persistent underdevelopment of Mexico, making it difficult for that nation to absorb the full potential of its labor force or to compete with wages available in the United States. Furthermore, the very sale of labor power by an underdeveloped country, such as Malawi or Mexico, to an economically advanced nation serves only to reinforce the relations of economic subjugation and domination. This is so despite protestations by the South African and United States governments that in employing nationals of underdeveloped countries they are doing these countries a service. In a narrow sense, they are doing just that by absorbing surplus labor that could present a political threat to the underdeveloped nation and by providing rural workers with "their only real opportunity for economic self improvement" and the possibility of remitting income home (Hancock 1959, p. 122). In a broader context, however, migrant labor exists only because of the uneven development of capitalism and reflects the economic dependence of Mexico on the United States and Malawi on South Africa.

It should be noted that some Mexicans who cross the border to work do not in fact return to Mexico on the termination of their employment, just as there are many Malawians residing illegally in South Africa. Many Mexicans attempt to find jobs elsewhere in the United States. Being illegal residents in the United States makes them much more vulnerable to arbitrary exploitation by employers. In many respects their position is akin to that of the internal migrant who faces limited employment opportunity and discriminatory practices. The Chinese and Japanese during those

periods when they dominated farm labor were dependent on finding employment in marginal occupations. Unlike Mexicans, they could not easily return home and become the responsibility of another state. However, because they are single, internal migrants can subsist on relatively small incomes.

Finally, brief mention should be made of attempts to establish a system of migrant labor among domestic workers when they dominated the farm-labor force in the interwar period. Apart from increasing exploitation through the employment of family labor, there were moves among growers to create subsistence economies so as to reduce the burden of the work force on the state and to stabilize its movement. Such programs for "land colonization" stemmed from the potential shortage of labor and the costs of armies of unemployed during the slack season, but they achieved little success before growers discovered alternative external labor reservoirs (McWilliams 1971, pp. 92-96, 200-210; U.S. Congress 1940, pp. 230-31, 240, 250). In effect, the programs were efforts to set up a system of "reserves" as in South Africa or a system of workhouses like those which provided a pool of labor for English employers in agriculture and industry during the 18th century (Redford 1926, pp. 21-23).

More common has been the technique of engendering dependence through the distribution of relief. Unemployed domestic labor is maintained during the slack season by the judicious provision of relief immediately suspended when openings appear in the fields. This ensures the availability of labor in the busy season (McWilliams 1971, pp. 285-96). Similar mechanisms for the distribution of relief appeared in England at the end of the 18th century: ". . . the perpetuation of the Speenhamland and 'roundsman' systems, in all their variety, was ensured by the demand of the larger farmers—in an industry which has exceptional requirements for occasional or casual labor—for a permanent cheap labor reserve" (Thompson 1968, p. 244). As the study of Piven and Cloward (1971, chaps. 1, 4) suggests, the distribution of poor relief is designed to meet the conditions for dual dependency upon the state on the one hand and the employer on the other, so that labor may be mobilized and distributed to accommodate the changing demands of the economy. Poor relief, therefore, may be regarded as a functional equivalent of migrant labor, in that both perform the same regulatory function, cushioning the seasonal labor requirements of the agricultural industry.

### The Regulation of Circulation

Poor relief and land colonization are designed to control the movement of *domestic* labor, so that it is available where and when it is needed and does not constitute a liability where and when it is not needed. What



mechanisms are available to control the movement of external migrants such as those from Mexico? The work contract, defining the relationship between migrant workers and growers or intermediaries, is by its very nature only for temporary employment; after it has expired, the workers have no alternative but to leave the agricultural areas. They may leave for their homes across the border, move into a California town, or migrate to some other part of the United States.

Just as influx control enforces the separation of maintenance and renewal functions while regulating the return of labor to its home, similar mechanisms operate to regulate the movement of Mexican migrants. Thus, border patrol (Samora 1971, chaps. 1, 2) attempts to restrict illegal immigration into the United States. Immigration laws are designed to separate workers from their families, so that the costs of labor-force renewal are borne in Mexico while the United States employer and government, either at the federal or regional level, are responsible only for maintaining workers during the period of employment (North 1970, pp. 92-93).

Immigration laws and their enforcement by border patrol and other government agencies aim to prevent the emergence of pools of unemployed Mexicans liable to become public charges. At the same time, they provide growers and other employers with adequate supplies of labor. The consolidation of the *bracero* program in the 1950s was accompanied by more stringent policies of influx control. During this period, immigration authorities attempted to restrict migration across the border to workers contracted for agricultural employment by agencies established in Mexico. At the same time, legislative measures in the United States were introduced to prevent *braceros* from "escaping" from farm employment and seeking jobs elsewhere. Accordingly, each worker was given a card bearing a contract number, an employer's name, and the names of counties in which it was valid (Galarza 1964, p. 83). In other words, it was a species of the notorious South African pass. These types of restriction on migrant employment in the United States and the removal of migrants from the country when the contract expired ensured their continuing reliance on Mexico and a binding connection to the processes of labor-force renewal.

### Restrictions on Occupational Mobility

The return of migrants to their homes after the termination of the employment contract serves to restrict them to unskilled occupations in particular sectors of employment. Under the system of migrant labor found in South Africa, the color bar broadly defines the boundaries between jobs monopolized by migrants and those held as the preserve of domestic white labor. Structural conflict within the working class of the mining industry occurs in a vertical dimension between a white labor aristocracy and black migrant

labor. By contrast, within California agriculture there is no need for the counterpart of a color bar, because virtually all jobs are unskilled. At the same time, the equivalent of a color bar does operate to prevent mobility out of agricultural employment.

The result is that conspicuous conflicts have occurred in the horizontal dimension between migrant workers (Chinese, Japanese, Mexicans, etc.) and domestic workers (white depression victims, Chicanos, etc.). Differing relations to the means of production have not been the axis of manifest conflict; on the contrary, the working class has been internally divided as a result of differing relations to superstructural elements—that is, differences of legal and political status in the place of employment. Though weak in comparison with organized labor, domestic farm labor is potentially more powerful than are migrant workers. Thus, during the last decade of the 19th century, domestic labor successfully resisted displacement by Chinese labor. Growers continued to employ Chinese labor after legislation had been passed to provide for the exclusion from employment of Chinese not legally resident in the country. The refusal of growers to bow before pressure from labor organizations led to riots throughout the state between 1893 and 1896, eventually forcing the removal of Chinese from the fields (McWilliams 1971, pp. 74–80; Fuller 1940, pp. 19814–15). Since then, domestic labor has had only limited success in establishing itself as a permanent farm-labor force, reflecting its vulnerability to the political power of agribusiness.

### The Vulnerability of Farm Labor

In discussing the reproduction of the system of migrant labor in South Africa, I contrasted the strength of the domestic white workers with the powerlessness of black migrants who confront a state organized for their repression. The perpetuation of the system turns on the ability of white workers to maintain the color bar at a skill level consistent with migrant labor. In California, the situation is reversed. There the reproduction of the system of migrant labor rests, not on the strength, but on the weakness of domestic labor, its inability to prevent growers from drawing upon foreign supplies of labor. We take it for granted that the migrant—internal or external—has little or no power, few if any rights, and virtually no means of appealing against infringements of his labor contract.<sup>11</sup> There-

<sup>11</sup> This is not entirely correct. Particularly in the case of braceros there is a long history of attempts by the Mexican government, under pressure from organized labor in Mexico and anti-American interests, to protect the rights of Mexican nationals employed in the U.S. Southwest. This has sometimes involved blacklisting employers who failed to comply with the terms of the agreement signed between the two governments concerning the working conditions and rights of braceros. For a period during the Second World War, the Mexican government refused to authorize braceros for work in Texas

fore, what is of interest is the manner in which *domestic* laborers have been systematically prevented from forcing the growers to employ them and them alone, under minimum wage conditions.

Farm labor has traditionally been excluded from labor legislation (Briggs 1973, chap. 5; Myers 1959). For example, the National Labor Relations Act of 1964 excluded farm workers from unemployment compensation. From 1910 to 1956, farm wages ranged from 40% to 75% below factory wages (Hancock 1959, p. 25). In 1966 farm wages were half the average of those in industrial employment as a whole. When domestic labor has threatened to organize, it has been either displaced by migrant labor—external or internal—or violently repressed. Thus, Galarza (1964, pt. 4) describes in detail how the *bracero* program fostered the replacement of domestic labor with Mexican labor paid at prevailing rates, ones which domestic workers found unacceptable since they were based on labor-force maintenance rather than maintenance and renewal. In this way the *braceros* came to dominate the picking of a number of crops. The segmentation of the farm-labor force into migrants (legal or illegal) and domestics has obstructed the development of effective union organization. As recently as 1973, strike activity by the United Farm Workers Union was unable to prevent the gathering of the harvest crop by labor recruited from foreign countries.

In other words, the ability of domestic labor to organize itself is severely circumscribed by the power of the growers, who have gained monopolistic access to external labor reservoirs. In achieving these ends, there has been a long history of collaboration between farmers and immigration authorities (Greene 1969) and of collusion between farmers and state police in suppressing labor organizations and labor protest. Where police efforts have been inadequate or ineffectual, growers have shown no hesitation in recruiting "citizen armies" and vigilante groups to combat resistance from farm labor (see, e.g., McWilliams 1971, chaps. 14, 15; U.S. Congress 1940, which was devoted to these issues). While the federal government has been aware of the collaboration of the rich and powerful in California and of the use of the state as an instrument for protecting the economic

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because of instances of extreme racial discrimination. In practice, growers either ignored many conditions of the *bracero* contract or chose to use illegal migrants when they were available. A historical treatment of attempts by the Mexican government to secure reasonable conditions for contract labor working in the United States under the *bracero* programs may be found in Scruggs (1960, 1963). The U.S. Labor Department has also tried to enforce conditions laid down in the *bracero* program. However, in the early 1960s such attempts in the Texas cotton harvest led to court cases in which growers entered suits against the Department of Labor. Where the court's ruling upheld the government, growers rapidly introduced cotton harvesting by mechanization, and the plight of domestic labor was exacerbated (Jones 1965, pp. 131–52). The severity of the growers' reaction attests to the infrequency with which conditions of employment of Mexicans were actually regulated.

interests of large-scale farmers, the strength of the farm lobby in Washington has managed to prevent any effective intervention (Galarza 1970). As recently as 1974, despite opposition from organized labor, the Supreme Court sanctioned the use of foreign migrants on the basis of the administrative fiction that they are legal residents of the United States.<sup>12</sup>

The power of the growers is reflected in their ability to establish common wage rates, and even in times of labor scarcity, these have prevented competition from redounding to the advantage of farm labor. Fisher (1953), McWilliams (1971), Galarza (1964), and others have documented the collaboration of growers in employer associations to define what is in effect a "maximum average," though it is referred to as the "prevailing wage." In theory, the prevailing rates are to be fixed by the free play of the market. In fact, they are established unilaterally by the growers according to the same criteria followed by the South African Chamber of Mines: "... a wage which is fair to one's neighbor in that it is no higher, and a wage which is fair to oneself in that it is no lower" (Fisher 1953, p. 110).

Unilateral wage fixing, monopolistic recruitment, militant antiunionism, and powerful lobbies in central government imply an inordinate concentration of power. For some time commentators have viewed the low wage levels and unhealthy working conditions of farm labor as a consequence of the concentration of land ownership and the vertical integration with the cannery industry, which has engaged in price fixing (McWilliams 1971, pp. 279-80). With the concentration of ownership and the absorption of agriculture into a national food industry, recent years have witnessed the entry of large corporations and industrial conglomerates into large-scale farming. Thus, one discovers that the four leading private owners of agricultural land are Southern Pacific Company, Tenneco Incorporated (the large oil and chemical conglomerate), Tejon Ranch Company (half owned by the *Los Angeles*

<sup>12</sup> Farm workers brought a suit for "declaratory and injunctive" relief with respect to the practice of the Immigration and Naturalization Service in permitting some aliens living in Mexico and Canada to commute to work in the United States on a daily and seasonal basis (October 17, 1974). The Supreme Court ruled (November 25, 1974) by a margin of five to four that "both the daily and seasonal commuters were immigrants who were lawfully admitted for permanent residence and were returning from temporary visit abroad when they entered the United States and were different from those groups of aliens which could be admitted only on certificate by the Secretary of Labor" (*Supreme Court Reporter* 1974, p. 272). The declaration supported the long-standing administrative practice of allowing migrants from Mexico to enter the United States for temporary work (42,000 daily commuters, of whom 25,000 are involved in agriculture, and 8,300 seasonal commuters), on the grounds that such migrants were permanent residents of the United States and therefore not subject to either quota restrictions or certification by the Secretary of Labor (such immigrants use the green card as a reentry permit in lieu of an immigrant visa). Dissenting judges felt the ruling was based on an "administrative construction of a statute which conflicts with the express meaning of the statutory terms" (*ibid.*, p. 283), but it has provided growers with unrestricted access to an external labor supply and as a result has constituted a major obstacle to the effective unionization of domestic workers.

*Times Mirror* Corporation), and Standard Oil of California (see Fellmeth 1971, vol. 1, chap. 1; vol. 2, appendix 1B; Agribusiness Accountability Project 1972). If this were not enough, the problems facing the United Farm Workers Union have been compounded by the intervention of the International Brotherhood of Teamsters, which has signed "sweetheart" contracts with many of the growers.

Does the dominant ideology exercise a moderating influence on the arbitrary use of this power and in particular on the reproduction of the system of migrant labor? Whereas the South African ideology of white supremacy legitimates the colonial superstructure (Burawoy 1974) that organizes the conditions of reproduction of the system of migrant labor and institutionalizes migrant powerlessness, the dominant ideology in the United States is conditioned by notions of "equality," "justice," and "citizenship." Accordingly, the United States government has frequently appeared to resist the use of migrant labor in agriculture, particularly when subjected to pressures from organized labor concerned to protect domestic farm workers (Hawley 1966; Scruggs 1960). The various bracero programs since 1942 have required growers to provide evidence of a shortage of domestic labor and to make visible attempts to recruit such labor. The agreement between the United States and Mexico also stipulated that braceros had to be paid at prevailing rates, and employers were required to make contributions to insurance schemes, housing, and nonprofit canteen facilities and to offer each worker a minimum number of hours of work every week (see, e.g., Galarza 1964, pts. 2, 3).

While these provisions are to be found in the agreements signed between the governments of the United States and Mexico, their execution has been quite a different matter. To supervise the scheme, the United States government appointed bodies sympathetic to the interests of the growers. Together with associations of farm employers, these bodies worked out ways to circumvent the provisions (Fisher 1953, chaps. 4, 5; Galarza 1964, pts. 4, 5). It was in the administration of the program that the government was able to conciliate the powerful growers opposed to restrictions imposed on their employment practices.

If the dominant ideology does not exercise much constraint over the practices of growers, it does tend to conceal those practices. First, it presents United States agriculture as composed of a large number of small-scale independent farmers who work on their own land. This hides the decline in the numbers of such independent farmers and the fact, particularly significant in California, that the overwhelming proportion of land is owned by industrial consortiums and worked by a migrant or migratory labor force. Second, the dominant ideology tends to obscure the typical conditions of migrant-labor exploitation. Just as in South Africa the racial perspectives of separate development have tended to conceal the position of

particular groups with respect to the means of production, in the United States the combination of an ideology which stresses ethnic pluralism with the coincidence of ethnicity and occupation has had a similar effect. Whereas in South Africa conflict between migrants and nonmigrants is highlighted but seen in racial terms, in California conflict between migrant and domestic labor is masked by their common Mexican heritage.

## CONCLUSION

We have learned that one condition for the separation of maintenance and renewal processes lies in the political status of migrant laborers. It is their relation to the state—the denial of legal, political, and civil rights—that distinguishes migrant workers from domestic workers. The distinction holds for both mine workers on the Rand and farm laborers in California. At the same time, we have observed a marked contrast in relations between domestic and migrant workers in the two areas. In South Africa a caste division in the form of a color bar separates the two sectors of the labor force, while in the United States competition between domestic and migrant labor prevails. In the former country, domestic labor has access to considerable resources of political power, while in the latter it appears relatively weak. What does this discrepancy signify?

## The State and Its Bearing on the Reproduction of Migrant Labor

The fact that unbridled competition between migrant and domestic labor is as ubiquitous in the United States as it is restricted and regulated in South Africa, *irrespective* of the particular industry, indicates that the skill differentials found in the mining industry and absent in agriculture cannot explain the different patterns of relations between migrant and domestic workers in our two case studies.<sup>18</sup> On the contrary, it suggests that we must turn to broader characteristics of the two societies in order to understand the differences alluded to in the previous paragraph.

First, there is the simple demographic fact that migrant labor, legal and illegal, is *relatively* insignificant in the United States (though not as insignificant as is commonly supposed) as compared with its central role in the South African labor system. Second, domestic labor in South Africa constitutes a minority segment of the total labor force and as a result is *relatively* undifferentiated as compared with the domestic labor force in the United States. The simple dichotomy between domestic workers with rights of citizenship and migrants with no rights may be a useful simplifi-

<sup>18</sup> Outside the context of gold mining, there is the added complication in South Africa that not all black labor is obviously migrant labor (see, e.g., Wilson 1972b, chap. 4).

cation in the South African context, but it is too crude for the United States, where such marginal fractions of the domestic labor force as migratory farm workers are incomparably weaker than organized labor in other sectors of the economy.

At the same time, the numerical and functional significance of migrant labor is contingent upon the state's capacity to reproduce a system of migrant labor. I have emphasized repeatedly that the volume of migrant labor is not something to be taken as given but is *created and recreated by the state*. Within a single nation, the *state* determines the relative importance of migrant and domestic labor. Accordingly, changes in the organization of the state, as in Zambia, can go so far as to transform a numerically dominant sector of the labor force from migrant to domestic status and at the same time deny a minority sector its domestic status. Similarly, in contrast to other European countries, Britain has until recently awarded full citizenship rights to immigrants from other parts of the Commonwealth. Whereas immigrants to France, Germany, and Switzerland have tended to assume the status of migrants, in Britain they became part of the domestic labor force (Castles and Kosack 1973, chap. 11). To what extent the political status of immigrants actually affects their economic status has been an issue for debate, with some playing down the importance of differences (Castles and Kosack 1973) and others giving them greater emphasis (Rex 1974). The point is, however, that the state determines whether an immigrant is to be a migrant or a domestic worker. Therefore, the first two factors considered above—the demographic importance of migrant labor and the differentiation of the domestic labor force—are contingent upon a third: the nature of the state, its organization and in particular the relative autonomy of the economy with respect to the political system.

In South Africa a dual labor market is organized by a monolithic state, so that one sector is largely composed of migrant workers and the other of domestic workers. In the United States, on the other hand, with its less centralized state apparatus, the dual labor market is defined in terms of relation to the economic structure. Low-profit service and competitive industries with an unstable nonunionized or weakly unionized labor force produce the lower income strata of the working class, while high-profit monopoly industry with stable unionized labor accounts for the higher income strata (O'Connor 1973, chap. 1; Harrison 1972). The dominant division in the South African labor market is based on relation to the state, whereas that in the United States is based on industry of employment, that is, relation to the economy. In one instance, migrant labor constitutes the basis of an entire segment of the labor force; in the other, it forms but a fraction of a segment. Yet in both instances, although for different reasons, the reproduction of migrant labor deepens the division

between the two segments. We may conclude, therefore, that the relevant differences between South Africa and the United States turn on the relative autonomy of the economy with respect to the state. In South Africa an overarching state intervenes in the organization of productive and market relations, whereas in the United States productive and market relations are reproduced with significantly less intervention from the state.

What, then, has this analysis of reproduction requirements accomplished? I have assumed that, although the conditions of reproduction may vary over time and between societies, what is being reproduced is defined by certain invariant structures. In the case of migrant labor, the invariant structure was found to be the separation of maintenance and renewal processes. Furthermore, the unique characteristics and consequences of a given system of migrant labor emerge out of the interplay between the invariant structure and a specific economic and political context. In other words the marked dissimilarity of the systems of migrant labor in South Africa and the United States may be attributed to the differing political, ideological, and economic situations in which the separation of maintenance and renewal processes is organized. Thus, reproduction analysis is a powerful tool in comparative analysis, between societies and over time, because it accounts simultaneously for similarity and diversity. Yet the very strength of such analysis is also its major weakness, as is apparent in my treatment of labor power. Throughout, I have assumed that labor power itself is invariant. This is implied by limiting the reproduction of labor power to two processes—maintenance and renewal. The treatment ignored the possibility that labor power, like machinery, may be adapted to the changing demands of capital and technological innovation. In my examples of migrant labor, adaptation is not a significant factor, because the jobs performed remain the same over time. But extending the analysis of reproduction of labor power to an entire labor force over a long period shows that requisite skills, education, and socialization in the broadest sense, that is, the content of labor power, undergo considerable change (Braverman 1974). Changes in the structure of capitalism, such as the consolidation of the dual economy, have repercussions for processes of labor force adaptation (Bowles 1972). In other words, a diachronic rather than synchronic analysis of the reproduction of labor power cannot, in general, restrict itself to the processes of maintenance and renewal but must be extended to include processes of adaptation.

### The Rise and Fall of Systems of Migrant Labor

So far, I have established the conditions for the reproduction of a system of migrant labor, but a complete theory of reproduction should embrace a characteristic dynamics (Cortés, Przeworski, and Sprague 1974, pp. 279–



80). The reproduction of any system in and of itself creates tendencies toward its change and persistence. Moreover, these tendencies can be deduced from the "laws" or conditions of reproduction. Are there any rudimentary processes which might constitute a theory of the dynamics of a system of migrant labor? Or are the changes brought about by the internal structure of the system, that is, by its dynamics, swamped by external exigencies which impinge in an unpredictable fashion upon the system?

I noted that the system of migrant labor in Zambia dissolved primarily because the colonial state disengaged itself from the organization of the separation of the maintenance and renewal processes. To what extent was this the product of a dynamics immanent in the structure of the system of migrant labor and its reproduction? To what extent was it the result of external forces? The expansion of the Northern Rhodesian (Zambian) economy required the expansion of the system of migrant labor. The increased involvement of Africans in wage employment led to their organization initially into tribal associations but also into embryonic and, later, strong trade unions. Organized economic class struggles inevitably led to increased remuneration and consequently undermined the foundations of the system of migrant labor and precipitated its dissolution. Advancing with economic struggles, political struggles eroded another central requirement of the reproduction of a system of migrant labor—migrant-labor powerlessness. In other words, the expansion of the system of migrant labor stimulated and structured class struggles which ultimately forced the breakdown of the system itself. At the same time, however, intertwined with such a "bottom up" view of the dynamics of the system of migrant labor are the "top down" concessions by the colonial government prompted by political changes in Britain and by the general climate in the colonized world. To disentangle the intricate interaction of concessions and struggles in the decline of the system of migrant labor in Zambia would be a worthwhile and challenging task. Suffice it to say here that internal dynamics are but a partial explanation of the dissolution of the system in Zambia.<sup>14</sup>

<sup>14</sup> The system of migrant labor has not completely dissolved. In a survey I conducted among a carefully selected sample of 218 mine workers in 1969, I found that 39% had returned to their home villages within the preceding two years and 71% within the preceding five. Breaking these figures down by age and length of time spent in urban areas (urban experience), I found that the older a worker (controlling for urban experience), the more frequently he returned to his home village, and the greater his urban experience (controlling for age) the less frequently he returned to it. Zambian workers have never had their land expropriated in the manner of the enclosure movement in England. Therefore they retain ties to the rural areas as a form of security, particularly for retirement. This may involve remitting a part of their income home—though very few (7%) claim to do so on a regular basis—or entertaining visiting kinsmen from the home area, as well as returning periodically to the rural areas. As an increasing proportion of the work force is born in the urban areas, with no home village,

## Migrant Labor: Functions and Reproduction

Nonetheless, Zambia does illustrate dynamics arising in the place of employment—namely, the weakening of the colonial state and the advance of the political and economic status of the migrant worker. By contrast, for South Africa we stressed the dynamics of the interaction of capitalist and precapitalist economies and the way in which the expansion of the former tended to erode the latter. In its reproductive role, the South African state organizes counteracting influences designed to re-create the precapitalist mode of production. But it is becoming increasingly apparent that, although the system of migrant labor contains its own contradictions that continually threaten to undermine the system, the major threat to the system, particularly as it affects the gold mines, is from relatively autonomous external sources.

Prior to 1950, southern Africa constituted a relatively coherent political unit bound together by various forms of colonial rule and organized around certain focal points of industrial development. The peripheral areas served as labor reservoirs and were made subservient to the economic interests of the extractive industries, most notably the copper mines of Northern Rhodesia, the coal mines of Southern Rhodesia, and the gold mines of South Africa, as well as agriculture in all these territories. Struggles for political independence in Malawi, Tanzania, and Zambia led to the "autonomization" of the foreign reservoirs that supplied labor for the gold mines. The ban on recruitment for South African industry imposed by the Zambian and Tanzanian governments meant that South Africa would have to become increasingly reliant on its own internal system of migrant labor. Hence there emerged renewed interest in the reserve areas and the creation of Bantustans. With no major industry of its own, Malawi continued to serve as a major foreign labor reservoir for South African industry, particularly the gold mines, reinforcing its own underdevelopment and its dependency on South Africa.

The sporadic but very definite success of guerrilla movements in Portuguese Africa led to a coup d'état in the metropolitan country and to the demise of Portuguese colonialism in Africa, precipitating disturbances throughout southern Africa. The white minority regime of Southern Rhodesia is now under pressure to negotiate with black nationalist leaders, and in 1974 Malawi declared a ban on the supply of migrant labor to South

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and as pension and welfare schemes improve in the economy as a whole, the retention of linkages between urban and rural communities should be expected to decline further. But the most telling statistics concerning the dissolution of the system of migrant labor are to be found in commitment to employment in industry: in 1969 the average length of service of Zambian mine workers was 9.4 years (as compared with 4.3 years in 1955), with a corresponding turnover of 6.4% per annum—an extremely low turnover by any standards.



Africa. With the independence of Mozambique, there is the possibility of another major source of labor withdrawing its supply to South Africa. The reaction of the South African Chamber of Mines has been as follows:

Energetic steps have been taken to attract South Africans and the proportion has increased from 22 per cent at 31st March, 1974 to 32 per cent at 30th April, 1975. It is hoped that it will be possible to increase this proportion even further, and it will therefore be necessary to compete for labour with other sectors of the economy and to provide more housing accommodations for South African workers. Inevitably the bulk of mining labour will remain migratory for many years to come but it is hoped that a core of stable South African employees can be built up on longer-life mines. . . . [South African Chamber of Mines 1975]

The South African state is now faced with the dilemma of choosing either the expanded reproduction of the system of migrant labor within its own boundaries or the dissolution of the system. (For further details, see South African Institute of Race Relations 1975, pp. 281-88; Leys 1975.)

The South African example demonstrates that a system of migrant labor is placed in jeopardy as soon as the external labor reservoir gains political autonomy. The study of Mexican migrant workers in the United States lends some support to such a conclusion. The utilization of Mexican labor to bolster the United States economy has been the subject of considerable political debate within Mexico, from time to time leading the Mexican government to impose controls and conditions on the use of such labor. The *bracero* program, with its elaborate although rarely entirely enforced system of regulations, reflected just such a concern for the treatment of Mexican nationals. In practice, however, political control over the supply of labor is only a minor factor in the determination of the ebb and flow of migrant labor across the border. Indeed, it may be argued that in this instance it is unrealistic to speak of a system of migrant labor at all, because any characteristic dynamics of the system are overwhelmed by a wide range of external factors, such as the state of the economy on either side of the border.

Parallels with slavery are intriguing and deserve brief mention. In its purest form, slavery is an extreme version of migrant labor in which processes of renewal take place in a distant country (insofar as male workers only are involved) and maintenance takes place on the plantation; the severance between maintenance and renewal is total and final. Indentured labor stands somewhere on the continuum between systems of slavery and the systems of migrant labor examined in this paper. Under a system of worldwide colonialism, such as existed in the 18th century, slaves could be procured readily; but with economic, political, and ideological changes in the system of world capitalism, the movement for abolition established itself. As a result, the survival of the system became

contingent upon the organization of labor-force renewal *alongside* its maintenance. Wallerstein (1976) argues that increased "costs" of slavery in the decades before the American Civil War noted by Fogel and Engerman (1974) and Genovese (1974) were a result of American prohibition of international slave trade in 1807. Further, he argues, slavery was a viable system of labor only so long as a substantial proportion of the costs of renewal were borne in external labor reservoirs, and therefore their collapse spelled the downfall of slavery.<sup>15</sup> In other words, the *specific* cheapness of slavery is to be found, neither in the powerlessness of slaves (though this is an inevitable and necessary condition) nor in their efficiency, but (if it is to be found at all) in the characteristic mode of reproduction of labor power. When monopolistic access to external labor reservoirs is lost, systems of slavery and migrant labor have to be reconstituted or transformed. It remains to be seen whether the South African system of migrant labor is to be successfully resurrected through the intervention of the state or whether it will follow the historic road of slavery in the United States.

### Beyond Migrant Labor

What light does our conceptual distinction between maintenance and renewal shed on systems of labor that are not migrant and in which internal differentiation of the domestic labor force is prominent? One approach to these broader issues is a reformulation of our analysis of the costs of reproduction of labor power. Earlier, the savings generated by a system of migrant labor were expressed in terms of the *externalization* of certain costs. That is, certain processes normally financed by the employer and the state of employment are externalized so that the employer and the employing state assume no responsibility. However, such savings could be viewed in terms of the *reduction* of certain renewal costs rather

<sup>15</sup> Although the heyday of American slavery appears to have come after the importation of slaves became illegal, with the slave population quadrupling in 50 years as a result of natural increase, the increase must be regarded as a consequence of the incorporation of renewal processes into plantation society. The reproduction of a system of slavery involves the reproduction not merely of slaves but also of slave owners. Thus, to show that slavery is a viable labor system at any point in time, it is not enough to show that the "break-even age"—when "the accumulated expenditures by planters on slaves were greater than the average accumulated income which they took from them" (Fogel and Engerman 1974, p. 153)—is less than life expectancy. It must also be shown that the difference reflects earnings greater than the costs of the life-style of the slaveholders. Although Fogel and Engerman (1974, p. 155) do note that the break-even age rose in the decades before the Civil War and imply that life expectancy also increased, they neither tell us the trend of the difference between these two values nor relate the difference to the costs of the reproduction of the style of life of the slaveholders. In other words, the data of Fogel and Engerman do not speak directly to the hypothesis presented here.

than their externalization. That is, it is cheaper to educate and bring up a family, and so forth, in a Bantustan or a Mexican shantytown than in Johannesburg or California, where the reproduction of labor power is organized for higher-income groups and where, as a result, lower-income groups are penalized. Luxuries superfluous to the basic processes of renewal in the Bantustan or Mexican town or village become necessities in Johannesburg or California. In other words, the requirements for a minimal standard of living vary from place to place, according to the level of industrial development. Increases in the level of consumption or, more broadly, the rise of the cost of reproduction of labor power, is a consequence of and a condition for the economic expansion of capitalist societies (Gorz 1967, chap. 4).

Against this background, the significance of migrant labor lies in the separation of the processes of maintenance and renewal, so that renewal takes place where living standards are low and maintenance takes place within easy access of employment. Thus, wages earned by migrant workers are lower than those of domestic workers, because the former require fewer resources to sustain the renewal process than the latter. Where a supply of migrant labor is not available, industry itself may migrate to areas where the costs of the reproduction of labor power are lower. Indeed, the migration of industry may be a more attractive proposition for capitalists, as it relieves them of responsibility for the social and political costs of the maintenance of migrant labor. On the other hand, when a host country assumes responsibility for the regulation and domination of the labor force, the capitalist enterprise is frequently subjected to political and economic uncertainties beyond its control.

Expressing economic benefits in terms of reduction rather than externalization of costs allows us to go beyond migrant labor and examine various ways of organizing the reproduction of labor power within a single economy. The question becomes: Are there areas or institutions within a given society which organize the process of labor-force renewal at reduced costs? If there are, what specific mechanisms perpetuate the coexistence of differing modes of organizing labor-force renewal? Thus, one might ask whether the urban ghetto in the United States is a functional equivalent of the Bantustan in South Africa. Although an adequate treatment would involve a careful comparison of the political economies of Bantustan and ghetto, a few remarks may be made in passing. One striking resemblance between the two places is the importance of the female-dominated household. Liebow (1967), for example, shows how the "matri-focal" family of the urban ghetto is a product of "the inability of the Negro man to earn a living and support his family" (p. 224). Marriages do not last long, and women of the ghetto, like women of the Bantustan and of the villages in British Guiana (Smith 1956), are forced to extend

their sources of income by whatever means they can muster and through alliances with a succession of mates. Liebow's observations suggest that ghetto life is characterized by the separation of the processes of renewal, engaging the mother and her children, from those of maintenance, which concern the "marginal" or temporarily employed lower-class male.

My argument would be that the ghetto, like the Bantustan and the Mexican town or village, is a definitely located institution whose function for the capitalist economy is the allocation of the renewal processes to areas where renewal costs are low. As one exposition of the dual labor market thesis for the United States points out, the reproduction of a differentiated labor force rests on the reduction of renewal costs through segregation in housing, education, and welfare (Baron and Hymer 1968). "Institutional racism" (Wilson 1973, p. 34) or "internal colonialism" (Blauner 1972, pt. 1) may then be understood as the apparatus, coercive where necessary, for the regulation of renewal processes of a particular segment of the labor force and their allocation to specific institutions and areas. In other words, I am suggesting that racism be interpreted as a particular mode of reproduction of labor power and that "powerlessness" is not so much the defining as a necessary condition for racism. Furthermore, distinctions between different types of race relations, such as van den Berghe's (1967) distinction between competitive and paternalistic ones, may be understood in terms of particular articulations of productive and reproductive processes. Thus, where the same elements of the dominant class organize both production of commodities and reproduction of labor power, a system of paternalistic relations emerges, whereas when the two processes are separated, with one organized by the employer and the other by the state, a different system—competitive race relations—emerges.

One may ask one further question: What is the importance of the variation in the costs of labor-force renewal? Low-profit industry is dependent on the existence and perpetuation of institutions which reduce the costs of labor-force renewal for segments of the working class. At the same time, the economy as a whole, but particularly high-profit monopoly industry, is dependent on increasing levels of demand and therefore on increasing the costs of labor-force reproduction. This opposition of short-term need for reducing the cost of labor and long-term need for increased demand is partially resolved through the bifurcation of the working class (both in South Africa and the United States). With the expansion of capital, the opposition develops and the schism within the working class widens (Fuchs 1968, pp. 53, 61; Bluestone 1972). One segment of the labor force devotes more time and money to renewal processes, while the other struggles to maintain itself from day to day. The consequences may be observed in the diverse forms of family organization measured in terms of the relationship between renewal and maintenance activities.

I introduced race into the discussion and could equally well have extended the argument to relations between the sexes<sup>16</sup> to defend the thesis that it is not just the "powerlessness" which certain minority groups share but also their different modes of insertion into the reproduction of labor power which determines their group characteristics. That blacks have had less political power than whites means that they are more vulnerable to excessive exploitation, but this has been realized only through specific modes of reproduction of labor power on the plantation and in the ghetto. What differentiates women from blacks and both from migrants can ultimately be reduced to the different modes of reproduction of labor power in which they are engaged or their different relations to a single mode of reproduction. Interpreting the significance of these various modes of reproduction of labor power thrusts the discussion back to the relationship between the rate of exploitation and the rate of profit. I have repeatedly pointed out that the relationship between exploitation and profit is mediated by an ensemble of structures that reproduce not merely labor power but also the social relations characterizing capitalist production. Intensifying exploitation does not appear spontaneously but has to be created and re-created by modes of enforcement; it requires a more elaborate and costly apparatus of reproduction, or more specifically the expansion of the state, which in turn eats away at profits. Yet at the same time, a reduction in the rate of exploitation may generate greater consent, thereby allowing a relative contraction of the state and conceivably leading to an increase in the rate of profit. Here I can only raise what is a complex empirical and theoretical problem.

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<sup>16</sup> First, the family facilitates the expansion and contraction of capital through its dual function as labor reservoir and reproductive unit. Increased labor demands, as occasioned, for example, by wartime emergency, lead to the expansion of the state's role in the renewal process, as reflected in the increase in day-care centers (Kleinberg 1974) and the release of women for participation in the labor force. The allocation of women to low-paying jobs is premised upon the costs of maintenance and renewal in a family with two sources of income. Second, as with race relations, I would argue that relations between men and women are the *effects* of the articulation of primarily two types of structures: modes of production and modes of reproduction. To be sure, people can be distinguished by their anatomy or their color, but these only become socially significant when they are the basis of the allocation of individuals to specific sets of roles. What are perceived as characteristics of blacks or women are, not some primordial givens, but the effects of the particular positions they occupy in the social structure. A theory of relations between sexes or between races is first and foremost a theory of empty places in the structures of production and reproduction and only secondarily a theory of the allocation of individuals to those places.

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# Ethnic Residential Segregation: Patterns of Change<sup>1</sup>

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This study traces changes in patterns of ethnic residential segregation for Cleveland from 1930 to 1970 and for Boston and Seattle from 1960 to 1970. For Cleveland the data indicate some clear declines in residential segregation since 1930 for "new" southern and eastern European ethnic groups; "old" groups, however, actually increased in segregation. Between 1960 and 1970, we could find few changes in patterns of ethnic segregation for Boston, Cleveland, and Seattle. On the whole, we found that differences in residential segregation among ethnic groups, both cross-sectionally and over time, were highly related to differences in social status. It is clear, nevertheless, that ethnic segregation would continue to exist even if social status differences among ethnic groups disappeared.

For several decades, sociologists have largely accepted the "Chicago School" or human-ecology conception of ethnic residential segregation as an aspect of status differences among groups which would disappear with social mobility. Robert E. Park (1967) propounded this position forcefully when he noted that "changes of economic and social status . . . tend to be registered in changes of location" (p. 60). Park's general perspective was shared by his Chicago colleague, Ernest W. Burgess (1923), in "The Growth of the City." Burgess argued that, while immigrant groups tended to concentrate in segregated areas around the Central Business District (CBD) upon arrival in an American city, in time the immigrant communities began to disperse. Implicitly, time was important because the groups adopted American patterns of behavior and assumed high-status social positions.

Recently studies of ethnic segregation in Toronto (Darroch and Marston 1971) and New York (Kantrowitz 1973) have challenged much of this conception of ethnic segregation. In this paper we attempt to resolve some

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of the issues by studying changes in ethnic residential segregation for the Cleveland Standard Metropolitan Statistical Area (SMSA) from 1930 to 1970 and for the Boston, Cleveland, and Seattle SMSAs from 1960 to 1970. We make a modified defense of the Chicago School, because our position is basically that differences in ethnic residential segregation are highly related to differences in status, while at the same time ethnic segregation remains strong in American cities and somewhat independent of social status.

## THE DEBATE

The study of ethnic segregation by ecologists has been limited by the nature of census data on tracts or small neighborhood units in the metropolis. Data on the races of all Americans have generally been collected from decade to decade, but data on country of origin have been restricted largely to the foreign stock, consisting of two groups, foreign-born persons (first generation) and natives with at least one foreign-born parent (second generation). It is thus extremely difficult to test the notion that third and later generations in the United States become assimilated into the general population, although the foreign stock may be compared with various other "baseline" populations.

In the most exhaustive empirical analysis from the perspective of the Chicago School, Lieberman (1961, 1963) focused on cross-sectional correlates of residential segregation for foreign-born groups and on changes in the degree of segregation for foreign born in political wards from 1910 to 1920 and in census tracts from 1930 to 1950. He selected for analysis 10 large northern central cities with significant foreign-born populations.

His analysis depended heavily on the distinction between "old" and "new" immigrant groups, largely northern Europeans versus central, southern, and eastern Europeans. The groups were distinguished by their times of arrival in American society and by their social statuses, with the old groups generally higher in status. Lieberman found on a cross-sectional basis that the old immigrant groups were generally weakly segregated from the native white (third or later generation) population, while the new ones were much more segregated. Over time decreases occurred in the average segregation of new and old foreign-born groups from the native population. He also found that many of the cross-sectional differences in segregation of various ethnic groups from the native white population were clearly related to group differences in social status, time in the United States, ability to speak English, and degree of naturalization of the foreign born. Finally, he determined that higher-status ethnic groups were generally more decentralized than lower-status ones and that the decentralization of all groups tended to increase over time.

Consistent with Lieberman's results, Taeuber and Taeuber (1964) re-

vealed a general decrease in the segregation of foreign-stock ethnic groups in Chicago from the third-generation (and later) white population during the period from 1930 to 1960. It was particularly characteristic of the new immigrant groups.

In his cross-sectional 1960 study of ethnic segregation in the New York Standard Consolidated Area (SCA), Kantrowitz (1973) attacked the Lieberman and Taeuber studies on empirical and methodological grounds. His research showed that ethnic residential segregation remained strong several decades after the great waves of European migration ended. His major methodological criticisms were twofold. First, he pointed out, most of the declines in residential segregation reported in the 1963 Lieberman study were not very impressive in magnitude. For instance, over the 10 cities, the average index of dissimilarity of the foreign-born white ethnic groups from the native white population was 43.9 in 1930 and 38.6 in 1950 (Lieberman 1963, pp. 66-67). Second, he criticized the use of the nonforeign stock as the base group for measuring the degree of ethnic residential segregation, pointing out that the composition of the base population has probably changed over time. As the grandchildren of the new immigrant groups have become more numerous, the base population has increasingly included the descendants of those groups. Thus the decline of ethnic segregation could be due as much to the changing nature of the base population as to real changes in the degree of segregation. Kantrowitz suggested that this problem could be solved by comparing the degree of segregation among various foreign-stock populations over time. While we have some sympathy with him on this point, it is also true that the foreign-stock population does not include the third generation of immigrants and that it is a proportion of the total population which is decreasing as many immigrants and their children from the early 1900s age and die. A study of foreign-stock populations, whether cross-sectional or longitudinal, can be only a crude indicator of the general degree of ethnic residential segregation in American society.<sup>2</sup>

In their 1961 cross-sectional research on Toronto, Darroch and Marston (1971) concentrated primarily on the importance of a social status model in explaining differences in ethnic segregation. Fortunately, the Canadian

<sup>2</sup> An analysis of the foreign-stock population over time may underestimate decreases in the degree of segregation by ethnicity. The European foreign-stock groups have become increasingly old as the great waves of migration have ended. Research (Spear 1970) in urban spatial mobility suggests that old persons infrequently change residence; thus the foreign-stock population would be expected to be rather stable in residential location. Decreases in residential segregation would be more frequent among the third generation, who are generally more mobile as a result of their age. The old age of many foreign-stock groups is suggested by data for the Cleveland SMSA in 1970 (U.S. Bureau of the Census 1973, table 15). While only 5.2% of the native white of native-parentage population is over 65, the following percentages of selected foreign-stock groups are over 65: Germany, 33.3; Ireland (Eire), 28.7; Sweden, 33.3; United Kingdom, 25.3.

census collects data on the ethnicity of all its citizens, regardless of generation. Thus the ethnic segregation of all Toronto residents could be studied. In the most persuasive part of their study, Darroch and Marston (p. 505) compared the residential segregation of individuals in different ethnic groups but with similar social status. They found that *individuals* of different ethnicity but similar status were still highly segregated from each other. This finding, in conjunction with other data analysis, led them largely to reject the social status model of ethnic segregation. They did not, however, highlight another finding, namely, that ethnic *group* differences in social status, as measured by income, education, and occupation, were highly related to group differences in ethnic segregation. In fact, more than half the variance in group differences in ethnic residential segregation could be accounted for by group differences in social status. We believe that the contradictory findings of the Darroch and Marston research may be resolved by distinguishing between individual and group models of ethnic residential segregation.

The individual model of residential segregation portrays each member of an ethnic group as basing his decision on where to reside on the actual social status of other persons in the neighborhood. Any member of an ethnic group, say German, looks at his potential neighbors and determines their individual social character—for example, their income. If the known incomes of all the neighbors are compatible with his and if ethnicity is not a criterion, the German will feel free to move into the neighborhood. Within this perspective, we should find Germans of high income willing to live near members of some other group, such as Poles, as long as all have high incomes. According to the individual model, if ethnicity is a criterion of location, Germans will not locate in a neighborhood, regardless of high individual income, if the potential neighbors belong to another ethnic group. This model is consistent with the Darroch and Marston finding that persons of similar income but different ethnicity do not live close together in Toronto. The model may be tested also by what is known as the demographic technique of indirect standardization, which has been so used by Darroch and Marston (1971), Lieberman (1963, pp. 88–89), and the Taeubers (1969, pp. 81–86), among others.<sup>8</sup> Using that technique, these three studies could not account for major differences in ethnic segregation by individual differences in social status.

The group model of residential segregation, which we believe to be more

<sup>8</sup> Indirect standardization is useful when one does not have an actual tabulation of ethnicity by status for each census tract but does know the distribution of persons by ethnicity and by status for each tract, plus the distribution of ethnicity by status for the whole city or metropolitan area. Using the metropolitan area relationships between ethnicity and status, one generates an expected distribution of persons by status for each census tract and then compares it with the actual distribution.

sociological in its orientation, portrays the residential location decision of an ethnic-group member as being based on his perceived knowledge of the average status of potential neighbors, which is in turn a consequence of perceived ethnic-group characteristics. Thus a high-status German views the status of his potential neighbors, not on the basis of their individual incomes, but on his perception of the average income of their ethnic group. He will be willing to live next door to Poles, regardless of individual incomes, if he perceives the Polish ethnics as a whole to be relatively similar in status to his group. This model permits individual high-status Germans to be segregated from individual high-status Poles, as long as the location decisions of the Germans are based on their overall evaluation of the Polish group as low in status. According to this model, once average status differences between groups become small, there should be little segregation between German and other ethnic group members, say Poles. The model is consistent with Darroch and Marston's finding that group differences in social status are clearly related to group differences in residential segregation. We test this model, which has received less research attention.

In summary, three questions deserve more investigation as a result of previous research. First, is ethnic residential segregation increasing or decreasing in American metropolitan areas? Second, is it related to social status differences between ethnic groups? Third, are ethnic groups distributed spatially around the CBD in a pattern consistent with the Burgess hypothesis? We turn shortly to each of these issues.

#### SMSA SELECTION

We selected the Boston, Cleveland, and Seattle SMSAs to investigate for a variety of reasons. Cleveland is the only SMSA with data for 1930 on ethnic segregation of foreign stock in both the central city and the suburbs, although the suburban areas are limited to the inner areas of East Cleveland, Cleveland Heights, Lakewood, and Shaker Heights (all in Cuyahoga County). It thus becomes possible to trace changes in ethnic segregation for the three censuses of 1930, 1960, and 1970, which report data on the total foreign-stock population by census tract. Boston is potentially an interesting metropolitan area to investigate in relation to Cleveland, because the two areas have somewhat different ethnic compositions, even though both developed primarily before the great waves of foreign migration ended with World War I. The foreign-stock population of Boston is heavily weighted with groups from Ireland, Italy, and the USSR (overwhelmingly a Jewish population), while that of Cleveland is more heavily weighted with groups from such Slavic areas of southern and eastern Europe as Czechoslovakia, Hungary, and Poland. Seattle is a worthwhile area to investigate, because it has low proportions of foreign-stock groups in general,

and because it has developed primarily in the period since the great migration to the United States ended. Most of the European migrants to Seattle probably came from other places in the United States, and thus enclaves of recent migrants from abroad did not have the same opportunity to develop as in Boston and Cleveland.

Our analysis of Cleveland at all three time points involves 232 comparable census tracts in the central city and the four inner suburbs. That of Boston and Seattle consists of 396 and 218 comparable census tracts, respectively, which fell in the urbanized area boundaries of 1970. A few census tracts in each place were eliminated because of large institutionalized populations, and a few census tracts had to be combined to make them comparable over time. Like other studies of the same topic, ours measures the degree of residential segregation by the index of dissimilarity, which can vary from 0 to 100 and indicates the percentage of one group which would have to switch census tracts to be equally distributed with the group with which it is compared. For a complete discussion of the index, see Taeuber and Taeuber (1969, pp. 195-245).

### SEGREGATION PATTERNS

Our primary concern is the changing nature of ethnic residential segregation in American cities; yet this cannot be understood independent of basic patterns of segregation, regardless of time. Our basic strategy is to analyze the patterns of ethnic segregation in Cleveland for 1930 and in Boston, Cleveland, and Seattle for 1960. We then analyze the changes in patterns.

In tables 1 and 2, we present matrices indicating the degree of segregation among various ethnic and racial groups at the baseline points of 1930 and 1960. Below each baseline index we indicate the absolute change during the time period at hand, either from 1930 to 1960 or from 1960 to 1970. We summarize levels of segregation at various time points in table 3.

Ethnic groups were selected for inclusion on the basis of the availability of published census tract data for the relevant time periods. All groups are foreign-stock populations except for Negroes and other nonwhites,<sup>4</sup> who include all generations, and Puerto Ricans, who are all Americans by birth but were born in or had at least one parent from Puerto Rico. Following the convention of previous studies, we distinguish three major groups or divisions: "old" immigrant groups (from Canada, Germany, Ireland, Sweden, United Kingdom), "new" immigrant groups (from Austria, Czech-

<sup>4</sup> In 1930, the category of other nonwhite or other races besides Negroes included persons of Mexican descent. Hence this group is not exactly comparable with the other-nonwhite category in 1960 and 1970, although we have presented the data. In 1930, 47.8% of the other-races population of Cuyahoga County, including all the area under consideration, was Mexican in origin (U.S. Bureau of the Census 1932, pp. 480-99).



TABLE 1  
INDICES OF RESIDENTIAL DISSIMILARITY FOR ETHNIC GROUPS, CLEVELAND SMSA\*

Group	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
<b>Old:</b>															
1 Canada .....	26.7	26.4	26.5	26.5	11.9	41.7	65.0	59.3	65.2	70.6	57.5	...	86.6	63.0	...
2 Germany .....	+0.4	+5.7	+7.6	+7.6	+8.5	-8.5	-19.8	-19.2	-22.0	-20.0	-8.4	...	+0.1	-10.1	...
3 Ireland .....	27.1	30.7	36.2	36.2	25.3	32.5	52.9	53.5	63.6	63.5	56.5	...	87.6	66.6	...
4 Sweden .....	+7.2	-0.3	+3.5	+3.5	-1.9	-7.7	-18.0	-16.2	-21.5	-20.6	-7.4	...	+1.3	-12.9	...
5 United Kingdom ...	32.1	30.4	37.3	37.3	24.0	43.7	64.5	59.1	65.7	69.0	60.2	...	87.0	60.9	...
6 Austria .....	+7.1	+6.5	+0.4	+0.4	+3.1	-1.2	-13.2	-14.1	-16.2	-9.6	+1.4	...	+2.3	-5.5	...
7 Czechoslovakia ....	34.1	39.7	24.9	24.9	24.9	44.8	70.0	65.0	68.6	72.0	54.7	...	88.0	68.2	...
8 Hungary .....	+10.1	+15.0	+5.4	+5.4	+5.4	+4.1	-10.8	-9.9	-16.0	-9.0	+2.6	...	+3.1	-6.0	...
9 Italy .....	20.4	23.4	30.3	30.3	30.3	39.4	62.8	59.1	64.8	68.4	56.0	...	86.9	64.2	...
10 Poland .....	+8.6	+7.4	+12.2	+12.2	+12.2	-6.5	-18.4	-18.1	-23.5	-18.1	-5.1	...	+1.1	-12.3	...
11 USSR .....	33.2	24.8	42.5	48.9	32.9	49.5	48.4	58.0	58.0	58.7	44.5	...	83.1	68.7	...
12 Mexico .....	+5.2	+0.6	+1.5	+1.7	+1.3	-16.2	-12.5	-18.4	-18.5	-18.5	+1.2	...	+5.6	-11.6	...
13 Negro .....	45.2	34.9	51.3	59.2	44.4	33.3	52.7	63.2	63.2	59.5	70.6	...	88.1	74.2	...
14 Other nonwhite ....	+1.4	-3.0	-1.5	-0.8	-2.3	-2.2	-14.6	-18.8	-18.8	-17.9	-14.8	...	+0.9	-8.9	...
15 Puerto Rico .....	40.1	37.3	45.0	55.1	41.0	35.9	38.1	68.2	70.3	70.3	66.4	...	85.2	76.3	...
Other:	+0.8	+2.6	+4.0	+0.6	-1.0	+0.7	+2.7	-22.5	-22.5	-18.5	-14.1	...	+3.0	-16.0	...
12 Mexico .....	43.2	42.1	49.5	52.6	41.3	39.6	44.4	45.7	45.7	74.0	60.2	...	76.6	70.8	...
13 Negro .....	+3.4	-1.5	+1.6	+2.9	+0.2	+3.6	+2.9	+0.9	52.6	-21.4	-6.0	...	+10.5	-10.7	...
14 Other nonwhite ....	50.6	42.9	59.4	63.0	50.3	40.2	41.6	51.8	52.6	62.7	62.7	...	87.9	76.2	...
15 Puerto Rico .....	+7.2	+1.1	+3.8	+3.5	+3.1	+3.6	-0.3	+2.7	+0.7	-13.5	-13.5	...	+0.6	-13.9	...
Other:	47.1	49.1	61.6	57.3	50.9	45.7	55.8	52.3	54.2	49.2	49.2	...	85.7	76.4	...
12 Mexico .....	+3.2	+1.1	+3.3	+5.3	+2.7	+3.7	+0.8	-0.2	+2.2	+2.2	+2.2	...	+3.1	-13.9	...
13 Negro .....	69.2	67.7	65.8	74.3	69.8	67.6	68.5	68.3	72.9	68.9	71.8	...	...	...	...
14 Other nonwhite ....	+0.8	+0.3	+7.7	+0.4	-0.2	+4.7	+3.2	+5.6	+0.8	+8.3	+8.7	...	...	...	...
15 Puerto Rico .....	86.7	88.9	89.3	91.1	88.0	88.7	89.0	88.2	87.1	88.5	88.8	86.9	...	72.9	...
Other:	-4.0	-3.2	+1.9	-5.7	-1.3	-3.6	-5.9	-3.0	+1.5	-3.8	+0.4	+4.8	...	-9.7	...
12 Mexico .....	52.9	53.7	55.4	62.2	51.9	57.1	65.3	60.3	60.1	62.3	62.5	68.8	63.2	...	...
13 Negro .....	-6.1	-7.7	+0.8	-1.4	-2.6	-7.4	-10.5	-9.9	-7.6	-7.4	-6.5	+5.7	+3.3	...	...
14 Other nonwhite ....	79.9	77.0	80.9	85.2	80.9	77.3	76.7	78.9	80.5	74.0	77.7	57.8	79.4	68.9	...
15 Puerto Rico .....	-0.6	-2.6	-1.2	-1.6	-1.6	-2.1	+0.4	+0.1	-1.3	+1.4	-1.0	+14.8	+11.7	-1.1	...

Sources: Census 1981 tables 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, 12, 13, 14, 15, 16, 17, 18, 19, 20, 21, 22, 23, 24, 25, 26, 27, 28, 29, 30, 31, 32, 33, 34, 35, 36, 37, 38, 39, 40, 41, 42, 43, 44, 45, 46, 47, 48, 49, 50, 51, 52, 53, 54, 55, 56, 57, 58, 59, 60, 61, 62, 63, 64, 65, 66, 67, 68, 69, 70, 71, 72, 73, 74, 75, 76, 77, 78, 79, 80, 81, 82, 83, 84, 85, 86, 87, 88, 89, 90, 91, 92, 93, 94, 95, 96, 97, 98, 99, 100.

# INDICES OF RESIDENTIAL DISSIMILARITY FOR ETHNIC GROUPS, 1960-70\*

Group	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
<b>Old:</b>															
1 Canada .....	14.2	27.6	16.2	13.7	25.1	29.4	35.9	38.6	28.1	26.1	52.0	85.2	59.2	77.1	
	+2.5	-0.1	+3.2	+3.0	+2.0	+5.2	+7.0	-3.2	+3.4	+0.4	-11.0	-7.2	-13.6	+3.8	
2 Germany .....	32.2	22.6	16.0	14.0	24.1	27.7	34.4	36.0	25.6	23.5	52.1	82.9	55.9	78.1	
	+0.6	+3.3	+5.7	+3.4	+1.7	+7.6	+9.1	-1.2	+5.7	+1.0	-8.8	-6.8	-12.7	+2.3	
3 Ireland .....	28.0	34.3	27.3	25.2	29.0	35.9	39.8	40.8	29.3	29.3	50.5	80.2	55.1	78.2	
	+2.1	+1.3	-0.8	+2.3	+6.5	+4.1	+4.4	-0.2	+8.1	+2.2	-1.8	-6.4	-7.7	+4.8	
4 Sweden .....	27.8	34.1	40.3	17.4	26.8	30.6	39.1	41.5	32.2	29.3	55.0	86.2	60.9	77.4	
	+3.2	+1.5	-1.6	+5.0	+4.2	+7.2	+5.0	-3.1	+2.8	+2.0	-10.2	-7.6	-13.8	+3.4	
5 United Kingdom .....	18.0	26.6	30.2	24.3	24.2	28.3	37.5	37.0	27.7	24.2	51.8	84.2	58.1	78.0	
	+2.6	+0.5	-0.2	+1.9	+4.2	+7.3	+5.5	-3.0	+3.6	+1.8	-8.2	-6.3	-12.1	+3.4	
<b>New:</b>															
6 Austria .....	38.8	35.5	49.0	47.0	38.8	35.9	41.7	34.6	32.0	29.1	52.4	83.3	56.9	76.9	
	+3.4	+4.5	-0.9	+0.9	+3.4	+6.0	+0.6	+3.2	+3.8	+2.1	-6.3	-6.4	-10.8	+4.1	
7 Czechoslovakia .....	45.7	47.8	55.3	50.9	45.7	50.4	44.7	44.3	34.6	36.1	55.3	86.6	59.5	75.7	
	+12.5	+6.9	+10.5	+10.4	+12.5	+6.2	+10.0	+0.8	+7.4	+3.4	-0.4	-9.3	-7.5	+2.0	
8 Hungary .....	55.6	44.1	57.9	57.2	52.1	39.7	56.2	48.5	38.9	37.8	60.1	81.4	58.0	80.1	
	+0.7	+7.4	+5.1	-0.5	+2.0	+9.1	+8.6	-1.4	+8.0	+3.0	-6.0	-3.7	-4.9	+3.7	
9 Italy .....	36.8	46.9	40.1	46.0	40.0	55.3	60.9	65.3	34.6	38.3	52.8	77.7	46.3	81.1	
	-1.0	-0.4	-1.1	-0.9	-2.0	-0.4	+12.0	+1.2	+1.7	-3.1	-0.8	-10.6	-11.9	-1.3	
10 Poland .....	42.5	41.7	47.0	53.4	43.1	37.8	55.1	51.7	25.4	25.4	54.4	78.7	52.6	75.9	
	-2.8	-1.3	-3.9	-4.6	-4.5	+2.8	+8.0	+7.7	-2.3	+9.0	-8.2	-6.6	-9.5	+6.7	
11 USSR .....	58.6	50.6	61.4	63.4	55.7	33.9	65.0	48.9	40.2	40.2	54.6	78.4	52.2	73.2	
	-5.0	-1.7	-6.5	-6.0	-6.7	+1.9	-2.9	+5.7	-2.0	-2.0	-7.0	-2.5	-6.8	+7.0	
<b>Other:</b>															
12 Mexico .....	79.0	78.0	83.9	79.6	78.2	81.8	77.8	80.6	85.4	86.0	86.1	78.3	59.2	73.0	
	-3.3	-6.7	-5.4	-3.9	-3.3	-7.0	-6.1	-2.2	-3.4	-6.3	-6.4	-2.7	-4.8	+11.4	
13 Negro .....	81.0	81.9	81.7	85.5	82.6	83.5	85.3	85.1	84.8	81.7	82.7	89.7	55.8	86.0	
	-1.3	-1.8	-2.1	-1.2	-2.9	-0.3	+4.8	+4.4	-0.4	-1.9	-0.7	+3.0	-2.2	+2.6	
14 Other nonwhite .....	56.4	53.9	57.8	64.0	55.9	57.8	64.3	61.2	65.8	60.8	66.3	82.4	67.8	79.5	
	-5.7	-6.8	-7.1	-4.7	-7.5	-4.4	+1.9	+3.0	-6.5	-9.4	-10.1	-5.2	+3.0	+3.9	
15 Puerto Rico ...	73.9	75.8	77.7	74.9	75.0	78.4	77.4	81.4	79.9	78.9	84.8	87.2	79.0	75.9	
	+1.7	+1.5	-0.8	+5.1	+2.0	+1.5	+8.1	+4.7	+1.0	-4.4	-4.8	+0.6	-16.2	-13.2	

Sources.—U.S. Bureau of the Census 1962, table P-1; 1972, tables P-1, P-2.  
 Note.—Data based on 193 census tracts in Seattle and 396 tracts in Boston.  
 \* Values in 1960 and 1970 are in parentheses. 1960-70: after diagonal. Seattle: before diagonal. Boston: after diagonal.

TABLE 3  
AVERAGE INDICES OF RESIDENTIAL DISSIMILARITY FOR ETHNIC GROUPS

Group	CLEVELAND			BOSTON		SEATTLE	
	1930	1960*	1970*	1960	1970	1960	1970
Old:							
1 Canada .....	50.0	42.9	46.4	47.3	50.5	37.8	37.4
2 Germany .....	49.6	41.2	42.5	45.6	46.5	36.2	37.2
3 Ireland .....	52.4	48.4	52.8	52.0	56.1	40.8	42.1
4 Sweden .....	54.7	52.6	56.9	56.5	60.1	39.7	39.9
5 United Kingdom .....	49.0	41.8	44.6	46.6	48.8	37.2	37.9
New:							
6 Austria .....	51.1	43.6	44.3	47.7	48.5	40.8	41.9
7 Czechoslovakia .....	64.4	50.2	48.6	53.4	52.3	44.6	47.7
8 Hungary .....	63.6	49.2	49.3	52.7	53.2	48.4	51.3
9 Italy .....	66.6	51.0	52.0	54.7	55.4	46.6	44.1
10 Poland .....	69.4	54.4	55.7	56.8	58.7	40.8	43.3
11 USSR .....	62.6	56.4	57.7	58.9	60.6	39.8	40.7
Other:							
12 Mexico .....	...	...	...	69.9	74.6	57.3	52.6
13 Negro .....	84.6	86.5	84.5	86.0	85.5	80.4	74.9
14 Other nonwhite .....	69.9	58.9	53.7	60.3	56.1	57.8	49.6
15 Puerto Rico .....	...	...	...	76.8	77.9	77.9	82.0
Average:							
Old vs. old .....	27.0	30.2	38.8	30.2	38.8	19.4	22.2
Old vs. new .....	59.0	46.5	48.3	46.5	48.3	31.3	34.6
Old vs. other .....	75.9	72.0	69.1	73.5	72.1	67.9	62.1
New vs. old .....	60.5	45.4	47.0	43.4	47.0	37.1	40.7
New vs. other .....	79.1	74.8	69.5	74.2	72.7	66.8	62.8
Other vs. other .....	72.9	63.2	66.5	70.8	77.4	72.0	73.3

\* Average index for same groups as reported in 1930.

oslovakia, Hungary, Italy, Poland, USSR), and non-Europeans (Negroes, Mexicans, Puerto Ricans, other nonwhites, primarily Asian). The old-new distinction refers to the approximate time of arrival of the group in the United States, not to that of the individuals in this study.

In general, the baseline data for 1930 in Cleveland and 1960 in the three metropolitan areas suggest a pattern similar to that found by Kantrowitz in his study of the New York SCA. Old groups of predominantly northern European descent, including Canadians, tend to be little segregated from each other, while new groups of other Europeans are relatively segregated from each other and from the northern Europeans. Puerto Ricans, Negroes, Mexicans, and other nonwhites tend to be greatly segregated from each other and from all other groups. In fact, the data suggest that the four latter groups form relatively separate communities in the cities, while the European ones are more dispersed throughout the total metropolitan area.

As Kantrowitz has pointed out, the levels of ethnic segregation are generally greater than or equal to levels of segregation found by occupation (Duncan and Duncan 1955; Guest 1970, pp. 195-96). The levels of segregation are also much higher than those found by family or life-cycle status (Guest 1970, pp. 193-94; 1972a).

Consistent with our expectations, there are also significant differences in the level of segregation in 1960 between Seattle on the one hand and Boston and Cleveland on the other. Particularly among the European groups, the general levels of segregation in Seattle seem to be at least 10 points below the indices for Cleveland and Boston, suggesting that Seattle's later development probably affected the opportunity for ethnic communities to develop.

In general, the summary data in table 3 suggest some clear changes in patterns of segregation in Cleveland from 1930 to 1960. However, patterns of segregation remained remarkably constant over the three metropolitan areas from 1960 to 1970.

As table 1 shows for the period from 1930 to 1960, some groups increased in degree of segregation from each other while others clearly decreased. In general, four patterns may be discerned. First, the black population was highly segregated from all other groups in both 1930 and 1960, with little change in overall pattern. Second, other nonwhites (generally Asian Americans) showed some major decreases in segregation from all other groups. Third, the old groups (United Kingdom, Canada, Ireland, Sweden, and Germany) generally became increasingly segregated from each other, although levels of segregation by no means reached those of the new groups. That is, the groups with the least segregation in 1930 generally showed the greatest increases in segregation. Fourth, the new European groups generally decreased their segregation from each other and from the old northern European groups. Some declines were fairly substantial. For instance, the

segregation of British from Czechs decreased from 62.8 in 1930 to 44.4 in 1960. That of Czechs from Poles decreased from 59.5 to 41.6. Among the new European groups, the smallest decreases, and some increases, were registered by Russians who were generally Jewish in origin. Although the Taeubers' study (1964) of Chicago from 1930 to 1960 focused on changes in ethnic segregation from the native white of native-parentage population, their results for the new and old groups generally parallel our own.

The results for changes between 1960 and 1970 in ethnic segregation in the three metropolitan areas are less clear-cut. In fact, it is difficult to state that segregation is increasing or decreasing for any specific group. In Cleveland and Seattle, the average segregation increased slightly; in Boston the index decreased by an extremely small amount. The trends from 1930 to 1960 for Cleveland are matched by the 1960-70 data only for the old groups, as the average degree of segregation among them continued to increase. The other possible relationships were remarkably stable, however, indicating that the declining segregation of the new groups from each other and from the old groups had ended by 1960.

The strongest gains in average degree of segregation for European groups occurred among Czechs in Boston and Hungarians in Boston and Seattle. In both places there were sizable growth rates for what had been very small ethnic populations in 1960,<sup>5</sup> and thus the increased segregation could be due to the establishment of new ethnic communities. Much of the Hungarian and Czech population growth is no doubt due to the in-migration of refugees from political turmoil during the 1950s and 1960s.

Over the three SMSAs among the non-European groups, Negroes and other nonwhites showed a slight trend toward decreases from high levels of segregation. This trend pertained especially to other nonwhites. Nevertheless, changes over the decade still left these groups much more segregated than the European ones, and the declines in segregation were by no means pervasive across ethnic groups. The Spanish-origin groups, Mexicans and Puerto Ricans, showed no noticeable general trend in segregation patterns. Interestingly, Mexicans and Puerto Ricans became increasingly segregated from each other.

To summarize the results to this point, they suggest the clear continuing importance of ethnic segregation in American society over time; the phenomenon seems relatively tenacious, and in the most recent time period, 1960-70, there have been only minor alterations in its pervasiveness. When

<sup>5</sup> Most specific foreign-stock populations have been on the decline as a result of the aging of many groups and the general decrease in foreign migration to the United States since World War I. However, the number of Czechs in Boston increased from 1,969 in 1960 to 2,495 in 1970, and the numbers of Hungarians in Boston and Seattle, respectively, increased during the same time period from 2,943 to 3,429 and from 1,664 to 2,184 (U.S. Bureau of the Census 1973, table 15; 1963, table 99).

the longer time span of 1930–60 is observed, however, for Cleveland there is some evidence that certain southern and eastern European groups are becoming markedly less segregated from the general population. Yet their segregation continues at substantial levels. Ethnic residential communities are far from having disappeared.

#### SOCIAL STATUS AND SEGREGATION

According to the traditional Chicago School interpretation of ethnic segregation, differences in ethnic residential segregation should be heavily related to those in social status between ethnic groups. We have already admitted that an individual model of this relationship is probably not very satisfactory, but we leave open the question of whether a group model works. Unfortunately, completely comparable data on social status differences among ethnic groups by metropolitan area are not available for the time periods 1930–70 and 1960–70. Thus it is impossible to determine directly how changing social status is related to changing residential segregation. We did perform, however, two types of analyses which should shed some light on the “group” phenomenon of ethnic segregation. First we determined the relationship of group differences in social status and other variables to group differences in residential segregation in 1970 for the three metropolitan areas. Then we determined how 1970 group differences in social status in Cleveland are related to changes between 1930 and 1970 in indices of residential segregation.

Using 1970 data, we wanted to determine how the difference in distribution by status is related to that in distribution by residence. The units of observation were all possible pairs of foreign-stock ethnic groups. The Negro, other nonwhite, and Puerto Rican populations were not analyzed in this section, as not all necessary data were available.

Besides dissimilarity in distribution of occupation, education, and income in 1970, it was also possible to measure dissimilarity among the foreign-stock groups for three other measures of group differences—the age distribution of the population, the mother tongue or language in early childhood, and the generation in the United States (first or second). The potential importance of these factors in determining differences in residential location beyond differences in social status seems rather obvious. For instance, there is abundant evidence (Guest 1972a) to suggest that families segregate themselves in neighborhoods by stage in the family life cycle; thus age segregation might be expected to be a prime cause of residential segregation by ethnicity.

The categories of analysis were determined by the reports on each SMSA in the special 1970 census report on national origin and language (U.S. Bureau of the Census 1973, tables 15, 16). The indices of dissimilarity were

computed over all possible categories. For occupation, males only were studied. For mother tongue, we eliminated respondents who reported none, and we treated "all other" languages (generally a very small group) as a separate category.

We first tested a social status model of ethnic residential segregation in 1970, using the indicators of dissimilarity in distribution of income, education, and occupation. As table 4 shows, between 59% and 70% of the

TABLE 4  
RELATIONSHIPS OF ETHNIC CHARACTERISTICS TO RESIDENTIAL DISSIMILARITY  
AMONG FOREIGN STOCK GROUPS, 1970

	Cleveland	Boston	Seattle
Dissimilarity:			
Income			
$r$ .....	.72	.84	.75
$\bar{X}$ .....	9.6	12.7	10.4
SD .....	4.7	6.7	4.9
Occupation			
$r$ .....	.61	.66	.61
$\bar{X}$ .....	15.8	21.6	12.8
SD .....	6.9	9.7	5.9
Education			
$r$ .....	.10	.71	.11
$\bar{X}$ .....	14.0	19.2	10.1
SD .....	5.7	9.4	3.2
Age structure			
$r$ .....	.66	.66	.33
$\bar{X}$ .....	20.5	20.9	21.1
SD .....	13.9	11.8	14.2
Mother tongue			
$r$ .....	.43	.50	.49
$\bar{X}$ .....	71.7	64.8	64.5
SD .....	16.2	15.6	17.5
Generation in U.S.			
$r$ .....	-.01	.66	.21
$\bar{X}$ .....	5.4	8.0	7.1
SD .....	3.6	6.1	5.2
Residential location			
$\bar{X}$ .....	50.7	52.5	36.3
SD .....	13.4	15.4	9.3
Regression effects in status model:			
Income			
$B$ .....	0.42	0.81	0.68
$b$ .....	1.20*	1.88*	1.29*
SE .....	0.34	0.30	0.23
Occupation			
$B$ .....	0.55	-0.09	0.11
$b$ .....	1.07*	-0.15	0.17
SE .....	0.30	0.22	0.19
Education			
$B$ .....	-0.34	0.12	0.14
$b$ .....	-0.81*	0.20	0.41
SE .....	0.27	0.24	0.25

TABLE 4 (Continued)

	Cleveland	Boston	Seattle
Constant .....	33.62	27.96	16.39
$R^2$ .....	.60	.70	.59
df = 62			
Regression effects in 6-variable model:			
Income			
$B$ .....	0.21	0.69	0.73
$b$ .....	0.60	1.59*	1.38*
SE .....	0.34	0.27	0.21
Occupation			
$B$ .....	0.54	-0.07	0.09
$b$ .....	1.06*	-0.11	0.14
SE .....	0.24	0.20	0.19
Education			
$B$ .....	-0.35	-0.07	0.02
$b$ .....	-0.82*	-0.12	0.05
SE .....	0.22	0.23	0.27
Age structure			
$B$ .....	0.28	0.06	-0.25
$b$ .....	0.27*	0.0	-0.16*
SE .....	0.09	0.20	0.06
Mother tongue			
$B$ .....	0.31	0.22	0.29
$b$ .....	0.25*	0.21*	0.15*
SE .....	0.06	0.08	0.05
Generation in U.S.			
$B$ .....	0.03	0.24	0.20
$b$ .....	0.10	0.62	0.35*
SE .....	0.25	0.40	0.14
Constant .....	15.53	16.59	10.63
$R^2$ .....	.76	.78	.70
df = 59			

NOTE.— $r$  = Pearsonian correlation coefficient with residential segregation index;  $\bar{X}$  = mean degree of dissimilarity; SD = standard deviation of mean;  $B$  = standardized partial regression coefficient;  $b$  = unstandardized partial regression coefficient; and SE = standard error of  $b$ .

\* Significant at .05 level, one-tailed  $F$ -test.

variance in segregation scores can be explained by the three indicators of social status; thus most of the variation in residential segregation can be attributed to group differences in social status. The greater the difference in social status between any two groups, the greater the difference in residential segregation patterns. Of the three measures of status, only income has universally strong and statistically significant effects, both at the zero order and partial levels. Small dissimilarity in income distribution, which is typical, can lead to relatively great dissimilarities in residential distribution. For instance, in Cleveland the partial regression coefficient of 1.20 for income suggests that every increase of 1 in the index of dissimilarity for income will lead to an increase of 1.20 in that for residence. The effects of education and occupation are less consistent; education has a negative effect in Cleveland and occupation a negative one in Boston. The meaning of the partial regression coefficients is difficult to determine, given the strong



causal relationships among the status measures. All the statistically significant partial regression weights at the .05 level (one-tailed *F*-test) are indicated in table 4 by an asterisk.

Given the power of income in the equations, one might ask how much variance in segregation scores can be explained by income alone. As table 4 shows, more than half the variance in residential segregation in each SMSA can be explained by income dissimilarity alone, and in Boston almost 70% of all variance in residential segregation can be attributed to income differences. The strong predictive power of income is particularly pleasing, since we generally agree with Hawley (1950), a noted human ecologist, that "rent operating through income is a most important factor in the distribution and segregation of familial units" (p. 282).

Nevertheless, differences in residential segregation can be explained also by differences on the nonstatus measures. The three nonstatus variables added a relatively large amount of variance in all three metropolitan areas (from 8% to 16%) once they were included in regression equations with the status measures (table 4). The partial effects of each of the six variables in the equations must be interpreted with some care, because the samples are small, and because dissimilarity on one indicator may be closely related to dissimilarity on another. For instance, dissimilarity in income is related to that in age, as young persons tend to have lower incomes than older ones.

Of the three nonstatus variables, only mother tongue has consistent and statistically significant results (groups with similar mother tongues have low degrees of residential segregation). While age dissimilarity has statistically significant effects in both Cleveland and Seattle, great dissimilarity in age distribution is actually related to less segregation in Seattle. Dissimilarity in generation in the United States has a statistically significant partial effect on residential location only in Seattle. The weak predictive power of this variable should not be too surprising, since the data refer to generation in the United States for members of the specific foreign-stock group. The data do not indicate for how many generations the ethnic group has been in the United States.

Fortunately, the constant terms (intercepts) in the regression equations have substantive interpretations. They indicate the average degree of residential segregation which would remain between any two groups if no dissimilarity existed on the status measure or on others. That is, if all the indices of dissimilarity between any two groups were zero, would residential segregation by ethnicity exist? The answer is a rather clear yes. For instance, in Cleveland, analyzing only the status model, any two ethnic groups would still have a residential dissimilarity index of 33.6 if they did not differ at all on the measures of income, occupation, and education. Although in Boston and Seattle the indices would be lower, ethnic segrega-

tion would still clearly exist in the absence of status differences between groups.

Some of what appears to be a high average degree of residential segregation in the three metropolitan areas, particularly Cleveland and Boston, when social status differences are controlled, is further reduced when the three variables of age structure, generation, and mother tongue are also included in the analysis. As table 4 shows, Boston would have the highest average index of segregation or constant, 16.59, if all ethnic groups did not differ on any of the six variables. We would interpret this index as being relatively low, particularly since one would expect some segregation among ethnic groups on the basis of simple chance. Unfortunately, it is not clear what minimum residential segregation indices would be expected by chance. In any case, we interpret our data to mean that most ethnic groups in American cities would only be slightly or, at most, moderately segregated from each other if differences in status (in particular), age distribution, and mother tongue were removed.

The possible importance of other variables in explaining differences in residential segregation may sometimes be determined by inspecting residuals, that is, the differences between the expected and the actual degrees of segregation among various ethnic groups, based on the regression equations. We present in table 5 the average residuals over the three metropolitan areas, based on the status model only. The residuals of the six-variable model were generally similar. After inspecting the residuals, we were convinced that other variables could not have major effects in explaining patterns of ethnic residential segregation. Most of the residuals across the metropolitan areas seemed distributed in a relatively random fashion. The most prominent trend was the tendency of Austrians, Germans, Canadians, and British (and, to a slight extent, Swedes) to be somewhat less segregated from each other than one would predict from either the social status or the six-variable model. All these groups are characterized by physical and cultural attributes clearly associated with what is sometimes perceived as the dominant Nordic or Anglo-Saxon culture, and all of them are well represented in the older American stock. Other groups may maintain a somewhat higher degree of segregation from each other and the native populations, because they have what are considered more distinctive or perhaps less acceptable social characteristics, independent of the variables in the above analysis. Their segregation may result from a combination of self-choice and the discrimination of the larger community.

We now turned to test a model of change in ethnic segregation between 1930 and 1970 for the Cleveland SMSA. We attempted to predict ethnic segregation levels in 1970 with four variables: ethnic segregation in 1930

TABLE 5  
AVERAGE RESIDUAL DEGREE OF RESIDENTIAL SEGREGATION OVER THREE SMSAs, BASED ON STATUS REGRESSION, 1970

Group	1	2	3	4	5	6	7	8	9	10	11	12	Average over All Groups
Old:													
1 Canada .....	...	-13.5	-4.3	-7.0	-16.0	-8.7	-3.0	-3.1	+1.3	+0.3	-2.4	-4.1	-5.5
2 Germany .....	...	...	-6.3	-3.2	-12.0	-11.8	-4.9	+1.2	-1.2	-1.7	-3.2	+2.8	-4.9
3 Ireland .....	...	...	...	+0.1	-4.0	-1.8	+6.5	+2.6	+5.9	+7.6	+5.3	-1.3	+0.9
4 Sweden .....	...	...	...	...	-8.2	-4.0	+2.1	-1.3	+2.1	+3.5	+5.0	-3.2	-1.3
5 United Kingdom .....	...	...	...	...	...	-5.8	+1.6	+0.4	+0.3	+2.1	+0.6	-1.6	-3.9
New:													
6 Austria .....	...	...	...	...	...	...	+3.6	+0.1	-0.4	+0.4	-2.4	+2.3	-2.6
7 Czechoslovakia .....	...	...	...	...	...	...	...	+10.2	+5.3	+6.1	+9.4	+5.5	+3.9
8 Hungary .....	...	...	...	...	...	...	...	...	+5.1	+11.9	+3.8	+11.5	+3.9
9 Italy .....	...	...	...	...	...	...	...	...	...	-0.1	-0.3	+3.3	+1.9
10 Poland .....	...	...	...	...	...	...	...	...	...	...	-6.0	+8.0	+2.9
11 USSR .....	...	...	...	...	...	...	...	...	...	...	...	+2.4	+1.1
Other:													
12 Mexico .....	...	...	...	...	...	...	...	...	...	...	...	...	+2.3

NOTE.—Negative numbers indicate less segregation than would be expected; positive numbers indicate more segregation than would be expected.

and the indicators of dissimilarity in 1970 for income, occupation, and education. One would naturally expect the 1930 levels of segregation to predict the 1970 levels; there would be a strong partial regression effect of 1930 segregation on 1970 segregation. The remaining unexplained variation for 1970 may be considered due to residential segregation increasing for some groups and decreasing for others. If social status can explain changes in segregation, we should expect the status indicators to have strong partial effects on 1970 status, independent of 1930 segregation. That is, groups with great dissimilarity in status would have the greatest increases in segregation.

In table 6, we present the standardized and unstandardized regression equations of the four variables on the 1970 index of segregation. The analysis does not include Mexicans, since data on their segregation in 1930 were not available.

The index of segregation in 1930 explained 42% of the variance in the 1970 index, indicating at least moderate shifts over time. One might expect an even higher correlation, since cross-time stability seems to characterize many aspects of social systems. The three status variables added 22% variance to the 42% explained by the 1930 index alone, indicating that much of the shift in residential segregation between 1930 and 1970 was related to the status differences between groups. Groups with little difference among each other in 1970 social status had relative decreases, while those with the greatest differences between each other showed the greatest relative increases.

Differences in education have effects opposite to expectations; nevertheless, the effects of income and occupation are consistent with expectations and, which is important, the standardized partial effect of occupation is greater than the partial effect of the 1930 segregation index. In summary, shifts in ethnic residential segregation between 1930 and 1970 are related, at least moderately, to differences in group status levels. At the least, our results indicate that 1970 levels of segregation are related more to 1970 measures of status dissimilarity than to 1930 levels of segregation.

#### STATUS AND DISTANCE

An important aspect of a social status interpretation of ethnic segregation is the notion that groups with higher status should be most decentralized in relation to the CBD of cities. This is consistent with the general notion that persons with higher status, regardless of ethnicity, tend to locate farther from the CBD than those with lower status. According to this perspective, one should also find increasing patterns of decentralization for ethnic groups over time as they increase in social status and are assimilated in the American social status structure (Cressey 1938; Ford 1950).

TABLE 6  
REGRESSION MODEL OF CHANGE IN RESIDENTIAL SEGREGATION FOR CLEVELAND SMSA, 1930-70

	Disimilarity:	1	2	3	4	5	Mean	SD	B	b	SE of b
1	Income	...	0.63	0.57	0.08	0.40	8.11	3.30	0.19	0.56	0.33
2	Occupation	...	...	0.82	0.37	0.61	14.72	6.63	0.60	0.86	0.24
3	Education	...	...	...	0.34	0.40	14.51	6.07	-0.38	-0.60	0.24
4	Residence, 1930	...	...	...	...	0.65	53.57	15.57	0.54	0.33	0.06
5	Residence, 1970	...	...	...	...	...	46.21	9.54	...	...	...

NOTE.—Constant = 19.99;  $R^2 = .64$ ;  $df = 50$ ; B = standardized partial regression coefficient; b = unstandardized partial regression coefficient; and SE of b = standard error of b.

Various research suggests that the tendency of social status to increase with distance from the CBD is by no means strong in most metropolitan areas; in some, social status tends in fact to decrease slightly with distance from the CBD (Guest 1971, 1972*b*). One might therefore expect that the strongest relationships between distance from the CBD and the relative spatial position of ethnic groups by status would exist in SMSAs with the strongest general tendency for status to increase with distance from the CBD. Fortunately, according to previous research (Guest 1971), the three metropolitan areas under investigation may be arrayed in terms of their general relationship of distance and social status. Cleveland has a relatively strong relationship between distance and status, and Boston has a moderate one. Seattle's is slightly negative.

In table 7 we present the zero-order correlation coefficients for the three SMSAs between the indices of centralization for each foreign-stock group and the average status of each group in terms of education, income, and occupation. The index of centralization, discussed extensively elsewhere (Redick 1956), measures the dissimilarity in the residential distribution of each ethnic group by distance from the CBD against the distribution of

TABLE 7  
SPATIAL LOCATION OF ETHNIC GROUPS IN CLEVELAND, BOSTON, AND SEATTLE SMSAS

	CLEVELAND			BOSTON		SEATTLE	
	1930	1960	1970	1960	1970	1960	1970
Ethnic group							
centralization:							
United Kingdom .....	-.15	-.25	-.25	-.06	-.06	+.07	+.06
Canada .....	-.13	-.24	-.23	-.11	-.06	+.01	+.01
Ireland .....	+.02	-.21	-.27	+.13	+.12	+.23	+.16
Sweden .....	-.20	-.32	-.20	-.15	-.14	+.03	+.08
Germany .....	-.05	-.13	-.10	-.00	-.02	+.06	+.05
Austria .....	-.06	-.13	-.12	+.01	-.02	+.13	+.16
USSR .....	-.16	-.23	-.22	+.13	+.08	+.21	+.15
Poland .....	+.10	-.00	+.06	+.04	+.03	+.20	+.17
Czechoslovakia .....	+.06	-.06	-.04	-.01	-.02	+.03	+.13
Hungary .....	+.04	-.07	-.12	+.11	-.07	+.12	+.17
Italy .....	-.04	-.17	-.13	+.18	+.16	+.20	+.22
Mexico .....	...	+.22	+.19	-.11	+.13	+.23	+.08
Puerto Rico .....	...	+.56	+.63	+.64	+.55	+.19	-.02
Negro .....	+.61	+.34	+.15	+.46	+.56	+.75	+.72
Other nonwhite .....	+.39	+.17	+.13	+.49	+.38	+.51	+.38
Correlation of foreign-							
stock centralization							
with status for:							
Income .....	...	...	-.81	...	-.06	...	+.38
Occupation .....	...	...	-.19	...	-.07	...	+.14
Education .....	...	...	-.46	...	-.04	...	-.05

NOTE.—Income = percentage of families in each foreign stock group who earned \$15,000 or more per year; occupation = percentage of males in professional and managerial occupations; education = percentage of persons, age 25 or older, who had attended college.

the total population by the same criterion. The index may vary from  $+1.00$  to  $-1.00$ , or from extreme centralization to extreme decentralization in relation to the total population. An index of zero would indicate a distribution by distance from the CBD equivalent to that of the total population. Distance was measured by linear miles from the CBD, using census-tract maps. We inspected the distribution of ethnic groups by one-mile zones from each CBD but could find few patterns not adequately measured by the index of centralization.

As table 7 illustrates, on the whole, most ethnic groups in the three metropolitan areas show little centralization or decentralization in relation to the total population. We also present the centralization indices for non-foreign-stock ethnic groups; they are generally much higher than for the foreign-stock ethnic groups. When the indices of centralization for the latter are correlated with their status levels in 1970, we find a clear relationship between decentralization and status only for Cleveland, consistent with knowledge that status varies most widely with distance from the CBD in this metropolitan area. The negative relationship between the index of centralization and status indicates that groups with the greatest centralization have the lowest status. Income is the best predictor of location for the ethnic group. Boston, with less general decentralization of persons with higher status, exhibits virtually no relationship in 1970 between decentralization and status of the group. Finally, in Seattle higher income is actually related to centralization of ethnic groups, while occupation and education have little relationship to location.

Since measures of status at various points in time are unavailable, it is impossible to determine how changing status is related to changing location in the three SMSAs. The data for Cleveland, particularly from 1930 to 1960, generally indicate that all ethnic groups were decentralizing slightly in relation to the total population, although the changes are indeed slight when one considers the length of the period. These results are compatible with our finding that both persons with higher status and ethnic groups were decentralized in Cleveland. They are also consistent with some recent research (Guest 1974) suggesting at least slight decentralization of persons with higher status in Cleveland from 1940 to 1970. As status has risen, the ethnic groups have moved slightly outward relative to the total population. Turning to changes between 1960 and 1970 in Seattle, Boston, and Cleveland, we have some trouble making clear-cut conclusions owing to the short time span; yet on the whole, it seems clear that there is little particular pattern of centralization or decentralization for ethnic groups. Most groups seem to have relatively constant location in relation to the total population, a finding consistent with the relatively stable degree of ethnic segregation in these three places during the same period.

## SUMMARY AND DISCUSSION

The continued existence of ethnic segregation in American cities is clear from these results. There is no evidence that it will disappear in the near future. In particular, its constancy in the 1960s, with slight increases among some groups, supports the idea of continued ethnic ties in American cities. Given the alleged rise of ethnic and racial consciousness in the past few years (Marden and Meyer 1973, pp. 1-4; Segal 1972, p. 91), this finding is hardly a surprise.

Ethnic communities may continue to serve two principal functions in cities several decades after the great waves of migration ended. First, ethnic groups may serve as political-interest groups in the metropolis, particularly since strictly class groups do not seem well developed in American society. An excellent example, according to Glazer and Moynihan (1970), is the recent turmoil in the New York public school system, where political conflict developed between the Jewish-dominated teachers union and black- and Puerto Rican-dominated parents' groups. A second function of ethnic groups is suggested by Louis Wirth's theory (1938) in "Urbanism as a Way of Life." Metropolitan areas—owing to their size, density, and heterogeneity—are often potentially, if not always actually, alienating and anomic. The organization of associational and residential ties on the basis of ethnicity may counterbalance some of the less agreeable aspects of urban life. A good general review of these and other issues concerning ethnicity has been provided by Greeley (1969).

Nevertheless, we do not want to overemphasize the role of ethnicity in residential segregation independent of social-structural characteristics of the groups. Declines in ethnic segregation have been occurring, particularly for "new" European immigrant groups in the United States; many of them were sizable for specific groups in Cleveland between 1930 and 1960. Furthermore, our results seem consistent also with a view that much of the existing ethnic segregation by residence is a function of group social status differences and that, by implication, future changes in relative group status among ethnic groups should affect the degree of ethnic segregation between them. When status differences are considered in conjunction with differences in age, mother tongue, and generation in the United States, our results suggest that most residential segregation by ethnicity may be statistically explained. We have also found that the relationship between distance from the CBD and status of the ethnic group in American cities is by no means uniform, but the small sample of three metropolitan areas suggests that the relationship is closely tied to the general tendency of status to increase with distance from the CBD, regardless of ethnicity.

Unfortunately, we have not been able to provide a statistical explanation



of changes in the degree of segregation between 1960 and 1970 for the three metropolitan areas. Such an analysis would be important, because segregation tended to remain constant or increase in some instances, in contrast to some of the patterns found for Cleveland between 1930 and 1960. Since the data for such an analysis are unavailable, we can provide no satisfactory explanation of this period. One possible interpretation is that ethnic communities have begun to stabilize in American society. That is, several generations have now passed since the great waves of foreign migration ended, and ethnic groups have clearly made the initial adjustment to American society. The 1960 and 1970 data may suggest more or less minimal values of ethnic segregation in American cities which will persist for several generations. Our 1970 data suggest that social status differences among European ethnic groups are far from having ended. Together with the continued social function of ethnicity in the urban community (suggested above), these status differences may serve as the foundations for a pattern of ethnic residential segregation which will continue for some time at roughly 1960 and 1970 values.

We close this paper believing that a modified defense of the Chicago School is justified. The view developed in the 1920s by Park and Burgess is still important for understanding the nature of residential segregation, although, as with most great ideas, the clarity and simplicity of the perspective become somewhat muddy when data are closely analyzed.

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## Simmel's Influence on American Sociology. II

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This article constitutes the second part of a discussion of the diffusion of Simmel's thought within the American sociological community initiated in the January issue of this *Journal*. His influence is traced with respect to the metropolitan mentality, small groups, interpersonal knowledge, conflict, and exchange. Different levels and historical functions of the appropriation of Simmel's work are identified in conclusion.

The first part of this paper discussed the agents and media through which knowledge of Simmel's sociology was diffused to the United States; the impact of that knowledge on the development of general theoretical orientations by American sociologists throughout the 20th century; and the two substantive traditions, those on the social psychology of the stranger and on social distance, stimulated by Simmel's seminal excursus "The Stranger" (1971, pp. 143-49). The present article analyzes Simmel's influence on five substantive areas of sociological enterprise—urbanism, small groups, interpersonal knowledge, conflict, and exchange—and concludes with some observations on the general configuration of Simmel's influence on the development of American sociology.

### THE METROPOLITAN MENTALITY

Simmel's essay, "The Metropolis and Mental Life" (1971, pp. 324-39), enjoys the same prestige today as his excursus on the stranger. However, while the latter has inspired research in nearly every conceivable genre, including cross-cultural comparison, field observation, historical reconstruction, laboratory experiment, mathematical model building, and survey research, the former can be linked with but a very few studies in the vast literature on urban life (in part, no doubt, because a complete translation of the metropolis essay was not readily available until 1950).<sup>1</sup>

Howard Woolston, who had studied with Simmel in Berlin, produced the first of these studies. In "The Urban Habit of Mind," Woolston (1912), drawing on Simmel more copiously than might be apparent from the single footnote, discussed many of the effects Simmel attributed to the metro-

<sup>1</sup> This was the translation by Gerth and Mills published in Simmel (1950). The translation by Shils was published later in Simmel (1971). As noted in Part I, an abridged translation of the essay appeared in a book coedited by Sorokin in 1930.

politan milieu: nervous hyperstimulation, accelerated mental activity, blasé attitudes, measurement of space and time, monetization of values, impersonal social relations, and the proliferation of cultural facilities. Largely ignoring Simmel's stress on the individuating aspects of the metropolis, however, Woolston instead emphasized the new possibilities for social assimilation in cities and the development of an orientation toward civic patriotism.

In Park's more celebrated essay on the city, published in 1915, Simmel was not mentioned. Nevertheless, in the long bibliographical essay appended to the volume containing a revised version of Park's essay, Louis Wirth (1925) wrote that Simmel's was "the most important single article on the city from the sociological standpoint" (p. 219), and drew on it freely a dozen years later (1938) in his own "Urbanism as a Way of Life."

Wirth's essay in turn became a principal point of reference for sociologists concerned with the effects of living in cities. In an extensive review of the relevant literature, Claude S. Fischer (1972) asserts that Wirth "provided the most comprehensive and seminal" statement on the subject. In Fischer's view: "[Wirth's] theory contains basically two arguments: a sociological one, quite Durkheimian; and a social psychological one derived from Simmel. First, on the structural level, population size, density, and heterogeneity (Wirth's definition of the city) lead sequentially to structural differentiation, formalization of institutions, and anomie. Second, on the individual level, urbanism (in the demographic sense) leads to high levels of nervous stimulation, psychological overload, and adaptation in the form of social isolation" (p. 189). Through careful inspection of a number of surveys and ethnographic studies as well as his own large-scale investigation, Fischer (1973) has raised considerable doubt about the validity of the thesis advanced by Wirth and others that isolation and social alienation become particularly acute in large cities.

Although Fischer has brought the analysis of the substantive questions involved to an unprecedented level of sophistication, his account must be corrected with respect to the complex issue of the filiation of ideas which concerns us here. First, his division between Durkheimian and Simmelian components of Wirth's theory is not quite accurate. Simmel clearly regarded group size as a major determinant of variations in the structure of social relations, in the metropolis essay as well as in many other places. Wirth himself cites Simmel to make his point that "increase in number thus involves a changed character of the social relationships" (1938, p. 11). Second, although Wirth did adapt the concept of urban anomie from Durkheim, he did not find the related concept of urban social isolation in Simmel. Simmel did not depict the metropolis as a source of social isolation so much as he emphasized the new forms of association it made possible. Third, insofar as Wirth is understood to be following Simmel's social

psychology of the city, it must be noted that he has incorporated Simmel's views somewhat selectively, for he failed to articulate one of the central features of Simmel's conception: the metropolitan environment provides unparalleled opportunities for personal freedom and self development.

Wirth did incorporate what was perhaps the most original and distinctive aspect of Simmel's essay on the metropolis: the proposition that the great number of stimuli bombarding the residents of large cities produce such typical responses as aloofness, aversion, and superficiality in personal relations. This thesis has recently been elaborated by Stanley Milgram (1970), who recast Simmel's point in the language of systems analysis. Milgram analyzes the features of the metropolitan mentality noted by Simmel as adaptive responses to system "overloading" and further delineates a variety of such typical urban responses, including a restricted sense of social responsibility and lowered expectations regarding traditional courtesies.

Another feature of the urban milieu treated by Simmel was the shift from associations based on kinship and propinquity to those based on special interests, a phenomenon discussed at some length in his chapter "The Intersection of Social Circles" (translated somewhat inaccurately by Bendix as "The Web of Group-Affiliations" [Simmel 1955]). Charles Kadushin has formalized parts of that chapter by conceptualizing the social circle as a social unit characterized by indirect interaction, orientation to common interests, and a relatively low degree of institutionalization and formal leadership. In "The Friends and Supporters of Psychotherapy" (1966) he presents data on one metropolitan circle and suggests that many other circles and sets would similarly confirm such Simmelian propositions as (1) social circles based on common interests tend to replace kinship and neighborhood forms of association in the modern metropolis and (2) individuals in modern societies tend to belong to a number of different circles. Indeed, Kadushin speculates that "what seemed to Simmel a European metropolitan phenomenon may have become a characteristic form of world organization" (p. 802).

#### SMALL GROUPS

Less elusive than circles and much more extensively studied are those social entities based on face-to-face interaction investigated in what has generally become known as "small-group research." To document Simmel's influence in this field is no simple matter. The field has antecedents in a number of disciplines which for years remained out of touch. The relatively independent development of knowledge about small groups by practitioners of group psychotherapy, industrial management, social casework, social psychology, and sociology engendered a great deal of diversity in the accepted

definitions of the subject matter and the theoretical assumptions underlying small-group research.

Within social psychology, the foundations of modern small-group research were laid in the 1930s by Jacob L. Moreno and Kurt Lewin, two Central European immigrants on whose thinking Simmel may have had more influence than is generally realized. Moreno, whose sociograms afforded the first graphic realization of Simmel's call for a geometry of social relations, at least acknowledged some indebtedness by citing Simmel as the first sociologist to have theorized about interpersonal relations and to have conceptualized certain aspects of sociometry. Comparable indebtedness is harder to determine in the case of Lewin, who characteristically cited only the experimental findings of other scholars, never any sources for his general ideas. That Lewin may have been influenced by Simmel is suggested, however, by the fact that Lewin took courses in philosophy at the University of Berlin between 1910 and 1914, shortly after the publication of *Soziologie* (Simmel 1908) and at a time when Simmel's popularity as a lecturer in philosophy was considerable, as well as by the point-for-point parallelism between the basic assumptions of Lewin's field theory and Simmel's sociology, a similarity noted in independent analyses by Herbert Bonner (1949) and Theodore Caplow (1968). In addition, Lewin's close associate and translator, Fritz Heider, was well aware of Simmel's work and cited it regularly.

Beyond what may be surmised as the probable influence of Simmel on the key figures of Moreno and Lewin, we may state more generally: although notable beginnings in small-group analysis were made early in the history of sociology, by Comte, LePlay, Le Bon, Durkheim, Cooley, and others, of the sociological writings before 1930 only Simmel's are cited today with any frequency in the literature on small groups.<sup>2</sup>

Simmel's direct impact on this field has been most visible in research on the significance of group size, especially that concerned with the properties of dyads and triads. In an *American Sociological Review* paper, Howard Becker and Ruth Hill Useem (1942) sought to formalize the sociological analysis of the dyad. Their article was essentially a faithful recapitulation of the main points of Simmel's argument about dyads, spiced with glosses from the psychotherapeutic writings of Moreno and supplemented by a rough classification of dyads adapted from von Wiese and Becker (1932), one hinging on the distinction between segmentalized and comprehensive, or diffuse, relationships. Nearly two decades later, Edgar Borgatta and Robert Guerrin (1960) wrote that the literature devoting systematic attention to two-person groups was limited to Georg Simmel and the work just

<sup>2</sup> See Eister 1957 and Strodbeck and Hare 1954. Of the literature on small groups Strodbeck and Hare found that Simmel's was the only work prior to the 1930s rated by five or more experts as having made important contributions to the field.

cited. Borgatta and Guerrin then reported on an experimental study of 15 two-person groups, concluding that agreement or disagreement on the discussion topic had no appreciable impact on the amount of interaction, that developmental trends were similar to those previously reported for larger groups, and that the amount of interaction within the dyad was not affected by the variable of prior high or low interaction rates of individual members.

Other studies, however, had utilized Simmel's conception of the dyad for more limited purposes. Fred L. Strodbeck (1951), for example, applying the technique of attaining agreement over revealed differences among group members to husband-wife interaction, found that the spouse who talked most tended to *ask questions* more frequently, and interpreted this unexpected finding as indicative of a mechanism for preventing withdrawal: "As Simmel suggested, in a dyad there can be no coalitions—the speaker does not have alternative audiences, so the 'threat of withdrawal' is generally a more compelling adjustment device in two-person than in larger groups" (p. 471). More recently, Murray Davis (1973) has substantially advanced the systematic study of two-person groups in a work of fine-grained naturalistic observation and Simmelian interpretation portraying the typical ways in which intimate dyadic relationships are initiated, built up, sustained, and terminated.

The line of inquiry stimulated by Simmel's analysis of the triad has been more prolific. In a pioneering study carried out at the Harvard Laboratory of Social Relations, Theodore M. Mills (1953) found that his data on the interaction patterns of 48 three-person groups "confirm Simmel's basic point that the threesome tends to break up into a pair and another party" (p. 356). Other propositions supported by his data were that the development most threatening to the position of any member in the threesome is the solidarity between the other two and that the condition most conducive to the intensification of a solidary bond, once formed, is the presence of a common object of opposition. Mills urged that these hypotheses be treated with data from other types of triads—a challenge shortly taken up by Strodbeck (1954), who analyzed interactions over revealed differences among father-mother-adolescent son triads observed in home settings. He found that a tendency to divide into a pair and another party was *not* exhibited in the familial groups.

Strodbeck's conclusion is misleading insofar as it suggests that coalition formation within families is not significant: the tendency of family members to pair off against third parties has been widely reported, most notably with reference to the Oedipal triangle. However, what is more important to note here is that Mills's initial hypothesis diverges in certain respects from Simmel's conception of triads. As Theodore Caplow (1956) has correctly indicated, Simmel's discussion concerned *Verbindungen zu dreien*,

associations of three parties, which are much more loosely organized than the structured and integrated groups connoted by the term "triads." Simmel did not consider segregation into a pair and an isolate the dominant developmental tendency of such threesomes. Most of his analysis in fact concerns the ways in which a third party brings the other two antagonistic members together or separates them when either maneuver is in his own interest. Finally, Simmel was more concerned with articulating the structural properties of different kinds of triadic formations. This concern has been followed up in a series of studies by Caplow (1956), who distinguished different types of triads on the basis of differential power distributions of the members and with respect to group goals and the duration and stability of the association.

Caplow's work is more faithful to Simmel in another respect. Whereas most of the recent research on triads has been concerned with three-person groups, Caplow has conceived the triad, as did Simmel, as a set of three *elements*, and has extended his analysis of the types and properties of triadic formations to include homologous structures within complex organizations, parliamentary bodies, and international relations. In later work, moreover, Mills (1958) also returned to Simmel's formulations for additional ideas concerning the properties of small groups. He has formulated some 15 hypotheses on the comparative dynamics of dyads and triads derived from Simmel and presents a suggestive analysis of different modalities of group play forms based on Simmel's essay on sociability. He concludes by observing that Simmel "invented a way—his formal-model—for dealing with the interrelations of parts of complex systems" and argues that sociological analysis will attain greater strength if it combines theories based on analytic variables with "configurational models on the order visualized but never fully developed by Simmel" (p. 650).

The scientific study of small groups has been greatly advanced in the past generation by the creation of new technologies for observation and analysis of face-to-face interaction. Caplow's words testify to the continuing relevance of Simmel's work to the frontiers of research in this field: "Triad theory is an example of an 'area' discovered and so grandly surveyed by Simmel that each new increment of information from empirical research raises problems already identified and half-resolved in his essays" (1968, p. 14).

#### INTERPERSONAL KNOWLEDGE

That persons in interaction necessarily know something but not everything about one another was a central theorem of Simmel's sociology. He treated it from two points of view. First, respecting A's knowledge of B, Simmel stressed, in the excursus "How Is Society Possible?" (1971, pp. 6–22), that



for human interaction to take place actors must possess some stereotyped images of one another. Second, respecting the information about himself which A gives to B, he discussed the forms, conditions, and consequences of different degrees of self-revelation represented by the phenomena of secrecy, privacy, and intimacy.

The first of these themes was elaborated with great skill by Gustav Ichheiser. Noting that "the tendency to perceive and evaluate other people not as individuals but as specimens of a social type . . . , according to Simmel, is one of the absolutely essential preconditions of the existence of any society," he sought, in a series of papers assembled posthumously as *Appearances and Realities* (1970, esp. pp. 60-77), to utilize that insight as a basis for systematic delineation of the misinterpretations and misunderstandings that occur in all human relations. Among Ichheiser's many contributions we may mention his identification of the "mote-beam mechanism"—perceiving certain characteristics in others which we do not perceive in ourselves and thus perceiving those characteristics as if they were peculiar traits of the others" (p. 92)—and his uncompromising argument against conventional views about stereotypes. In the latter he rejects the view that prejudices are peculiar to certain kinds of people, wholly false, and a main cause of intergroup tensions, in favor of a conception that prejudices are universal, based on a combination of truth and falsehood, and defensive rather than aggressive mechanisms.

In contrast to the phenomenon of stereotyping, which stems from the inexorable limitations of interhuman perception, Simmel discussed secrecy and secret societies as phenomena entailing the deliberate concealment of information about self from others. H. B. Hawthorn (1956) was perhaps the first to seek some empirical validation of Simmel's numerous generalizations about secret societies. Drawing on material from only a single case, the Sons of Freedom sect of the Doukhobors of British Columbia, Hawthorn used those data to confirm or disconfirm about a dozen of the relationships postulated by Simmel. In addition, the Doukhobor data led Hawthorn to formulate new hypotheses about secret societies concerning their peculiar effects on the personalities of second-generation members, the special opportunities they afford for exploitation of members, and the circumstances which may lead to defection and disorganization.

In some of his proposed revisions of Simmel's hypotheses, however, Hawthorn demonstrably misunderstood what Simmel had written. His errors are stressed in an article by Lawrence E. Hazelrigg (1969). From such misreading Hazelrigg concluded that more rigorous work is needed to test Simmel's ideas, work involving the deliberate construction of well-formulated propositions based on Simmel's discursive essay. He himself presents nine such propositions which formalize Simmel's ideas concerning the formation and structural properties of secret societies. For example,

"The greater the value of the ideas, objects, activities, or sentiments that constitute the focus of secrecy, the greater the tendency of the secret society toward total inclusion of its members' activities, sentiments, ideas, and objects, and the greater the members' isolation from other interactional units. . . . The greater the tendency toward total inclusion, the more likely the members possess aristocratic self-conceptions" (p. 328).

Hazelrigg also discusses the methodology needed to verify these propositions and points out in a note that, because of the fears of intrusion and discovery on the part of members of secret societies, the preferred empirical route would combine techniques of direct observation and archival data analysis.

Stanford M. Lyman (1964) also has modified one of Simmel's propositions about secret societies, the suggestion that they conceal their rituals from outsiders because total secrecy protects their actual purposes from discovery. In connection with a comparison of Chinese and Masonic secret societies, Lyman develops a typology of secret societies (conformative/alienative; instrumental/expressive) which enables him to demonstrate that the purposes served by rituals and ritualistic secrecy vary significantly with the type of society.

In addition to stimulating efforts at formalization and verification, Simmel's ideas on secrecy have been used to throw light on other kinds of phenomena. Thus, in a speculative article, "The Secret Ranking," Hans Zetterberg (1966) joins Simmel's ideas on secrecy with ideas about status hierarchies to illuminate the phenomenon of "erotic ranking," which he defines as the secretly kept probability that one can induce in persons of the opposite sex a state of being emotionally overcome. Erotic hierarchies are considered salient among teenagers, in coeducational settings such as hospitals and offices, and in occupational communities such as filmdom, advertising, publishing, and banking. The societal devices to protect secrets which Simmel described are presumed operative in maintaining the secrecy of these erotic rank orders.

Simmel placed secrecy at one end of a continuum of reciprocal disclosure, the other end being the state of complete disclosure represented by intimacy. In the middle of the continuum are privacy, discretion, and reserve, topics which have been explored further by a number of other sociologists. Much of the work of Erving Goffman is grounded on Simmel's notion that persons are surrounded by a variable sphere of traits, possessions, and sentiments, which constitutes their private domain. To Simmel's concept of deference, the expression of respect for this sphere of others, Goffman (1967) added the complementary concept of demeanor, the symbolic means by which persons elicit deference from others. Whereas deference images relate to the place an individual has reached in a societal hierarchy, demeanor images reflect qualities which social positions give their incumbents

to display and which are utilized differentially according to the varying styles of incumbents. Goffman (1959, 1961, 1963) has dissected the numerous ways in which individuals act to preserve their private spheres in a variety of situations, including the minimal intrusiveness found in everyday life, the moderate intrusiveness represented by behavior in public places, and the maximal intrusiveness found in total institutions such as prisons and mental hospitals.

The social as well as the psychological consequences of privacy and deprivation are considered in a wide-ranging essay by Barry Schwartz (1968). Schwartz makes particularly effective use of Simmel's notions about boundaries, withdrawal, and disaffiliation as the necessary counterparts of association, drawing on the formulations of "Brücke und Tür" (Simmel 1957) as well as of *Soziologie*. He sees guarantees of privacy as essential conditions of stable interaction systems. The social consequences of privacy include the affirmations of status divisions and the permission of localized deviation in a way that does not disrupt a group.

Discussing the problematic status of privacy in the modern democratic state, Alan Westin (1967) draws on Simmel's realization that privacy is a function of two variables, the individual's own display of reserve and the use of discretion by others. Elaborating Simmel's point that the person who reveals too much of himself undermines many relationships and eliminates a private sphere, Westin argues that if many individuals abandon their reserve, the exercise of discretion by others will inevitably be weakened: "Those who tell all prompt others to ask all" (p. 53). This argument forms the foundation for Westin's analysis of modern surveillance techniques and other intrusions on privacy.

Close personal relationships such as friendship and marriage raise for Simmel and many others the question of the extent to which mutual disclosure can and should be carried. In dealing with this question, Suzanne B. Kurth (1970) refers to Simmel's conjecture that the complete intimacy often associated with friendship in the past probably becomes increasingly difficult as differentiation among persons in modern society increases. She uses this point to develop the concept of the "friendly relation," a pattern contrasting with the more diffuse, unique, and intimate relation of friendship. Kurth argues that individuals prefer limited friendly relations to the deeper disclosures and demands of true friendships. Mirra Komarovsky (1962, p. 143) deals with a related problem in her analysis of blue-collar marriage. She finds among working-class couples (though not among middle- and upper-class ones) a relationship described by Simmel: reserve may contribute to marital solidarity because, in Simmel's words, a couple may "belong more to one another qualitatively if quantitatively they do so less" (1950, p. 327). Clearly a good deal of contemporary research con-

cerning intimacy and reserve in personal relationships deals with problems adumbrated by Simmel, though frequently without specific reference to him.

## CONFLICT

The keen interest in social conflict displayed by such pioneers of American sociology as Small, Park, and Ross was replaced in later generations by a focus on phenomena of personal adjustment and societal consensus. The shift in orientation was so striking that Jessie Bernard ruefully observed that "American sociologists in recent years have been content to leave the scientific study of conflict where Simmel left it" (1950, p. 11).

In a determined effort to reverse that change, Lewis A. Coser published *The Functions of Social Conflict* (1956), a discursive propositional inventory based on Simmel's chapter on conflict in *Soziologie*.<sup>3</sup>

Renewal of scientific interest in social conflict was further advanced the following year by James S. Coleman's *Community Conflict* (1957), a work which, guided by Simmel, developed a set of generalizations on the initiation, dynamics, and developmental patterns of conflicts in American towns based on an extensive examination of case studies.

Coser enhanced the theoretical value of Simmel's ideas about conflict by casting them in the form of discrete, clearly formulated propositions; refining them through logical scrutiny and comparison with relevant material from psychoanalysis, psychology, and social psychology; and showing how they could be qualified by the interposition of intervening variables. For example, Simmel had stated that conflict furthers group persistence by making life with unbearable associates tolerable. Coser introduced the variable of system rigidity to distinguish between the direct expression of aggression toward an offending object and the use of safety-valve institutions to channel antagonistic feelings toward substitute objects; in the latter instance, group persistence might be threatened because reduced pressure for modifying the system to meet changing conditions might create the potential for disruptive explosion. To use another example, Simmel had argued that conflict among group members promotes group unity by resolving divergent currents of opinion. Coser, considering the type of issue on which conflict centers, suggested that conflict concerning the basic assumptions of the group is likely to have disunifying consequences. The latter point has recently been challenged on ideological grounds by William Newman (1973, esp. pp. 118-28) and supported through empirical work by Ralph Turner (1972), who analyzed college

<sup>3</sup> Several other papers on social conflict were published subsequently by Coser (1967).

newspaper coverage of campus violence in the 1960s and found that perceived threats to the basic campus consensus motivated students to rally to assert solidarity and suppress dissent.

While these and some of the other propositions codified by Coser concerned the conditions and consequences of conflict within groups, most of them concerned the conditions and consequences of conflict between groups. Simmel's contention that external conflict enhances intragroup cohesion has been called "the most recurrent explicit proposition" of what Levine and Campbell (1972, p. 31), in a survey of the social science literature on conflict and ethnocentrism, refer to as "Realistic Group Conflict Theory." Coser sought to qualify the proposition by considering the character of the group and the substance of the conflict as intervening variables: if a group is a "going concern" and the conflict appears to threaten the whole group, opposition promotes cohesion and group mobilization. Coleman's study outlines some of the dynamics involved: in-group cohesion reflects a process of getting rid of all social encumbrances which impede the action needed to win a conflict and is accompanied by other organizational changes, such as the formation of partisan associations and the emergence of new leadership.

Another aspect of this process noted by Simmel and Coser is the crystallization of group boundaries and identity through intergroup conflict. Kai Erikson (1962) turns the point inward to show that deviance performs a similar boundary-defining and boundary-maintaining function for a society. Joseph S. Himes (1966) extends the point further: racial conflict necessitates a kind of intragroup cohesion that enhances group and personal identity, which in turn promotes more effective participation in the larger societal system.

Simmel's point that conflict-generated group cohesion tends to increase the repression of deviance was elaborated by Coser, who specifies size, degree of personal involvement of members, and frequency of external conflict as the variables determining the extent to which groups repress internal dissent. Raymond Tanter (1966) purports to test this hypothesis in his study "Dimensions of Conflict Behavior Within and Between Nations, 1958-1960." Finding that internal and external conflict vary independently, Tanter concludes that Simmel's statements do not apply to macrosociological phenomena. It was, however, precisely Simmel's point, sharpened by Coser, that larger, internally differentiated groups can afford to be relatively tolerant even when engaged in occasional struggles with outside groups.

With respect to the affective bonding between groups in conflict, Coser explicates Simmel's point that hostile impulses do not suffice to account for social conflict and that the hostility drive is frequently stimulated by

and conveniently added to the interaction of parties in conflict. Coleman continues the discussion:

Simmel says that it is "expedient" and "appropriate" to hate one's opponent just as it is "appropriate" to like someone who agrees with you. But perhaps there is a stronger explanation: we associate with every person we know certain beliefs, interests, traits, attributes, etc. So long as we disagree with only one or a few of his beliefs, we are "divided" in our feelings toward him. . . . But when we quarrel, the process of argument itself generates new issues; we disagree with more and more of our opponent's beliefs. Since these beliefs constitute *him* in our eyes, rather than isolated aspects of him . . . our hostility is directed toward him personally. [1957, p. 11]

Coleman thus delineates a developmental process in which continued controversy generates a wider spectrum of controversial issues, turning disagreement into antagonism, a process which promotes recruitment into conflict groups through reciprocal vilification of enemies. This transition can be mitigated through mediation by third parties and by crosscutting personal loyalties, phenomena treated elsewhere in *Soziologie* by Simmel and deftly integrated into the analysis of conflict by Coser and Coleman. Coleman further notes that the process of mutual vilification leaves scars on a community, creating lasting cleavages and increasing the likelihood of future conflict. This finding indicates the importance of supplementing the largely ahistorical propositions about conflict in Simmel and Coser with a diachronic perspective.

Coser (1956) likewise amplifies Simmel's assertions that conflict between groups establishes relations where none existed before and that conflict typically generates or activates rules to govern and restrain the conduct of hostilities (points which had been central in Park's theory of social interaction). In the process, he challenges an earlier interpretation of Simmel's argument on this matter by George Simpson (1937), who held that what binds the parties is not the conflict as such, but their shared values. In Coser's view, "Simmel shows that the binding values or norms are brought into awareness through conflict so that conflict, far from being only incidental to an affirmation of common values, is an agency through which these values come to be affirmed" (1956, pp. 127-28, 178). Applying these ideas to black-white relations in the United States, Himes (1966) shows that conflict strengthened interracial relationships by requiring the construction of new modes of communication between racial groups if their conflicts were to be resolved and by reaffirming the more inclusive value orientations concerning equality under law, as well as by creating power resources enabling the previously subordinate racial minority to participate more fully in the larger society.

Finally, we might mention arguments stimulated by Simmel's proposition that the most effective prerequisite for preventing struggle, exact knowledge of the comparative strength of two parties, is often attainable only by fighting out the conflict. Both Coser (1963) and Ritchie Lowry (1972) have deduced from this premise a policy recommendation: international conflict might be forestalled by eliminating secrecy concerning military strength in international relations.

Although Coser's codification of the function of social conflict has been considered a significant contribution, certain dangers of relying on it uncritically as a substitute for Simmel's text have also been noted. One of these has been signaled by Coser himself in his notice that the propositions discussed in the book do not exhaust the content of Simmel's essay on conflict. A second is that Coser's translation of Simmel's ideas into the idiom of functional analysis somewhat beclouds Simmel's intention to present a straightforward analysis of the process of conflict and the structural implications of conflictual relationships. This point has been sharply made in a critique of Coser by two anthropologists, Alan Beals and Bernard Siegel (1966): "Any given conflict has functional and dysfunctional aspects; nothing is gained by debating whether or not conflict in the abstract is functional" (p. 24).

Other difficulties are created, moreover, by Coser's distinction between "realistic" (instrumental) and "nonrealistic" (expressive) conflict and his related point that conflict arises wherever there exists an excess of claimants over opportunities for adequate reward. Although at the time of writing Coser did well to emphasize that conflictual interaction is not inherently irrational or pathological, as had often been assumed in the contemporary literature, his concentration on realistic conflict again obscures the important Simmelian premise that conflict is simply one of the basic, universal, and inexorable modes of human interaction. Beals and Siegel also take issue with this aspect of Coser's treatment, citing the argument that "conflict is least likely to occur over the allocation of scarce goods, for the allocation of scarce goods without conflict is considered a principal function of society" (p. 18). Ichheiser similarly rejects the notion that intergroup tensions not based on conflicts of economic interests are not "real" and argues that intergroup tensions originating in psychological or cultural differences are as real as those rooted in conflicts of interest (1970, p. 76). Beals and Siegel further stress that Simmel himself never accepted the notion that conflict inevitably occurs over scarce goods. Observing that *every* action conveys a definite meaning to other actors, reflecting either assistance, opposition, or irrelevance (and that Simmel's discussion of these basic social motives, "as usual, is the most inspired"), Beals and Siegel propose an alternative generic conception of conflict: it "exists when two parties belonging to the same organization exchange behaviors that sym-

bolize opposition" (1966, pp. 16, 18). Proceeding from this conception they develop a typology of forms of conflictual interaction based on the criteria of the amount of disruption, the extent to which the conflict involves organized groups, and the level at which it occurs within an organization. We may question Coser's apparent assumption that "realistic" conflict is generally constructive and "nonrealistic" conflict the chief source of disruption; Simmel referred to cases wherein conflict aimed at "tension release" may strengthen group boundaries and personal identities and hard-fought battles over possessions may engender irremediable divisiveness and bitterness.

In spite of these shortcomings, Coser's work must be acknowledged, as Himes noted, as having "led the revival of sociological attention to the study of social conflict" and thereby "injected the very considerable contributions of the German sociologist Georg Simmel into the stream of American sociological thought" (1966, p. 2). Beyond that, it must be seen as perhaps the most fruitful attempt to recover, modernize, and utilize Simmel's work as a basis for current sociological inquiry.

## EXCHANGE

Another central Simmelian theme came to prominence in American sociology at the end of the 1950s following the publication of George Homans's "Social Behavior as Exchange" (1958). Ironically, although Homans's paper appeared in an *AJS* issue commemorating Simmel, the author revealed no familiarity with Simmel's own statements about exchange, but associated his essay with Simmel simply on the grounds that its tone was "suggestive" and its subject was elementary social behavior. In further work on this topic Homans (1961) sought to account for elementary transactions not in Simmelian terms but with the aid of Skinnerian operant conditioning principles.

Simmel's own ideas on exchange began to surface in the modern sociological literature with Alvin W. Gouldner's paper on the norm of reciprocity (1960). In this closely reasoned exposition, Gouldner focuses on the types of duties people owe one another, not by virtue of occupying certain statuses but because of the history of previous interaction between them. Refining Simmel's proposition that "all contacts among men rest on the schema of giving and returning the equivalence" (1950, p. 387), as well as related material from Parsons, Malinowski, and other social anthropologists, Gouldner delineates the differences between reciprocity and complementarity, specifies how the norm of reciprocity serves both to initiate and stabilize interaction systems, and distinguishes two forms of equivalence, "heteromorphic" and "homeomorphic" reciprocity.

Testing Simmel's notion that the rational habit of mind involved in



economic exchange is generalized to other kinds of social relations in metropolitan settings, Muir and Weinstein (1962) interviewed samples of female respondents in lower- and upper-middle socioeconomic strata to determine their attitudes about social favors. They found that respondents from both strata felt that obligations should be discharged voluntarily if possible and disapproved of "debtors" who did not do so: "Simmel's thesis of metropolitan rationality was thus supported by these attitudes concerning sociable exchange" (p. 537). In addition, they found demonstrable differences between the two social classes; for example, lower-class respondents were unlikely and upper-middle-class respondents likely to cut off "social credit" from people who did not repay favors.

Peter M. Blau's *Exchange and Power in Social Life* (1964) introduced a major new conceptualization of the general theory of social exchange. "In large degree built on Simmel's pioneering observations on the role of gratitude in human interaction, and on the function of exchange in effecting gratitude," as Nisbet (1970, p. 64) has written, Blau's work revitalized (but did not cite) von Wiese and Becker's project (1932) of grounding the analysis of complex societal processes on the properties of microscopic interpersonal transactions. The central process in question is termed social exchange, a transaction contingent on rewarding reactions from others; the main dimension of structural analysis is that of reciprocity and imbalance. Imbalances in exchange are viewed as sources of differences in status and power, which in turn generate legitimate authority—the basis of all large-scale organization—and structures of political opposition. Blau cites Simmel's *Soziologie* (though not the discussion of exchange in *Philosophie des Geldes* [1900]) frequently in this monograph and explicitly stakes out a Simmelian position which he (like Levine earlier [1957]) contrasts with the analytic approach focused on value orientations espoused by Weber and Parsons.

In more circumscribed ways, some other efforts to link exchange theory with macrosociological analysis have also drawn on Simmel. Richard M. Emerson (1972) sought to formalize the properties of exchange relations and exchange networks through a series of definitions, theorems, and corollaries which include a reformulation of the Simmel-Kadushin conception of social circles as exchange networks among persons within the same social category and the proposition that such networks have an inherent tendency to change until they become "closed." Coser (1965) has elaborated Simmel's points about the harmful effects of unreciprocated exchange on those who receive welfare assistance. He suggests that more mutuality in the transactions between the national bureaucracy and the poor would increase the personal and social resources of aid recipients, enabling them to escape the stigma of being on welfare. Carol Andreas (1971) applies the point to the analysis of international aid. Drawing also on Simmel's essay, "The

Poor," she documents the preoccupation and dissatisfactions of editorial writers in the newspapers of India and Pakistan with current aid programs, concluding that the internationalization of aid might serve to mitigate the problem of expected gratitude and repayment from recipients.

Two papers by Barry Schwartz, finally, draw on other aspects of Simmel's work to illuminate the social psychology of exchange. In an essay on the gift, Schwartz (1967) attends chiefly to Simmel's remarks on exchange in the excursus on faithfulness and gratitude, extending them into a discussion of the utility of gift giving as a means of social control and noting "the negative consequences of the norm of reciprocity, which prescribe vengeance, or at least grudge, for harm done, just as their counterparts call for reimbursement and gratitude for benefits received" (p. 9). In a later paper, "Waiting, Exchange, and Power: The Distribution of Time in Social Systems" (1974), Schwartz delves into Simmel's thought on value and exchange as expressed in *Philosophie des Geldes*, relating it to such propositions as that waiting is not simply a barrier to service but the very condition of its subjective value, and that persons can maintain and dramatize their worth by purposely causing others to wait.

## CONCLUSIONS

Simmel's impact on American sociology has been recurrent, variegated, and erratic. Though at no point a predominant influence, his work has set currents in motion in nearly every decade of this century. This has happened at three different levels. Simmel helped to define a *subject matter* for American sociology. Boskoff (1969, p. 59), for example, reviewing the major sources of theory in American sociology and listing six "nuclear problems" as constitutive of the general subject matter of the field, has identified Simmel as a major contributor to five of them—more than any other of the "Old Masters."

As the source of an *analytic perspective*, Simmel's thought has helped to orient those who have sought to develop sociology by attending to the interchange of items among positions in a relational network: of benefits, as in exchange theory; of oppositions or injuries, as in conflict theory; of information and expectations, as in symbolic interactionism; of attachments and enmities, as in sociometry and coalition theories. At the same time, it should be noted that other key aspects of Simmel's analytic orientation have had relatively little influence: in particular his conceptualization of the forms and properties of individuality, of the dualistic nature of social experience, and of the dialectical process by which cultural forms emerge and are transformed.

A third level of influence has been the articulation of a number of novel *topics and propositions*. By many, Simmel is appreciated chiefly for his

provocative formulations concerning the stranger, dyads and triads, secrecy, deference and reserve, faithfulness and gratitude, and conflict—not to mention a number of other topics not touched on in this survey, such as sociability, fashion, and domination.<sup>4</sup>

In saying that Simmel's influence has been erratic, we refer in part to the highly selective and often arbitrary way in which his ideas have been incorporated. Although there have been some efforts to formalize, scrutinize, and test a number of his propositions—certain of the studies discussed above have been exemplary in this respect—in many instances the treatment has proven either naively adulatory, citing bald assertions as though they were verified empirical generalizations, or carelessly distorted, through patent misreading of the text and ignoring relevant parts of the argument.

Furthermore, the Simmelian tradition in American sociology has been extremely disjointed and noncumulative. To be sure, the erratic career of Simmel's thought in American sociology exhibits a fundamental feature of all scientific evolution. As Shils (1957) describes it, "The growth of knowledge is a disorderly movement. It is full of instances of things known and overlooked, unexpected emergencies, and rediscoveries of long-known facts and hypotheses which in the time of their original discovery had no fitting articulation and which found such articulation only after a considerable time" (p. 144).

Regarding Simmel's influence, this general tendency has been accentuated by certain specifiable factors. One is the character of Simmel's work itself: the scatter of topics, the failure to integrate related materials, the paucity of coherent general statements, and the cavalier attitude toward academic tradition. A second factor is the nature of the English translations: made at intervals of decades, scattered in various places, the full range of relevant translations has not been readily accessible. In many instances scholars have erroneously faulted Simmel for neglecting crucial considerations or have omitted references that materially support their own conjectures simply because of ignorance of the relevant texts, including those in translation. Another factor has been the relatively weak institutionalization of rigorous scholarly standards within American sociology. Still another appears to be the difficulty experienced by American sociologists in dealing with nonunivocal assertions: the ambiguities, dualistic conceptions, and dialectical aspects of Simmel's thinking have often been screened out by those trained in American modes of thought, which stress univocality and one-dimensional metrics.

In spite of its erratic character, Simmel's influence on American sociology

<sup>4</sup> On sociability see Riesman, Potter, and Watson (1960); Duncan (1962, chap. 2); and Aldrich (1971). On fashion see the special issue of the *American Journal of Sociology* (vol. 62, no. 6 [May 1957]); and Blumer (1969). On domination see K. J. Newman (1951).

does exhibit some pattern. One pattern, at least, emerges if one looks at the special features of Simmel's work which appeared especially salient to American sociologists during each of the three periods of diffusion described in the opening section of Part I of this paper. It then seems clear that different aspects of Simmel's work appealed to American sociologists in each period, because they related differentially to the intellectual needs of the times.

During the first period of diffusion, when American sociologists hungered for academic respectability and a suitable professional identity, it was Simmel's efforts to provide solid philosophical underpinnings for a science of sociology that made his work so appealing. During the second period, when finer spirits in the profession despaired of the intellectual mediocrity infesting the field, it was Simmel's philosophical depth and creative intellectuality that seemed so important. In the third period, when American sociologists were striving to establish rigorous propositions that could be tested empirically, it was Simmel's achievement as a precocious formulator of sociological generalizations that came to the fore.

It is possible that other aspects of Simmel's work will appear more significant in the future. Of perhaps most lasting interest—beyond his functions as a source of scientific identity, theoretical ingenuity, and substantive generalizations—may be the luxuriantly rich personal knowledge and subtle imagination which inform his investigations. The point was made over 15 years ago, with unmatched elegance, by Robert A. Nisbet (1959): "It is also likely, I think, that less of Simmel's personal genius will be lost in the nameless body of advancing knowledge—the common fate of even great contributions in the history of science—than will be true of Durkheim and Weber. This is merely another way of saying that in Simmel's work there is a larger element of irreducible humanism and that as with Darwin and Freud, for example, it will always be possible to derive something of importance from him directly that cannot be absorbed by the impersonal propositions of science" (p. 481).

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# Fashions in Shaving and Trimming of the Beard: The Men of the *Illustrated London News*, 1842–1972<sup>1</sup>

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This research note presents sample-derived measures of comparative frequencies over time (1842–1972) for changing modes in men's facial barbering. Students of the dynamics of taste have been slow to follow up A. L. Kroeber's pioneering demonstration that shifts in the comparative proportions of women's dress design over time have generally tended to follow alternating directions over long periods which are notably consistent and regular in their recurrence. The remarkable similarity of the chronological patterns emerging from my measurements to those found by Kroeber strongly suggests that they are common expressions of underlying conditions and sequences in social behavior. The hypothesis that stylistic changes are subject to common behavioral influences is reinforced now that the two sets of data are available for comparison.

Almost as if in disregard of the arresting results of A. L. Kroeber's pioneering quantitative study of changing proportional dimensions in women's dress fashions, in which he (with Jane Richardson) found remarkably regular waves of approximately a century's duration, no comparable attempt has been made to trace fluctuations in other forms of ornament. Kroeber measured annual fluctuations in width and length of skirts, waistlines, and décolletage as ratios to women's heights. This report presents the results of a careful sampling of the comparative frequencies over time of men's choices of forms of grooming their facial hair. It does not recapitulate Richardson and Kroeber's article (1940) but points out where the present research replicates their findings. My data show that men are just as subject to fashion's influence as women.

## SOURCE OF DATA AND SAMPLING PROCEDURE

In selecting data, the first desideratum, as for Kroeber, was "ease of obtaining material which is not only fairly abundant but strictly comparable from decade to decade and even from century to century" (Richardson and Kroeber 1940, p. 111). I settled for the period of 1842–1972 on the

<sup>1</sup> My particular debt of gratitude is to Lois Wallace Fenske for her good work in conducting the sample counts. I am grateful to Professor Hirokuni Tamura for helpful discussion. Patricia Hall and Linda Van Kirk assisted in preparing the tables and manuscript. Responsibility is mine alone.



ground that those were the years of continuous weekly publication of the world's most venerable pictorial news magazine, the *Illustrated London News*, the single source of the sample. Although Kroeber's study covered three centuries, his detailed analysis was confined to the last 150 years sampled, a time span not much longer than the 130 years of this study.

The gentlemen of the *News* were, of course, members of a cultural subgroup, mainly British, and nearly all prominent in one way or another in their nation's affairs. This limitation of the sample carried with it compensating advantages. We could be sure that the more crucial socioeconomic characteristics of the subjects, particularly age, occupation, income level, and social status, would remain much the same over the years.

My procedure for gathering data was, quite literally, to take a head count, determining for any one year the comparative frequencies of men's choices among five major features of barbering: sideburns alone, sideburns and moustache in combination, beard (a category that included any amount of whiskers centering on the chin), moustache alone, and clean shavenness. To obtain distinct likenesses, to minimize duplication, and to avoid bias, I excluded pictures of groups (because such pictures often obscure parts of faces or show them at angles), pictures of royalty (because the royal family gets more press coverage than the average newsmaker), pictures illustrating fiction (because fictional time is not necessarily contemporary), pictures in advertisements (because models photographed are obviously selected with special purposes in mind), and pictures of non-Europeans (because individuals of other national origins are often subject to cultural influences unrelated to general Western tonsorial fashion).

I did not exclude likenesses of men in uniform. During war years the overwhelming majority of photographs were of young men in military service. Had I excluded them, I should have run the risk of compiling inversely biased samples. However, because of the strong likelihood that younger men are more inclined to follow the ascendant style and to avoid the outgoing, the statistical effect would naturally be to step up the rate of increase of the former and to accelerate the decline of the latter.

I was able to accumulate a considerably greater number of observations for every year sampled than was Kroeber, who rarely measured more than 10 dress models annually. My aim was to gather a minimum of 100 observations in each year by sampling either a three- or a six-month period. In the earlier years (1842-77), because illustrated likenesses were sparser, the average number of observations per annum was 76. From 1878 on, yearly sample numbers fell below 100 in only two years, 1931 and 1933, with 95 and 97, respectively (see Appendix).<sup>2</sup>

<sup>2</sup>For some years the more zealous compilers exhausted the issues of an entire year. The effect of such erring on the side of generous samples is in all probability to improve the frequency estimates for the years concerned.

## PRELIMINARY ANALYSIS OF THE TIME SERIES

The swings plotted for changing fashions in the disposition of facial hair exhibit as much "stateliness of march" as so impressed Kroeber in the history of dress, if not more (figs. 1, 2). The time series for clean shaving is the most persistently and uniformly regular of all the five categories (fig. 2). It may be noted that the number of clean-shaven men in any year

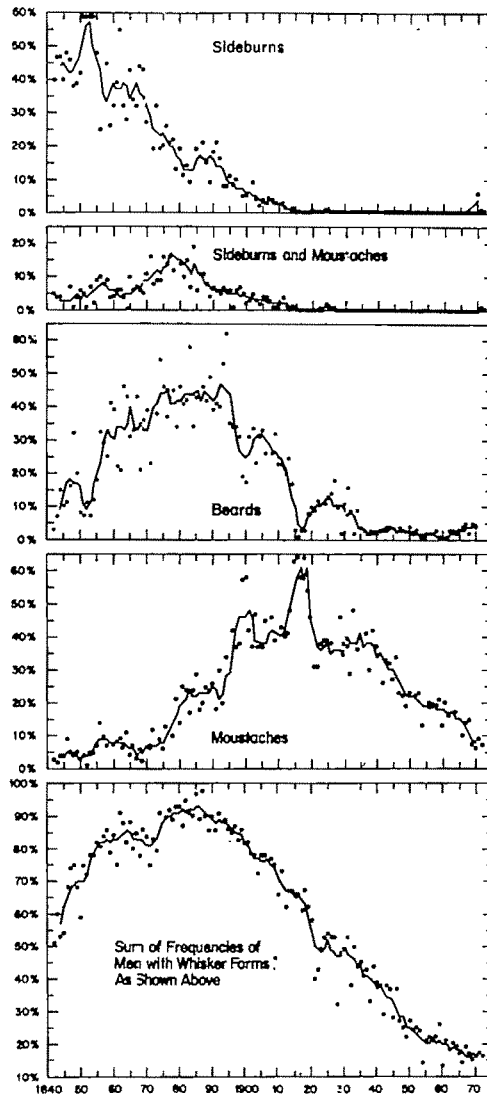


FIG. 1.—Frequencies of whisker forms, 1842-1972. Lines = five-year moving averages; dots = frequencies as percentiles of yearly samples. Source: *Illustrated London News*, 1842-1972.

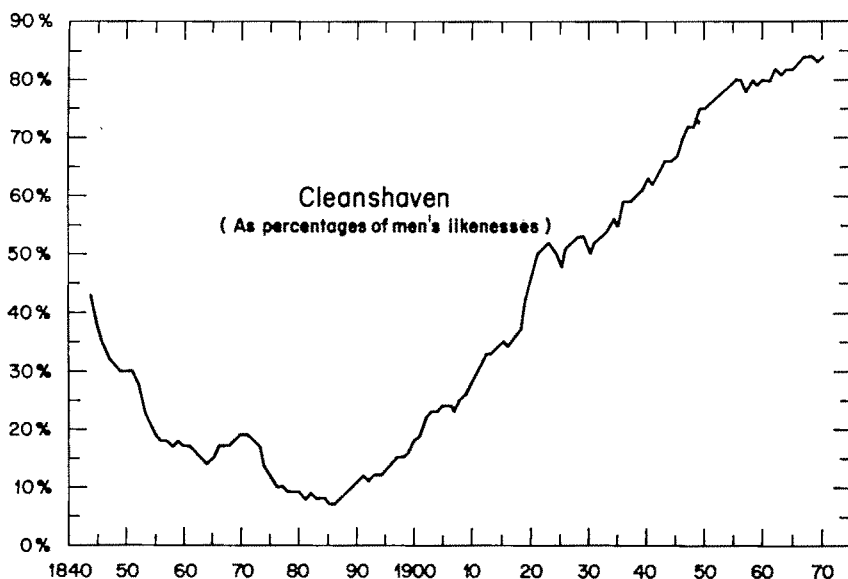


FIG. 2.—Frequencies of cleanshaven likenesses

is by definition the reciprocal of the sum of those in the four whisker categories. The two opposite directional swings in clean shavenness are marked by extreme longevity, as well as by very slight fluctuation in annual rates of change. The playing down, on the one hand, and playing up, on the other, of ornamentation appear to be fundamental to shifting style preferences in successive eras.

The peaks of the several whisker types were as follows (fig. 1): sideburns, 1853; sideburns and moustache, 1877; beards, 1892; moustaches, 1917–19. An interesting consideration that emerges from all the time series, except that for clean shavenness, is that a long period of disappearance or nearly complete dormancy of a style form seems to follow its decline. The patterns of the curves make it clear that the standard procedure of measuring curves from low point to low point is not entirely suitable to the data. However, simply to compare with Kroeber's finding of a mean wave length approximating a century, we may compute trough-to-trough measurements to arrive at wave lengths as follows: sideburns and moustaches (1846–1916), 70 years; beards (1840–1960), 120 years; moustaches (1850–1970), 120 years; with an average for the three of about 103 years.<sup>3</sup>

When we compare Kroeber's width of skirt wave, which rose and fell

<sup>3</sup> Sideburns are omitted because the measurements of this fashion take into account only slightly more than their period of decline (1853–1915). The upward swing in clean shaving, which began in 1886, appeared to peak out only in 1966, which suggests the likelihood of a far longer wave.

between 1811 and 1926, and the beard wave, which rose and fell between 1842 (or very possibly 1840) and 1956, we find one of the most remarkable correspondences in the entire body of time-series measurements (fig. 3). Astonishingly, the respective lengths of upswings are 50 years and 48 or 50. Each wave occupied a period of almost exactly 115 years. With respect to ranges of variation, skirt diameters reached the anatomical minimum of approximately one-fifth of height of figure in the years 1811 and 1926, having peaked at 105% of figure height in 1861. (Five-year moving averages, Richardson and Kroeber [1940], table 8.) Beard frequencies rose and fell from less than 10% in 1844 to zero in 1957, attaining their maximum of 47% in the five years centering on 1892.

I computed the Pearsonian correlation coefficient between the two time series, allowing for different lead-lag intervals because the ups and downs of each series took place at different times, probably a consequence of age differences between fashion models and the prominent men of the *News*. An interval of 21 years added to the actual dates of the skirt width wave

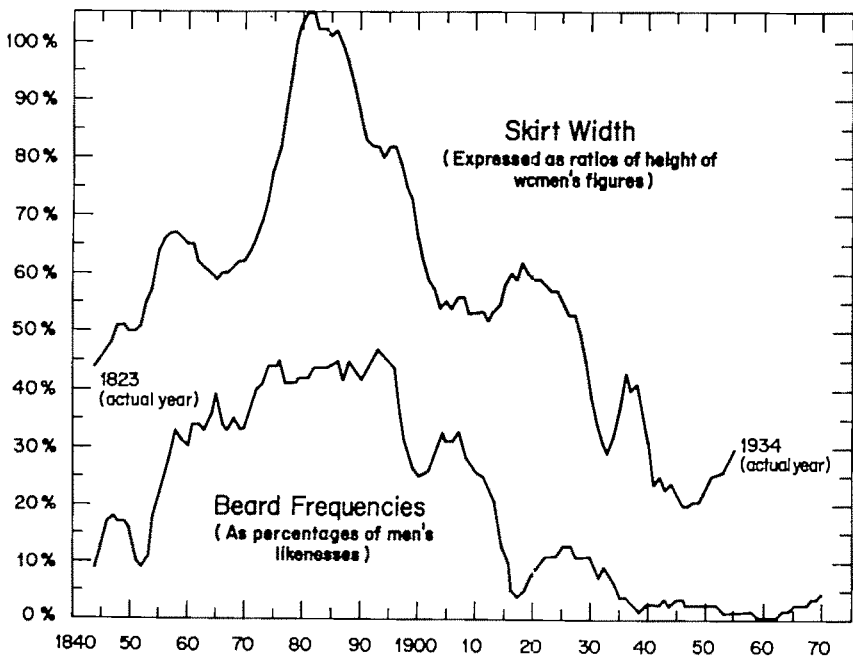


FIG. 3.—Skirt width (1823–1934) and beard frequency fluctuations (1844–1955), five-year moving averages. The time scales of the two curves have been positioned to allow for assumed 21-year lead in skirt fluctuations possibly related to comparative youthfulness of subjects in Richardson and Kroeber's population samples for dress (see text for further explanation). Coefficient of correlations for the two series,  $r = .867$ . Sources: skirt widths (Richardson and Kroeber 1940, table 8); beard frequencies, Appendix.

yielded the highest value (Pearson  $r$  equals .867) of the correlation coefficient.

#### FASHION FLUCTUATION AND EXTERNAL FACTORS

The remarkable regularity of our wavelike fluctuations suggests a large measure of independence from outside historical events. The innovation of the safety razor and the wars which occurred during the period studied appear to have had negligible effects on the time series. King C. Gillette's patented safety razor began its meteoric sales rise in 1905. But by that year beardlessness had already been on the rise for more than 30 years, and its rate of expansion seems not to have augmented appreciably afterward. Far from initiating a great style wave, Mr. Gillette rode one to fame and fortune. As to wars, it seems that any significant alterations in rates of fluctuation in the time series are due mainly to sampling error. Beards (fig. 1) declined markedly in frequency during the Boer War, 1899-1902, and World War I. Clean shavenness (fig. 2) underwent a particularly rapid rise between 1915 and 1922. These changes probably reflect the unusually high incidence of younger men in the sample for the periods. High Commands do, of course, issue regulations permitting or proscribing whiskers, but I suspect that they merely reinforce the prevailing style.

#### CONCLUSIONS AND RECOMMENDATIONS FOR FURTHER RESEARCH

Measurements of style shifts in other frames of reference, such as architecture, furniture, and industrial design, should be undertaken. Also, I would like to see the present study expanded to include men in countries other than Great Britain and to extend the time span. A follow-up study of biographical information on the men of the *News* might determine their birth dates and thus provide clues to the extent to which older men cling to declining modes of whisker grooming.

In this report, my attention has centered on Kroeber's concern with "problems of *how* aesthetic styles change in general, to which in turn we must have some answer before we can hope to inquire fruitfully *why* they change" (Richardson and Kroeber 1940, "Conclusions"; italics mine).<sup>4</sup> My only venture here into the "why" is to subscribe to an explanation suggested by Agnes Brook Young, who observed that people consider two types of fashions ugly or ridiculous; not only the discarded variants of the current direction of style but even the fashions of the preceding era (Young

<sup>4</sup> The present study is an outgrowth of a long-standing interest in the understanding of fashion causation (see, e.g., Robinson [1963] and references cited therein).

1937).<sup>5</sup> This means that as long as any considerable number of people who have stuck to a superseded form of personal appearance are still living, the young may tend to avoid such a mode as old hat. These distasteful associations seem to be safely overcome only after the passage of a century or more.

<sup>5</sup> Miss Young was concerned with the shift in women's preference over time among three categories of skirt form: bell shaped, tubular, and full in the back. She found that each type dominated fashion to the exclusion of the other two for approximately a third of a century, but she made no attempt at time-series measurements.

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## APPENDIX

TABLE 1. NUMBERS AND PERCENTAGES OF FACIAL HAIR STYLES COUNTED PER YEAR, 1842-1972

Source: *Illustrated London News*

Year	Months Counted	Clean Shaven No.	Clean Shaven %	Moustaches No.	Moustaches %	Sideburns No.	Sideburns %	Moustache & Sideburns No.	Moustache & Sideburns %	Beards No.	Beards %	Total Number in Sample (1002)
1842	12	18	49	1	3	15	40	2	5	1	3	37
1843	6	43	40	2	2	51	47	4	4	8	7	108
1844	6	23	47	3	4	36	47	3	4	11	15	76
1845	12	31	46	1	2	27	40	1	2	7	10	67
1846	6	14	32	4	9	21	48	0	0	5	11	44
1847	6	11	26	2	5	20	46	3	7	7	16	43
1848	6	18	25	3	4	27	38	1	1	23	32	72
1849	6	19	32	3	5	23	39	2	4	12	20	59
1850	12	29	41	2	3	30	42	4	6	6	8	71
1851	12	25	25	4	4	60	59	5	5	7	7	101
1852	12	19	28	1	1	37	39	1	1	7	11	67
1853	12	20	22	5	5	55	59	7	7	7	7	94
1854	12	19	22	5	5	51	59	2	2	11	12	88
1855	12	17	18	6	7	45	48	9	9	17	18	94
1856	6	35	19	25	14	44	25	17	10	55	32	176
1857	6	21	16	13	10	48	37	10	8	37	29	129
1858	6	22	14	12	7	74	45	14	9	41	25	163
1859	6	30	21	12	8	38	26	6	4	58	41	144
1860	12	14	16	6	7	27	32	3	4	33	39	83
1861	6	9	25	3	8	14	39	2	6	8	22	36
1862	6	3	9	3	9	18	55	2	6	7	21	33
1863	6	6	12	3	6	16	32	2	4	23	46	50
1864	6	5	18	3	11	8	28	0	0	12	43	28
1865	12	7	12	2	4	25	43	6	10	18	31	58
1866	12	12	20	4	7	21	34	3	6	20	33	60
1867	12	8	15	2	3	18	32	4	7	24	43	56
1868	12	10	22	3	6	20	44	3	6	10	22	46
1869	12	8	14	1	2	25	43	3	5	21	36	58
1870	12	11	16	5	7	19	27	8	11	28	39	71
1871	6	11	25	3	7	13	29	7	16	10	23	44
1872	6	8	17	6	12	9	19	4	8	21	44	48
1873	6	9	21	0	0	14	32	4	9	16	38	43
1874	6	6	9	6	9	12	19	6	9	35	54	65
1875	12	20	12	10	6	33	20	25	16	75	46	163
1876	9	10	10	13	13	26	26	14	14	37	37	100
1877	12	5	8	8	12	14	21	11	16	29	43	67
1878	12	11	11	10	10	22	22	12	12	45	45	100
1879	9	7	7	21	21	13	13	25	25	34	34	100
1880	12	17	7	39	14	50	19	39	14	124	46	269
1881	6	13	13	25	25	11	11	10	10	41	41	100
1882	9	5	5	25	24	15	14	16	15	44	42	105
1883	6	9	9	17	17	9	9	7	7	58	58	100
1884	3	10	10	24	24	13	13	19	19	34	34	100
1885	12	5	3	60	29	38	19	13	6	88	43	204
1886	6	11	11	18	18	17	17	11	11	43	43	100
1887	9	2	2	20	20	21	21	11	11	46	46	100
1888	6	10	10	25	25	15	15	8	8	42	42	100
1889	6	14	14	24	24	9	9	4	4	49	49	100
1890	12	24	10	63	26	43	18	17	7	93	39	240
1891	3	14	14	18	18	21	21	6	6	41	41	100
1892	3	9	9	30	30	16	16	5	5	40	40	100
1893	3	12	12	20	20	8	8	7	7	53	53	100
1894	3	11	11	13	13	8	8	6	6	62	62	100
1895	12	64	14	163	34	54	11	26	6	168	35	475
1896	6	49	15	140	42	28	8	4	1	112	34	333
1897	6	55	13	159	37	40	10	27	6	148	34	429
1898	6	70	17	160	38	29	7	31	7	134	31	424
1899	6	93	14	382	57	34	5	30	5	128	19	667
1900	12	146	18	470	58	37	5	18	2	140	17	811
1901	3	35	18	80	42	11	6	6	3	59	31	191
1902	3	39	21	69	37	18	9	10	5	52	28	188
1903	3	44	23	88	47	7	4	5	3	43	23	187
1904	6	55	28	66	37	4	2	3	2	67	32	195
1905	12	102	22	167	37	17	4	17	4	153	35	456
1906	6	77	22	154	45	11	3	14	4	91	26	347
1907	6	129	23	217	41	23	4	18	3	150	29	537
1908	6	66	23	133	46	12	4	2	1	73	26	286
1909	6	97	25	157	39	10	3	2	1	126	32	392
1910	12	284	34	321	39	19	2	21	2	195	23	840
1911	3	96	27	122	43	8	3	11	4	66	23	283
1912	3	132	38	140	40	2	1	1	1	71	20	346
1913	3	59	33	72	41	4	1	0	0	36	25	172
1914	3	71	33	103	48	2	1	3	1	37	17	216
1915	12	583	34	1097	63	0	0	0	0	50	3	1730

# Shaving and Trimming of the Beard

## APPENDIX TABLE 1 (Continued)

Year	Months Counted	Clean Shaven No.	Clean Shaven %	Moustaches No.	Moustaches %	Sideburns No.	Sideburns %	Moustache & Sideburns No.	Moustache & Sideburns %	Beards No.	Beards %	Total Number in Sample (100%)
1916	3	83	33	157	64	0	0	0	0	2	1	242
1917	3	90	39	134	58	0	0	0	0	5	3	229
1918	3	79	33	154	64	0	0	0	0	7	3	240
1919	6	38	38	54	54	0	0	0	0	8	8	100
1920	12	197	42	218	46	4	1	2	1	48	10	469
1921	3	152	60	79	31	0	0	0	0	22	9	253
1922	3	104	57	56	31	0	0	0	0	22	12	182
1923	3	56	51	42	38	0	0	0	0	12	11	110
1924	3	56	47	47	39	1	1	2	2	14	11	120
1925	12	189	46	159	39	1	1	1	1	55	13	405
1926	3	47	47	38	38	0	0	1	1	14	14	100
1927	3	47	47	35	35	0	0	0	0	18	18	100
1928	3	68	68	22	22	0	0	0	0	10	10	100
1929	3	58	52	51	46	0	0	0	0	2	2	111
1930	12	178	51	130	38	0	0	0	0	39	11	347
1931	3	45	47	35	37	0	0	0	0	15	16	95
1932	3	68	62	32	29	0	0	0	0	10	9	110
1933	3	48	50	47	48	0	0	0	0	2	2	97
1934	3	61	55	40	36	0	0	0	0	10	9	111
1935	12	239	55	177	41	0	0	0	0	20	4	436
1936	3	69	59	44	38	0	0	0	0	4	3	117
1937	3	197	57	140	41	0	0	0	0	6	2	343
1938	3	91	67	41	30	0	0	0	0	4	3	136
1939	3	69	56	52	42	0	0	0	0	2	2	123
1940	12	284	61	172	37	0	0	0	0	12	2	468
1941	3	90	62	50	35	0	0	0	0	5	3	145
1942	3	88	71	32	26	0	0	0	0	4	3	124
1943	3	95	62	51	33	0	0	0	0	6	5	152
1944	3	94	63	48	32	0	0	0	0	6	5	148
1945	12	434	72	160	27	0	0	0	0	7	1	601
1946	3	110	63	59	34	0	0	0	0	6	3	175
1947	3	91	73	28	23	0	0	0	0	5	4	124
1948	3	79	75	22	22	0	0	0	0	4	3	103
1949	3	135	78	31	19	0	0	0	0	6	3	172
1950	12	243	73	78	23	0	0	0	0	12	4	333
1951	3	80	76	23	22	0	0	0	0	2	2	105
1952	3	92	75	27	22	0	0	0	0	3	3	122
1953	3	116	76	35	23	0	0	0	0	2	1	153
1954	3	122	86	19	13	0	0	0	0	1	1	142
1955	12	401	79	99	19	0	0	0	0	9	2	509
1956	3	104	78	27	20	0	0	0	0	2	2	133
1957	3	107	79	26	19	0	0	0	0	3	2	136
1958	3	110	78	28	19	0	0	0	0	5	3	143
1959	3	74	74	21	21	0	0	0	0	0	0	100
1960	12	119	87	18	13	0	0	0	0	0	0	137
1961	3	115	79	30	20	0	0	0	0	2	1	147
1962	3	165	81	32	16	0	0	0	0	6	3	203
1963	3	170	80	36	17	0	0	0	0	6	3	212
1964	3	150	81	32	17	0	0	0	0	4	2	186
1965	3	151	83	27	15	0	0	0	0	5	3	183
1966	3	228	86	27	10	0	0	0	0	10	4	265
1967	3	227	81	40	14	0	0	0	0	13	5	280
1968	3	153	83	28	15	0	0	0	0	4	2	185
1969	6	195	85	15	7	6	3	0	0	12	5	228
1970	3	180	84	14	6	12	6	0	0	9	4	215
1971	3	168	83	18	9	2	1	2	1	13	6	203
1972	6	195	84	17	7	1	1/2	1	1/2	17	8	231

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# **Toward a Revised Model of Residential Segregation and Succession: Puerto Ricans in New York, 1960-1970<sup>1</sup>**

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Generally accepted models of ethnic assimilation outline a pattern of decreasing residential segregation associated with increasing similarity to native whites. Similar models for the black population posit continuing residential concentration combined with rapid turnover and succession. Our analysis of data on recent settlement patterns of Puerto Ricans in New York indicates that this group is conforming to neither type of previously accepted model. Competition between the Puerto Rican minority and the larger, more economically advantaged black minority, a new set of public housing opportunities, and the return migration of successful Puerto Ricans are factors that were not considered in previously developed models. A new model of residential segregation and succession must incorporate these realities of contemporary urbanization.

As part of a study of changes in settlement patterns of Puerto Ricans in New York from 1960 to 1970, we have found that none of the generally accepted models of residential segregation and succession are fully applicable to the Puerto Ricans. The "melting pot" model of ethnic assimilation, for instance, proposes that immigrants in American cities become less segregated residentially as they approach native whites in education, income, and occupational prestige (Lieberson 1963; Taeuber and Taeuber 1965). Models of black-white segregation suggest that rigid segregation is maintained despite minority socioeconomic achievement, probably as a result of the inferior racial status ascribed to blacks (Duncan and Duncan 1957; Taeuber and Taeuber 1965). The latter model has been extended to describe the process of white-to-black residential succession as "an irreversible sequence of stages" in the direction of maintaining black segregation (Taeuber and Taeuber 1965, p. 101). Our examination of available data provides evidence that recent patterns of Puerto Rican settlement in

<sup>1</sup> Charles Perrow, Erich Goode, Gerald Suttles, Kirsten Grønberg, and James Bearden contributed to earlier drafts of this note, which was first presented as a paper at the 1974 meetings of the Population Association of America. A number of anonymous reviewers also provided helpful criticisms and bibliographic suggestions.

the New York SMSA coincide neither with models of ethnic assimilation nor with models of black segregation and succession. This finding highlights the distinctive elements of the Puerto Rican case, identifies pertinent new aspects of the contemporary urban context that were not considered in earlier models, and suggests the need for further work toward a general model of residential segregation and succession.

Data for our investigation were abstracted from 1960 and 1970 census tract publications reporting detailed characteristics of Puerto Ricans in the New York SMSA for tracts with a specified minimum number of Puerto Ricans, 250 in 1960 and 400 in 1970 (U.S. Bureau of the Census 1962, 1972). From these sources, we identified 258 tracts for which we could obtain data for both years. Tract boundaries for 1970 were adjusted to coincide with those established in 1960; for tracts divided between 1960 and 1970, we compiled statistics to accommodate the 1960 definitions. The vagaries of minority underenumeration, the change in the minimum concentration required for reporting detailed characteristics, and related limitations of the data set indicate that some Puerto Ricans were omitted from our sample. As an indication of the seriousness of the problem, we note that the 258 tracts include 62% of the enumerated Puerto Rican population for the New York SMSA in 1970. Improved levels of inclusion are achieved in sections of the SMSA with relatively high concentrations of Puerto Ricans; for example, the selected tracts include 81% of all Puerto Ricans enumerated in Manhattan in 1970. Realizing these constraints, we proceed with an examination of changing patterns over the decade.

The Puerto Rican population<sup>2</sup> in the New York SMSA experienced rapid growth over the past decade: between 1960 and 1970 it increased by 34.4%, from 629,430 to 845,775. At the same time, Puerto Ricans continued to be highly concentrated in the three central city boroughs—the Bronx, Brooklyn, and Manhattan—with little decentralization to the outlying boroughs (Queens and Richmond) or the suburban counties (Nassau, Suffolk, Rockland, and Westchester). In 1960, 94.2% of the SMSA's Puerto Rican population resided in the Bronx, Brooklyn, and Manhattan; in 1970 the proportion was still 91.5%. In comparison, of the SMSA's black population, 76.0% in 1960 and 74.0% in 1970 resided in the three central city boroughs. The small population shift to other areas of the SMSA suggests that Puerto Ricans are not as yet following the patterns set by earlier immigrants and are still segregated from non-Puerto Rican whites to an extent similar to that of the black population (U.S. Bureau of the Census 1962, table P-5; U.S. Bureau of the Census 1972, table P-2).

<sup>2</sup> Throughout the note "Puerto Ricans" include persons of Puerto Rican birth and Puerto Rican parentage, in other words, first- and second-generation Puerto Ricans.

Despite this finding of innercity centralization, further examination of the data reveals that the residential patterns of Puerto Ricans differ from those of blacks along several significant dimensions. The percentage of Puerto Ricans for the 258 selected tracts is on the average much lower—at both census dates—than corresponding figures for blacks reported in earlier studies of other cities that have formed the basis for models of black residential segregation (Duncan and Duncan 1957; Taeuber and Taeuber 1965). The mean percentage of Puerto Ricans for our entire tract sample was 27.3 in 1960 and 32.5 in 1970. Since by definition the 258 tracts contain more than the requisite minimum number of Puerto Ricans, these figures actually overestimate the average level of Puerto Rican concentration throughout the SMSA.

Of even greater interest than these general averages are the detailed local changes from 1960 to 1970 presented in table 1. Focusing on concentration measured by 20% categories at each year, it appears that residential succession for Puerto Ricans is not the "inevitable" process that has been described for blacks in earlier studies. The majority of tracts in each category maintained the same level of minority concentration from 1960 to 1970. Few tracts increased markedly in percentage of Puerto Ricans, and the level of concentration declined in a significant number of tracts. No tract was more than 80% Puerto Rican in either 1960 or 1970. Even considering the restrictions of our sample, the data offer no evidence of a unidirectional turnover process for areas "invaded" by Puerto Ricans in New York.

TABLE 1  
CHANGES OF TRACTS BY CATEGORIES OF PERCENTAGE OF PUERTO RICANS, 1960-70,  
FOR ALL 258 SELECTED TRACTS IN NEW YORK SMSA:  
TRANSITION PROBABILITIES

PERCENTAGE PUERTO RICAN IN 1960	PERCENTAGE PUERTO RICAN IN 1970				Total
	0-20	21-40	41-60	61-80	
0-20 .....	.59 (62)	.28 (30)	.13 (14)	.00 (0)	1.00 (106)
21-40 .....	.16 (16)	.51 (52)	.28 (28)	.05 (5)	1.00 (101)
41-60 .....	.02 (1)	.24 (11)	.59 (27)	.15 (7)	1.00 (46)
61-80 .....	.00 (0)	.00 (0)	.20 (1)	.80 (4)	1.00 (5)
Total .....	.31 (79)	.36 (93)	.27 (70)	.06 (16)	1.00 (258)

NOTE.—There were no tracts in either year with 81%–100% Puerto Rican. Absolute numbers are shown in parentheses.

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Stages of the white-to-black succession process implying the inevitability of continued turnover (Duncan and Duncan 1957, p. 118; Taeuber and Taeuber 1965, p. 106) were derived from a set of observations not duplicated by the Puerto Rican data. The Duncans found, for example, that "once an area had reached a proportion of, say, 10 percent Negroes . . ." (1957, p. 11) the percentage of blacks tended to increase inexorably. The Taeubers (1965) identified *very few* stable interracial areas and *very few* areas in which a once-established black population was displaced. In contrast, we found both of the latter situations to be common in our sample of Puerto Rican tracts. In view of the impracticality of utilizing the stages of succession derived for blacks in our analysis of the Puerto Rican data, we have established a revised typology of tracts (table 2) which allows a more meaningful interpretation of residential transition patterns of New York Puerto Ricans. The first and third types in particular represent departures from established models.

TABLE 2  
DEFINITION OF TRANSITION TYPES OF TRACTS BY PERCENTAGE OF PUERTO RICANS IN  
1960 AND 1970

TRANSITION TYPE	DEFINITION	PERCENTAGE PUERTO RICAN IN	
		1960	1970
1. Stable mixed (62) . . . . .	Low Puerto Rican representation at beginning and end of the decade	0-20	0-20
2. Slight increase mixed (30) . . . . .	Increase in percentage of Puerto Ricans from low representation at beginning of decade to moderate representation at the end.	0-20	21-40
3. Stable moderately mixed and displacement (80) . . . . .	Moderate representation of Puerto Ricans throughout the decade or decrease in percentage Puerto Rican over the decade	21-40 21-40 41-60 41-60	0-20 21-40 21-40 0-20
4. Early consolidation (42) . . . . .	Increasing percentage of Puerto Ricans over the decade from a low or moderate level of representation at the beginning to a higher one at the end	0-20 21-40	41-60 41-60
5. Late consolidation (44) . . . . .	Increasing percentage of Puerto Ricans over the decade with a relatively high level of representation at both beginning and end of the decade.	21-40 41-60 61-80	61-80 41-80 61-80

NOTE.—N's shown in parentheses.

Utilizing this typology of Puerto Rican tracts, we examined the relationship between status characteristics of the minority group and the level of minority concentration in a tract. Two closely related findings emerge. First, for both census dates we have evidence of differentiation by socioeconomic criteria within the Puerto Rican population paralleling the level of Puerto Rican concentration in a tract. Focusing on educational level, for example, in 1970 the mean level of median school years for the Puerto Rican population in tracts with a 0%–20% level of concentration is 8.3 years; the comparable educational level in tracts with the highest Puerto Rican representation—61%–80% Puerto Rican—is 7.3 years. Second, although studies of black succession report considerable stability in socioeconomic status rankings of tracts in which transition is occurring, we found little evidence of such stability among the Puerto Rican tracts in our sample. Again focusing on educational level as an example, the zero-order correlation between median school years of Puerto Ricans in 1960 and 1970 for our selected 258 tracts is only  $+.372$ —positive, as might be expected, but rather weak in comparison with findings for blacks reported in studies cited earlier (e.g., Duncan and Duncan 1957, p. 353).

In view of these findings, we next examined the interaction between changes in socioeconomic status characteristics of the minority population and changes in minority concentration over time. For selected socioeconomic measures we divided all tracts into those below the mean and those at or above the mean; this division was made for both 1960 and 1970. Then (in a manner similar to Hunter's [1974]) we further divided the tracts into four possible categories of change for a given socioeconomic measure; for example, tracts below the mean in both 1960 and 1970, ones below the mean in 1960 but at or above it in 1970, and so on.

This longitudinal analysis uncovered evidence of noticeable interaction between changing minority concentration and status change for Puerto Rican tracts over the decade. We have summarized our findings for changes in educational status in table 3. "Stable mixed" and "slight increase mixed" tracts have the greatest probability of exhibiting high educational status in both 1960 and 1970; tracts in the "stable mixed" category are also most likely to improve from a low status rank in 1960 to a high status rank in 1970. Tracts in the "late consolidation" category are most likely to be *low* status ones at both dates and most likely to change from high to low status over the decade. These figures provide evidence for the importance of simultaneous changes in minority concentration and status differentiation within the minority group. A stable intergroup population mixture is likely to be associated with Puerto Ricans of higher status; increasing consolidation of the minority group appears to be associated with lower minority status ranking.

Our analysis of changes in the residential distribution of New York's

# Residential Segregation and Succession

TABLE 3

TRANSITION TYPES OF TRACTS BY SOCIOECONOMIC CHANGE—EDUCATION—FROM  
1960 TO 1970 FOR ALL 258 SELECTED TRACTS IN NEW YORK SMSA:  
TRANSITION PROBABILITIES

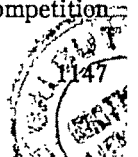
TRANSITION TYPE	CHANGE IN EDUCATIONAL LEVEL				Total
	<1960 Mean	<1960 Mean	>1960 Mean	>1960 Mean	
	<1970 Mean	>1970 Mean	<1970 Mean	>1970 Mean	
1. Stable mixed .....	.11 (7)	.27 (17)	.15 (9)	.47 (29)	1.00 (62)
2. Slight increase mixed ..	.20 (6)	.20 (6)	.17 (5)	.43 (13)	1.00 (30)
3. Stable moderately mixed and displacement .....	.37 (30)	.24 (19)	.11 (9)	.28 (22)	1.00 (80)
4. Early consolidation ...	.31 (13)	.17 (7)	.24 (10)	.28 (12)	1.00 (42)
5. Late consolidation ....	.48 (21)	.16 (7)	.25 (11)	.11 (5)	1.00 (44)
Total .....	.30 (77)	.22 (56)	.17 (44)	.31 (81)	1.00 (258)

$$\chi^2 = 33.4 \text{ with 12 df} \quad P < .001$$

NOTE.—The socioeconomic measure used here was "median school years of Puerto Ricans" in 1960 and in 1970. Tracts were classified as either "Less than the Mean" or "Greater than or Equal to the Mean" in both 1960 and 1970. The mean was calculated for all tracts with available information. Absolute numbers are shown in parentheses.

Puerto Rican population indicates significant discrepancies between observed patterns and the expectations based on models developed for blacks and earlier immigrant groups (Cressey 1938; Ford 1950; Duncan and Duncan 1957; Lieberman 1963; Taeuber and Taeuber 1965). Upon reflection, it appears that the applicability of the earlier models is limited by underlying assumptions prompted by the conditions extant at the time of their development. The Puerto Rican data allow us to propose several additional elements of the contemporary urban scene that should be incorporated in a general model of residential segregation and succession.

Several researchers have alluded to the advisability of considering the residential location of Spanish-speaking minorities (Puerto Ricans in the Northeast and Chicanos in the Southwest) in conjunction with that of blacks (Kantrowitz 1969; Schnore 1965). Their suggestions, however, have never been adequately acted upon. Earlier models of residential segregation and succession have uniformly failed to consider the possible influence of *interminority competition* on residential patterns in the context of a housing market incorporating two or more minority groups as well as the majority. According to our analysis, the Puerto Rican minority in New York not only seems to suffer from the direct effect of competition



for housing with the non-Puerto Rican white majority but also appears to be affected indirectly by the housing constraints imposed upon blacks.

In order to gain an initial appreciation of the role of interminority competition, we compared the 1960-70 growth rates of the three groups in our 258 tracts. It appears that despite majority group out-migration, residential changes within the larger black minority have been instrumental in forestalling transition to Puerto Rican dominance. As expected, we found negative growth rates for the nonminority white population in all transition categories. As Molotch (1969) has pointed out, out-migration of the nonminority population provides the potential for residential succession. Examining the same tracts further, however, we found that blacks had higher growth rates than Puerto Ricans. In "stable moderately mixed and displacement" tracts, for example, while the nonminority population declined by 35.8% on the average, the black population increased by 91.3%, and the Puerto Rican population declined by 17.7% over the decade.

Within the private housing market, majority group preferences for Puerto Rican versus black tenants and neighbors are balanced against the relative socioeconomic standing of the minority groups to determine the outcome of interminority competition. With somewhat higher ascribed racial status, Puerto Ricans may be preferred as tenants and neighbors; on the other hand, discrimination against blacks may be somewhat mitigated by their higher ranking on socioeconomic status measures and consequently greater purchasing power. Comparison of median family income levels for the two groups (table 4) illustrates this advantage. With their financial edge, it appears that despite a low racial status blacks can compete more successfully for housing in a private market that selects for ability to pay.

TABLE 4

MEANS OF BLACK MEDIAN FAMILY INCOME AND PUERTO RICAN MEDIAN FAMILY INCOME BY TRANSITION TYPE FOR SELECTED TRACTS IN NEW YORK SMSA, 1970

TRANSITION TYPE	MEANS OF MEDIAN FAMILY INCOME FOR	
	Blacks	Puerto Ricans
1. Stable mixed .....	\$6,924	\$6,265
2. Slight increase mixed .....	6,467	5,289
3. Stable moderately mixed and displacement .....	6,327	5,275
4. Early consolidation .....	5,824	5,204
5. Late consolidation .....	5,502	4,780

NOTE.—Black median family income was available for only 238 of the selected tracts. Puerto Rican median family income was available for 258 tracts. For the entire SMSA, the median family income of blacks was \$7,313; for Puerto Ricans it was \$5,666.

The transfer of housing stock from the private to the public sector through urban renewal and housing development programs adds another important—and overlooked—dimension to the operation of minority com-

petition in New York. Given the absence of a quota system in public housing and the absence of a policy designed to maintain the ethnic/racial composition of neighborhoods subject to renewal, the larger number of potential black tenants produces a residential mixture in which blacks outnumber Puerto Ricans, another factor preventing the development of Puerto Rican dominance.

Finally, previously accepted models have viewed the competition between majority and minority groups as a closed system. In contrast with the experiences of both blacks and European immigrants, however, Puerto Rican migration appears to be of a more temporary nature. Increasing evidence, restricted thus far to journalistic and informal accounts, points toward a high frequency of return migration for successful Puerto Ricans. Were these individuals to remain on the mainland, they might act like the elite of other groups and initiate Puerto Rican expansion into new residential areas.

In summary, New York Puerto Ricans compete for housing with non-Puerto Rican whites *and* with the larger black minority. As was true for earlier immigrants, low income levels and housing discrimination have inhibited the assimilation of the Puerto Rican minority. At the same time, however, an expanding black population and encroaching urban renewal have prevented the establishment of Puerto Rican concentrations of sufficient scale to allow the development of indigenous political and economic resources, a prerogative traditionally available to upwardly mobile ethnic groups (see Fitzpatrick [1971, p. 57] for a discussion of this organizational dilemma). The increased mobility and possibility of return migration of these airborne migrants, furthermore, tends to deprive the group of its organizational elite.

Each of the factors we have pointed out is likely to differentiate Puerto Rican residential patterns from those of earlier groups: continuing research is required to document their impact. In addition, research should aim at identifying the relative numbers and characteristics of blacks and Puerto Ricans in predominantly white areas, as well as the relative importance of public housing for each group. Data for New York on these factors, together with comparative data from other cities, will lead to a more comprehensive model of minority residential distribution.

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## Commentary and Debate

### A CRITICAL COMMENT ON SUCCESS-VALUE RESEARCH

This is a critique of L. Richard Della Fave's analysis (*AJS* 80 [July 1974]: 153-69) of the "value stretch" concept and his data purporting to test that concept. My basic criticism is that the data do not necessarily lead to the conclusions drawn by Della Fave because the items used as measures have little to do with values.

Asking a person how many years of education he prefers and what particular occupation he prefers are not appropriate ways of measuring the extent to which education and work are basic values for that person, his guides to the good life or the good self. The questions used by Della Fave ask persons to project themselves into particular social roles or positions. The finding that lower-class students prefer fewer years of education and lower-status jobs than upper-class ones is merely a validation of socialization into class roles.

Della Fave, as he himself points out, did not invent this approach to measuring "success values." He is expanding on a tradition. It is necessary, therefore, as part of this critique, to offer ways of testing the traditional approach. Both reasoning and data will be offered.

If my view is correct and respondents' preferences as well as expectations tend to be influenced by their current class position, there should be a strong correlation between preferences and expectations of individuals within social-class divisions. This can be readily determined. Only if it can be shown that preferences are unrelated to expectations is it reasonable to talk of the two measures as indicators of two independent psychological processes.

In my view, the more appropriate way of determining values is to leave out specific levels of accomplishment and ask persons the importance of obtaining certain general goals such as a good education or employment. This procedure was followed in a study aimed at comparing the orientations toward life and work of more than 5,000 poor and nonpoor persons (Goodwin 1972). A series of questions was asked about general life aspirations, or goals, and life avoidances. Thus for each aspiration, such as "having a good education," its converse was also asked about, in this instance "having very little education." Each item was rated on a four-step ladder that said "best way of life" at the top and "worst way of life" at the bottom. (The set of 14 life aspirations and 14 life avoidances formed two separate scales with high reliability [see Goodwin 1972].) To illustrate the criticism of Della Fave's approach, some previously unpublished data from the study will be used.

The data come from five sets of youths, ranging in background from long-term welfare recipients to wealthy suburbanites. Their characteristics are presented in table 1. Variations in family income and educational level are striking, indicating that responses came from different class groups. Table 2 presents the mean values given by the five sets of youths to the two education items just mentioned: a good education and very little education.

TABLE 1  
CHARACTERISTICS OF WELFARE AND MORE AFFLUENT TEENAGE MALE YOUTHS

Characteristic	Long-Term Welfare*	Short-Term Welfare*	Outer-City Blacks†	Outer-City Whites‡	Suburban§
Age .....	17	17	17	17	17
Years of education .....	9	9	10	11	11
School dropout (%) .....	18	19	10	3	...
High school graduates .....	8	6	11	29	...
Mother's years of education .....	8	9	11	12	13
Father's years of education .....	...	...	10	13	14
Years on welfare .....	16	1	...	...	...
Combined annual earnings of father and mother (\$) .....	2,800	3,100	9,100	11,200	16,000
Respondents (N) .....	267	122	250	100	107

NOTE.—All numbers are averages except number of respondents; the data are from the Baltimore area, 1970; respondents were between the ages of 15 and 19. For details on sampling, see Goodwin (1972).

\* All welfare youths in the samples were black because the vast majority of welfare recipients in the city of Baltimore are black.

† In interracial neighborhoods outside the Baltimore ghetto but still within the city limits.

‡ In the same neighborhoods as the outer-city blacks.

§ In the affluent suburbs of Baltimore.

|| Estimate based on the average welfare payment.

TABLE 2  
EDUCATION AS A VALUE AMONG TEENAGE MALE YOUTHS

CATEGORY	MEANS		Indicator of Value Stretch†
	Having a Good Education*	Having Very Little Education*	
Suburban .....	3.61	1.49	0.10
Outer-city white .....	3.77	1.25	0.02
Outer-city black .....	3.89	1.43	0.32
Short-term welfare .....	3.91	1.31	0.22
Long-term welfare .....	3.75	1.38	0.13

\* Rated on a four-step ladder that said "best way of life" at the top and "worst way of life" at the bottom.

† The difference between the distance from the bottom of the four-step ladder of the mean value for "having very little education" and the distance from the top of the ladder of the mean value for "having a good education." The quickest way of calculating the number is to add together the two means and subtract five.

If anything, the poor, lower-class youths appear to value a good education even more than the higher-class ones. Della Fave might argue that if you asked respondents more specifically what constitutes "a good education,"

you would then get the responses he reported. I would agree, and that is precisely the point. The underlying value component can be, and needs to be, separated from its particularization in social roles.

This is more than an idle distinction. It means, for example, that public policies and programs supporting education have public approval (other things being equal) because "having a good education" is valued by large segments of the society. The same is true regarding programs of work training and work. They are supported because of a general belief in the value of work. Conversely, programs of public welfare, where money is transferred without work, violating the work ethic, are anathema. See Goodwin (1975) for a discussion of the work and welfare situation.

The next question is how the data in table 2 relate to the value stretch. That concept, as initially presented by Rodman (1966), had to do with lower-class persons in Trinidad accepting formal marriage as a value but accepting other forms of union as well. Implied in the "stretch" is that formal marriage is a life aspiration but failure to fulfill that goal is not a serious threat to one's self-image or way of life. To middle-class persons, failure to fulfill the goal of formal marriage would be a greater threat, closer to "the worst way of life." With this specific interpretation of the value stretch, what do the data in table 2 indicate?

The average rating of suburban youth for "having a good education" is 3.61, and the average rating for "having very little education" is 1.49. The latter figure indicates the extent to which lack of education is taken as a threat to the best way of life and as part of the worst way of life. At issue is whether the 0.49-point distance from the bottom of the four-step ladder is as great as or greater than the 0.39-point distance from the top for "having a good education." Subtracting 0.39 from 0.49 yields a value of 0.10 as an indicator of the extent to which suburban youth aspire to a good education but would not be disappointed if they received only a little education. The value-stretch hypothesis would predict that lower-class respondents would exhibit higher indicators than higher-class ones, the former being more tolerant of failure to fulfill their generalized goal of a good education.

The data in table 2 show indeed that the outer-city white youth and the suburban youth have the lowest indicators. Higher ones are given by the upwardly mobile black outer-city youth and those on welfare. However, the highest indicator (.32) is given by the former set rather than the welfare recipients, who come from the lowest class. Moreover, the lowest indicator of all is for the outer-city white youth rather than the suburbanite. Interpretation of these findings depends on an interpretation of the psychology underlying the value stretch.

My interpretation is that persons develop the value stretch as a defense against failure or fear of failure—against a sharp loss of self-esteem. The

appearance of such a stretch across class lines occurs where the upper classes have achieved success or are more sure of it in comparison with the lower classes. But such a stretch would occur, according to the view presented here, among any set of persons facing a common threat to value attainment. The black outer-city youth come from families in the midst of upward mobility who are having to cope with considerable risks—a lower family income than their white neighbors, for one thing. Other data (see Goodwin 1972) indicate that this is the most psychologically threatened group of teenagers. Hence the high value-stretch indicator for them can be explained in terms of high fear of failure.

The fact that suburban youth exhibit a higher indicator than the outer-city white youth, who are of somewhat lower class status, might be explained by the greater relative threat experienced by those at the top because of increased demands on their performance. Such interpretations of course need further testing. In any case, the data do suggest a greater value stretch among lower-class than higher-class youth.<sup>1</sup>

Although the findings presented above are hardly conclusive evidence, a more extensive analysis (Goodwin 1973) of the way different classes pattern the 14 pairs of goals and avoidances, presented in another report, also supports the value-stretch concept. The point is that the data presented in table 2 challenge Della Fave's conception of the value stretch.

Do his findings illustrate anything more than the class socialization of respondents? There is an important finding that the difference between preference and tolerance for specific educational levels and occupational roles (what Della Fave has called the "value range," as seen in his table 6) turn out to be similar across classes. This, I argue, indicates a commonality in the ability to cope with psychological stress. That is, the gap between specific preferences and minimally acceptable achievements creates psychological stress or threat. Persons in different class groups are able to cope with about the same level of threat and accordingly bring their preferences and tolerances into a similar relative alignment, with lower-class persons lowering and higher-class persons raising both preferences and tolerances concerning specific achievements.

Della Fave's data show a weak tendency for higher-class as against lower-class persons to see their preferences regarding educational and occupational achievement more closely coinciding with their expectations (his table 5). Such a relationship disappears for monetary expectations and preferences. And indeed, the variable of monetary attainment is less well related to class status with respect to expectation and tolerance than are the variables of educational and occupational achievement. This is to be

<sup>1</sup> Class and race differences happen to coincide in the data presented here. Additional data, not presented here, show that white men on welfare show the same kind of value stretch as black youth on welfare. Hence, the phenomenon is not racially bound.

expected in view of my criticism of Della Fave's approach. Gaining money is less tied to specific class roles than is either occupation or education.

Della Fave has an interesting unexplained finding with respect to money. A substantial percentage of persons in each class category, around 22%, give a lower rating to their expectations about how much money they will earn than the level of money which they would be willing to accept. (This, at least, is my interpretation of the situation; the actual items were not included in the article.) It is likely that there is another level to be considered below that of tolerance. This might be called the rejection level; it would be measured by the statement, "I would reject as insufficient the following: . . ." Levels of education, occupational achievement, and income could then be mentioned.

In sum, it seems that Della Fave has not tested the value-stretch concept with his data. By equating values with specific occupational and educational levels of achievement, he has built class differentials into the questions asked. When measures of value are made independent of specific roles, there is a tendency for lower-class respondents to score as high as upper-class ones. A value stretch is indicated in the sense that certain sets of persons, while accepting good education as a positive value, are also relatively tolerant of gaining only a little education.

The challenge offered here to the Della Fave interpretation has practical as well as theoretical significance. National policies on education, occupational training, and so forth for lower-class persons will differ depending on whether policymakers perceive such persons as having values similar to their own—namely, middle-class ones or ones different from theirs. While social research rarely has public-policy impact, in dealing with the poor it has had in the past and can have in the future a substantial impact on policymakers (see Goodwin 1975). Thus, both for advancing knowledge of the social realm and for advancing public policy, it is desirable to design further research to test explicitly the challenge raised here about class-differentiated values.

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PUBLIC-POLICY IMPLICATIONS OF RESEARCH AND  
LIBERAL SENSITIVITIES: A REPLY TO GOODWIN

There is in Goodwin's reaction to my article a dual critique, one aspect directed at conceptual and methodological issues and the other concerned with policy implications. While in general this can be quite appropriate, here I see it as a case of the political tail wagging the scientific dog. For it appears to me that a number of social scientists of liberal persuasion, among whom I count Goodwin, are intent upon demonstrating that the lower class (poor, disadvantaged, etc.), after all, really has the same values as the middle class in order to bolster arguments in favor of programs that would expand opportunities for members of the lower class. And, while I share their general outlook, I will attempt to show that existing data are more than sufficient to support liberal programs without having to demonstrate homogeneity of values.

But before pursuing the political issues, let us take up Goodwin's conceptual and methodological arguments. First, I am puzzled by his contention that my measures have "little to do with values" and are "merely a validation of the socialization process," for is it not through the socialization process that values are transmitted? But this is minor. Much more serious is Goodwin's apparent confusion of the conceptual or logical independence of variables with their statistical independence. Two variables that are logically independent may or may not be correlated with one another in any particular instance. For example, in Weber's (1946) classic essay on the dimensions of social stratification, class, status, and party are conceptualized as logically independent. Only because of this logical independence can we ask meaningfully, To what extent are these three variables related to one another empirically in one particular society at one particular point in history? Even if at many specific places and times class, status, and political power (party) are nearly perfectly correlated with one another, that fact does not by any means gainsay their logical independence. To insist, as Goodwin does, that preferences and expectations must be statistically independent in order to qualify as logically independent and distinct variables is to insist that the empirical dice be loaded in favor of the value stretch.

Our disagreements over the proper measurement of values, pursuant to a test of the value stretch, are rooted in divergent readings of Rodman's description of this concept. Rodman's (1963, p. 206) original paper on the topic contains a footnote in which he makes clear that the term "value" is to be construed in its broadest sense:

No formal distinction is made in this paper between values, norms, aspirations, and (desirable) goals. I realize that they can be distinguished from

each other and that several volumes could be devoted to the task. I realize also that some authors may complain, and with some justice, that by using their statements about norms, aspirations or goals as an index of their position on values I am distorting their point of view. (Most authors, I should add, themselves use some of these concepts interchangeably.) But it is because I want to focus on the common element of *desirability* that lies behind all these concepts, as I use them, that I am running the risks.

At least there is no case to be made a priori for measuring values in the more general terms suggested by Goodwin instead of the particularized manner on which my article relies. Beyond the aforementioned footnote, Rodman, in the same (1963) article, uses an example in which he treats values exactly as I do. In fact, in a later unpublished paper (Rodman and Voydanoff 1969) he even measures them in precisely this way.

In the same vein, Goodwin's central criticism is that by measuring values in terms of particular levels of aspiration one is virtually assured of arriving at findings similar to mine. This is simply false: two other papers in which this approach has been used reported findings different from mine (Rodman and Voydanoff 1969; Han 1969).<sup>1</sup>

Let us now move to Goodwin's interpretation of my data and his. He attempts to explain away my finding of the nearly identical size of the value range across classes by saying that it probably represents "a commonality in the ability to cope with psychological stress." Whatever the reason, however, the fact that this measure of value range behaves as it does casts doubt on Rodman's hypothesis concerning the existence of a wider range of values in the lower class. My interpretation is reinforced by the finding that this constancy in the size of the value range is the outcome of lower levels of both preference and tolerance among members of the lower class than among the more privileged. If it is true, as Goodwin claims, that lower-class youngsters attempt to reduce psychological stress by lowering preferences (and this is perfectly consistent with my data), it tends to refute Rodman's pivotal claim that middle-class values are not abandoned by members of the lower class. Thus, by using some of Goodwin's own arguments, I feel I have, if anything, strengthened my original case.

Finally, I will attempt to show that Goodwin's own data do not support

<sup>1</sup> This is not to say that there are no differences in measurement technique between my article and theirs. There are, and I maintain that they did affect the nature of the findings, as I state in my article. Also, a reanalysis of my data (Della Fave 1974) employing synthetic-cohort analysis, has turned up some evidence of a value-stretch pattern among one specific segment of the sample—lower- and lower-middle-class youngsters in terminal (as opposed to college prep) curricula. Though not conclusive, these findings suggest that it is not the youngster's social class of origin, his background, that is crucial; instead it is his anticipated class of destination. This is further evidence that the use of specific levels of aspiration as a measure of success values does not predetermine the findings one way or the other.



Rodman's predictions, even though they are derived from a general rather than a particularized measure of values. Looking at Goodwin's table 2, specifically at his indicator of value stretch in the right-hand column, we would expect to find the smallest value in the top group (suburban) and to see the values increase monotonically down the column, producing an ordering of groups: 1, 2, 3, 4, 5. Instead we find the following order: 2, 1, 5, 3, 4. What are we to make of this? Goodwin devises an *ex post facto* explanation that appears to fit the data.<sup>2</sup> All well and good, but my original purpose was to test hypotheses derived directly from Rodman's description of the value stretch. This I feel I have done, and my findings cast doubt on the validity of the hypotheses under discussion here. I also feel that I have refuted Goodwin's argument that it is the way in which values are measured that determines the findings. Goodwin's revised interpretation of the value stretch, conceived in the light of his own data, needs to be tested through the use of new data in order to avoid the dangers of circular reasoning. But even before this can be done, he must explicate his interpretation more clearly. His main hypothesis appears to be that the indicator of value stretch will increase as a positive function of psychological threat. Specifically, then, we need to know what will serve as a logically independent indicator of threat. However, in defining this indicator, Goodwin must confront Rodman's (1963) description of the principal factor behind the process by which tolerance is lowered—namely, a fear of not being able to achieve middle-class levels of attainment.

This brings us back to the issue of policy implications. The relevant question here is not, I maintain, whether there is some degree of class differentiation in values but rather whether lower-class youth prefer substantially more in the way of educational and occupational attainment than they expect or will probably achieve. My data, as well as those of many others (Han 1969; Caro and Pihlblad 1965; Turner 1964; Stephenson 1957; Sewell, Haller, and Straus 1957; Empey 1956) show that this is clearly so.<sup>3</sup>

This type of finding suggests to me that if opportunities for lower-class youth were opened up, it is very likely that they would take advantage of them. In addition, however, expectations and, if Goodwin is right, even preferences would also rise in a sort of benign spiral. Given the demonstrable reservoir of unfulfilled ambitions on the part of lower-class youngsters, policymakers need not be deterred from greatly expanding opportunities for them simply because their expectations and preferences happen to fall somewhat short of those of upper-middle-class youth.

<sup>2</sup> But even here the discrepant position of the bottom group is not taken sufficiently into account.

<sup>3</sup> I point out in my article that the expectations (not even preferences) of the boys in the lowest class are extremely high in absolute terms.

After all, as Turner (1964) has pointed out, the latter group's expectations often border on the extravagant. In my estimation, it is not a shortage of supportive data that prevents large-scale programs designed to greatly increase opportunities for the disadvantaged from getting off the ground, and staying there. No, it is the fact that such programs would require a substantial reallocation of resources. As for the future prospects of this kind of reallocation, pessimists can cite Senator McGovern's resounding defeat in the presidential election of 1972, while optimists can point to Rainwater's (1974) recent findings.

If one is inclined to point to the apparent influence of the "culture of poverty" approach taken by some social scientists concerning the Johnson administration's War on Poverty, it might be well to raise the question of whether that viewpoint was acceptable to policymakers because it fell within their own definition of what was politically feasible in the first place.

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ON HECHTER'S INTERPRETATION OF WEBER

In his recent series of articles, some appearing in this *Journal* (*AJS* 79 [September 1973]: 319-42; [March 1974]: 1151-78), Michael Hechter is to be congratulated for making what may well be landmark advances in several areas of substantive social theory and analysis. Without faulting Hechter's original contribution, it is the case that he has misinterpreted the writings of his chief classical foil, Max Weber.

Hechter suggests that in Weber's work we find *alternative* categories of group orientation described as status and class. In this light, referring to the writings on the contemporary fate of status-oriented behavior, Hechter writes: "Weber (1968, pp. 927-36) described the process as involving a shift from affinity on the basis of status group (*Stände*) to affinity on the basis of class" (1974, p. 1153). It is no doubt the case that other classical social theorists have defined status and class as two alternative sources of group affinity. But this is not the case for Weber. In what appears to be the most relevant of Weber's writings on this subject, writings to which Hechter makes reference, Weber argues: "And today the class situation is by far the predominant factor. After all, the possibility of a style of life expected for members of a status group is usually economically conditioned" (1968, p. 935). Elsewhere Weber has defined "economically conditioned" as "... phenomena which are *not* 'economic' in our sense and the economic effects of which are of no, or at best slight, interest to us . . . but which in individual instances are in their turn more or less strongly influenced in certain important aspects by economic factors . . ." (1949, pp. 64-65). From this definition it should be clear that Weber's observations of affinity on the basis of status within the present era (where class affinity indeed does play a major role) do not mandate its absence or elimination. Weber rather appears to indicate that the economic circumstances in which class affinity originates *circumscribe* or *condition* the social circumstances in which status affinity originates. There is no suggestion of mutually exclusive categories of group orientation. Thus, it seems quite possible to suggest that Weber would not have been surprised to learn that certain forms of status affiliation such as those within ethnic groups had been found to develop or persist within the context of capitalist societies. Hechter's suggested interpretation of Weber in this regard thus seems in error.

Hechter is repeating here a sin that too many sociologists of the past several generations have committed. These sociologists have attempted to emphasize dichotomies and predictions made on the basis of these dichotomies within the context of Weber's work. It is the case, however, that Weber's choice of conceptual devices for theoretical analysis and his choice of data both emphasize the complexity of individual social circumstances

rather than impose categories such as those of a dichotomy. Thus, the ideal type, such as class or status group, is used by Weber to identify similarities and contrasts in reality. By this means, Weber is able to overcome the simplification of reality inherent in any form of categorization such as the dichotomy. Weber's choice of data also reveals his concern with the specific characteristics of individual social events. Thus, Weber focuses attention on historically descriptive events as the basic source of the data he uses for the solution of the problems which interest him. These methodological observations again strongly suggest that Hechter is mistaken in attributing to Weber the assertion that group affinity moves simply from that of status to that of class during the capitalist era.

This of course is not to argue that Weber did not emphasize the significance of class as the predominant source of group affinity in the capitalist era. But Weber's argument seems to be that it is the usurpation of power from a status-based social structure by a social structure based upon markets and market-based classes that characterizes the emergence of the capitalist era (Weber 1968, p. 337). At no point does Weber specifically exclude the possibility of the persistence or development of groups based on status affinity within the context of the structure of markets or market-based relations. For Weber, status groups are economically conditioned, not economically precluded. The development of one such economically conditioned group would seem to be Hechter's main observation from his empirical data on the Welsh. In this light, then, there is no reason to believe that Hechter's findings contradict Weber's work.

Additional support for the reconciliation of Weber's work with Hechter's findings comes from a reading of *The Protestant Ethic and the Spirit of Capitalism*. Here it must be recognized that Weber's argument concerning the place of classes and status groups in the capitalist era ultimately depends upon his analysis of the rationalization of society induced, in part, by the emergence of a market-based economy (which in turn had been conditioned by the Protestant Ethic). In *The Protestant Ethic and the Spirit of Capitalism* with reference to the future development of the "iron cage" of rationalism Weber writes: "No one knows who will live in this cage in the future or whether at the end of this tremendous development entirely new prophets will arise or there will be a great rebirth of old ideals and ideas or, if neither, mechanized petrification embellished with a sort of convulsive self-importance" (1958, p. 182). From this passage, published first in 1905 and intended by Weber for republication in 1920, it seems obvious that Weber was essentially uncertain as to the character of social life that would come to pass as the capitalist era continued to develop. Here again, Hechter's sense of Weber's observation concerning the transition from affinity based on status to affinity on the basis of class would seem to be in error.

Although I applaud both the theoretical and analytic work that has been presented by Hechter, it is indeed ironic that Hechter should present observations that suggest the complexity of the relationship between ethnicity and the capitalist economy within the context of a critique of Weber's work. In making his observations and drawing his conclusions, Hechter in fact is working toward an analysis that will actually extend and enhance Weber's work.

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#### RESPONSE TO COHEN: MAX WEBER ON ETHNICITY AND ETHNIC CHANGE

##### I

It is apparent that Max Weber's famous discussion about classes and status groups continues to be misconstrued at this late date. For Weber, the distinction between class and status is an analytic one. An individual may be in a particular class and belong to a particular status group simultaneously. Further, status groups can be composed of individuals of several classes; likewise, classes are usually composed of individuals of several status groups. But there is a profound difference in these two bases of association. Status-based action is, by definition, taken by an individual *acting as a member of a particular community* with which he shares a specific "style of life" as well as a given quantum of "social honor." On the other hand, class-based action is essentially *individualistic*. A person's class position, for Weber, depends only upon his position in the marketplace; this, in turn, decisively affects his "life chances."

In so far as discrete acts are concerned, class and status orientations may best be thought of as competing types of identification. To illustrate: When a man enters a voting booth in order to express his political preference on a given issue, to the extent that his class and status interests diverge he will most likely be faced with a difficult decision. A successful black

businessman must decide if his vote will be determined by his economic position as a capitalist or by his feelings of solidarity with other blacks of all stations. For the purposes of argument, assume that these interests do imply opposing political preferences in a given election, but that there is only one chance to vote. In this case, the decision will tend to reflect that identity which is most salient for the individual with respect to politics, in this specific historical situation.

Every society in which exchange of any kind occurs, that is, in which the market relation has been established, has both status and class elements. In those societies in which the greatest proportion of individual acts are carried out as a function of membership in a community or group of one sort or another the status principle may be said to predominate. In those societies where the greatest proportion of individual acts are not constrained by such memberships, the class principle may be said to predominate. In these circumstances, Weber feels it is appropriate to refer to status and class societies, respectively (1968, p. 306).

Once this distinction is made, it immediately suggests two critical questions: What are the conditions leading to the predominance of one or the other principle of association (1) at any given point in time, and (2) in the course of long-term social change? The first question leads to Weber's definition and conceptualization of ethnicity; the second to his theory of ethnic change. This note will take up these aspects of Weber's work seriatim. It will be seen that Weber's treatment of the former question is considerably more satisfactory than his treatment of the latter.

## II

Several prescient ideas concerning the nature of ethnicity are buried in *Economy and Society*. For this reason alone, no serious student of ethnicity can afford to ignore Weber's relevant writings. Because much of this discussion of ethnicity is so obscure, it may be worth elaborating briefly.

Weber rejected the position, still held by many contemporary writers, that ethnicity is an irrational and primordial "given" in the analysis of social groups. In his conception, ethnic identity is clearly a social construction. Hence, its existence is always problematic: "We shall call 'ethnic groups' those human groups that entertain a subjective belief in their common descent because of similarities of physical type or of customs or both, or because of memories of colonization and migration; this belief must be important for the propagation of group formation; conversely, it does not matter whether or not an objective blood relationship exists. Ethnic membership (*Gemeinsamkeit*) differs from the kinship group precisely by being a presumed identity, not a group with concrete social action, like the latter" (p. 389). It is impossible to specify the kinds of similarities of

custom around which ethnic identity may crystallize: any particular cultural differences can lead to a consciousness of kind (p. 387). While it is true that language and religion frequently serve as salient cultural *differentiae*, neither difference is necessary for the development of group consciousness. This consciousness of kind, in turn, leads to the formation of social circles (*soziale Verkehrsgemeinschaft*); these appear to be the obverse of Frederik Barth's (1969) notion of the ethnic boundary.<sup>1</sup> The actual extent of cultural difference between two groups is irrelevant to the emotional impact of the ethnic tie which unites the individuals within each of them.

All differences of customs can sustain a specific sense of honor or dignity in their practitioners. These differences often come to have an "ethnically repulsive effect" (Weber 1968, p. 391) because they are thought of as symbols of ethnic membership. Weber notes, in passing, that this repulsion need not always rest on the symbolic character of the distinguishing traits. The fact that the Scythian women oiled their hair with butter, which gave off a rancid odor, while Greek women used perfumed oil to achieve the same purpose, apparently thwarted all attempts at social intercourse between the aristocratic women of the two groups. Repulsion is engendered between two groups because they find each other's customs peculiar and because "the different custom is not understood in its subjective meaning since the cultural key to it is lacking" (p. 387).

But what caused cultural differences to emerge or persist among groups in continuous contact? Here, Weber is rather vague: pronounced differences of custom are usually caused by the "diverse economic and political conditions of various social groups" (p. 392). While this formulation does not preclude the salience of a cultural division of labor (Hechter 1975), the problem is that it is too unspecific to be of much value to the modern researcher.

However, Weber's analysis of monopolistic closure, as a general tendency of economically organized groups, provides a succinct account of the process by which the cultural division of labor is established. Once a particular cultural *differentia*, no matter how apparently trivial it is, becomes salient, it can serve as a starting point for the tendency to monopolistic closure (1968, p. 388). In periods where there is a contraction of economic resources,

the participants become interested in curbing competition. Usually, one group of competitors takes some externally identifiable characteristic of another group of (actual or potential) competitors—race, language, religion, local or social origin, descent, residence, etc.—as a pretext for at-

<sup>1</sup> Max Weber was not alone in anticipating the concept of the ethnic boundary. Elsewhere, I have argued (Hechter 1973) that Karl Marx's essays on the Jewish Question (1964a, 1964b) implicitly point to this concept as well.

tempting their exclusion. It does not matter which characteristic is chosen in the individual case: whatever suggests itself most easily is seized upon. Such group action may provoke a corresponding reaction on the part of those against whom it is directed.

In spite of their continued competition against one another, the jointly acting competitors now form an "interest group" toward outsiders; there is a growing tendency to set up some kind of association with rational regulations; if the monopolistic interests persist, the time comes when the competitors, or another group whom they can influence (for example, a political community), establish a legal order that limits competition through formal monopolies; from then on, certain persons are available as "organs" to protect the monopolistic practices, if need be, with force. [Pp. 341-42]

All told, then, Weber's discussion of ethnicity is as conceptually sophisticated as that of the best of our modern writers. For those contemplating research on problems of ethnicity, Weber's analysis is one of the mandatory starting points. However, his notions about ethnic change must be approached with due caution.

### III

Weber's theory of ethnic change is less well developed than his description of ethnicity *in stasis*. He offers only scattered *aperçus* concerning the long-term fluctuation of the principles of status and class in history. If ethnic boundaries are a natural consequence of the tendency toward monopolistic closure, there is no adequate explanation of the structural antecedents of closure. Hence, the dynamics between tendencies to closure or openness within particular groups can only be guessed at in his analysis. True, Weber does state that initially open economic relationships tend toward closure "when the number of competitors increases in relation to the profit span" (1968, p. 341). But in what types of situations does this tend to occur? What governs the relationship between "the number of competitors" and "the profit span" in any case? Whereas Marx, for example, might push the analysis in the direction of the social structure, to include such concepts as "the reserve army of labor," on the one hand, and the tendency of "the declining rate of profit inherent to mature capitalism," on the other, Weber appears content to explain closure by giving intervening variables a causal status.

This hesitancy on Weber's part may be due to his well-known reluctance to construct *any* explicit theories of long-term social change. In reaction against Marx, he disdains the search for mono- or even multicausal explanations of social change: "I would like to protest the statement . . . that some one factor, be it technology or economy, can be the 'ultimate' or 'true' cause of another. If we look at the causal lines, we see them run, at one time, from technical to economic and political matters, at another



from political to religious and economic ones, etc. There is no resting point. In my opinion, the view of historical materialism, frequently espoused, that the economic is in some sense the ultimate point in the chain of causes is completely finished as a scientific proposition" (1968, p. lxiv). Of course, this reluctance enables Weber to hedge his bets about social change. It also permits a degree of ambiguity and internal inconsistency which any theorist explicitly concerned with the causes of change would find uncomfortable.

As is the case with all literature which some hold to be scriptural, in perusing the corpus of Weber's work it is easy to find ideas to suit a broad range of tastes. That is one reason why he is always stimulating to read and to reread. It is also why his work has inspired so much debate between rival interpreters.

However, when Weber, ever so infrequently, does permit himself the luxury of speculation about long-term trends in patterns of stratification, he clearly aligns himself with the position I have termed the "functionalist theory of ethnic change": "When the bases of the acquisition and distribution of goods are relatively stable, stratification by status is favored. Every technological repercussion and economic transformation threatens stratification by status and pushes the class situation to the foreground. Epochs and countries in which the naked class situation is of predominant significance are regularly the periods of technical and economic transformations. And every slowing down of the change in economic stratification leads, in due course, to the growth of status structures, and makes for a resuscitation of the important role of social honor" (Weber 1968, p. 938). Since one of the hallmarks of industrialization is that it tends to routinize "technical and economic transformations," the above statement clearly implies that the class principle should overcome the status principle in industrial societies. This is one of the hypotheses my analyses (Hechter 1976) were designed to test; as it happens, it did not appear to be verified.

But this is not the only example of functionalist theorizing about change to be found in *Economy and Society*. For purposes of illustration, it will be sufficient to call attention to another, less familiar example. In part 2, which deals broadly with the economy and the economic relationships of organized groups, there is a brief discussion of the causes of the disintegration of the household. In preindustrial societies, the household was, of course, a much more important institution than it has subsequently become in the wake of industrialization. Why has this particular transformation occurred? The answer, according to Weber, is that

in the course of cultural development, the internal and external determinants of the weakening of household authority gain ascendancy. Operating from within, and correlated with the quantitative growth of economic means and resources, is the development and differentiation of abilities

and wants. With the multiplication of life chances and opportunities, the individual becomes less and less content with being bound to rigid and undifferentiated forms of life prescribed by the group. Increasingly he desires to shape his life as an individual and to enjoy the fruits of his own abilities and labor as he himself wishes. . . . Even where the household unit remains outwardly intact, the internal dissolution of household communism by virtue of the growing sense of calculation (*Rechenhaftigkeit*) goes on irresistibly in the course of cultural development. [Pp. 375-76]

It would be difficult to find a more pristine statement of the functionalist theory than this, though not so difficult, perhaps, to find a more precise one. What, one wonders, does the phrase "in the course of cultural development" actually entail? If such generalizations seem ill founded in retrospect,<sup>2</sup> it is perhaps understandable that some contemporary partisans of Weber's style of sociology seek to downplay these theoretical indiscretions, thereby presenting the most favorable image of the Master's work. The easiest way of doing this is to argue that these theoretical speculations are uncharacteristic of Weber's mode of analysis and as such may be largely dismissed from serious consideration.

However, this kind of intellectual defensiveness is not only misplaced; it is also fundamentally wrongheaded. Good theories exist precisely to be superseded. Conversely, a so-called theory which cannot be disproven is really no theory at all. Is not this problem really the biggest of the skeletons in the structural-functionalist's closet (Smith 1973)? No important theorist is well served by those who seek to protect his propositions from rigorous empirical verification.

Even if Weber's occasional lapses into explicit theoretical generalization about long-term social change are momentarily put aside as being somehow uncharacteristic mistakes, it cannot be denied that there is, in his most characteristic analytic tool—the construction of the ideal type—an implicit theory of social change (Bendix 1974; Roth 1971). The discussion of the transition from patrimonial to bureaucratic domination provides a case in point. While Weber may have insisted, and rightly so, that certain patrimonial elements persist in the most highly bureaucratically organized

<sup>2</sup> Edward Shils's (1957) cautionary tale about the persistence of primordial ties in the industrial setting can be seen as an attack on the evolutionary part of Weber's position. And Immanuel Wallerstein has convincingly argued that Weber's theory of ethnic change must actually be turned on its head: "Status honor is not only a mechanism for the achievers of yore to maintain their advantages in the contemporary market, the retrograde force described by Weber; it is also the mechanism whereby the upward-strivers obtain their ends within the system. Weber was wrong. Class consciousness does not come to the fore when technological change or social transformation is occurring. All of modern history gives this the lie. Class consciousness only comes to the fore in a far rarer circumstance, in a 'revolutionary' situation, of which class consciousness is both the ideological expression and the ideological pillar" (1972, p. 219).

societies, the thrust of his argument is that, in the long run, bureaucratic domination supplants patrimonialism in sovereign states. This is bound to occur because of the inherent technical superiority of bureaucratic organization. For states competing with one another in a world economy such a consideration simply becomes imperative.

In sum, when Weber turns his attention to matters of long-term historical change, his position tends to converge with that of more explicitly developmental theorists like Marx.<sup>3</sup> With respect to this range of substantive, as opposed to methodological, problems, the supposed differences between the analyses of Weber and Marx have often been overdrawn (Turner 1974, pp. 171-84).

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<sup>3</sup> Some reasons for this almost universal convergence of 19th-century theories of ethnic change are discussed in Hechter (1976).

COMMENT ON "THE VILLAGE INFLUENCE ON PUNJABI FARM MODERNIZATION"<sup>1</sup>

Sandhu and Allen (*AJS* 79 [January 1974]:967-80) intend to evaluate the effects of farmers' personal characteristics (land size, education) and social context (village-level organizational help and communication) on the adoption of modern farm technology. They claim to demonstrate the "primacy of village characteristics over the individual factors" (p. 967). While the attention given to the subject is laudable, errors in sampling, measurement, and analysis render such a conclusion invalid and obscure the nature of social relations in agrarian communities.

The data were gathered in 495 interviews with farmers in 93 different villages. In 55 villages the authors report an average of eight respondents per village (implying some variance), while in the remaining 38 villages they interviewed a total of only 55 respondents. Obviously, some of the latter villages contained only one respondent. How good an estimate of village characteristics could be expected from such samples? The matter is scarcely simplified by the omission of information on exactly how village-level characteristics were estimated. Presumably, they were averages of indexes involving a series of questions answered by individual farmers, although the methods used to construct such indexes are again not reported beyond an indication that the items were weighted and summed. Furthermore, the use of state agricultural inspectors as interviewers might easily bias the results of an interview focused on participation in state agricultural programs.

The measurement of wealth or resources as the number of acres of land owned by an individual ignores wide differences in land value. It also slights problems stemming from the registration of ownership in different family members' names to avoid adherence to government land ceilings. Considering the importance of resources in the adoption of innovation and the reputed importance of stratification variables in India, the authors' measurement of resources leaves much to be desired.<sup>2</sup>

However, these difficulties are almost trivial compared to those involved

<sup>1</sup> I would like to thank Woody Beck, Ken Westhues, Carolyn Elliott, T. R. Balakrishnan, and R. D. Sharma for their thoughtful comments on my comment and to apologize for any contamination they may have suffered from exposure to the article. C. David Gartrell arrived independently at some of the same conclusions.

<sup>2</sup> Given their use of respondents' educational status, the statement that "landholding size was the only indicator of the respondent's socioeconomic status" is somewhat mystifying. In measuring land resources the authors could have used respondents' estimates of land value for the whole household or the estimate of land value generated by the land-consolidation authorities. There are, of course, other significant aspects of status, such as the prestige accorded to commissioned army officers.

in their measurement of the "intervening" village-level variables. Besides not revealing how village-level estimates were made or how indexes were constructed, Sandhu and Allen's verbal report of the content of indexes of modernization, organizational help, and communication suggests a number of tautologies. Questions about organizational help included ones about sources of financial help (including village cooperatives), sources of advice (Department of Agriculture), and attendance at "agricultural demonstrations, lectures, and training camps." Communication was defined to include "visits to the agency," "agency advice," and "attending a lecture." The content appears to overlap somewhat, and the picture becomes even murkier when we are informed that the dependent variable, adoption of modern farm technology, includes membership in a village cooperative and the number of loans taken from it, thus overlapping organizational help. When the number of times agricultural experts were consulted and demonstrations or training camps attended is included in modernization, we have a rare example of a three-way tautology (with organizational help and communication)! Finally, the number of magazines subscribed to is included in both modernization and communication. While these indexes probably contain some conceptually independent content, the fact that they are highly intercorrelated (see fig. 1) is hardly satisfactory proof of the primacy of village characteristics in determining modernization.

Sandhu and Allen's discussion of the effects of education and resources on modernization introduces problems with confounded effects. Better family resources probably produce both higher educational opportunities and the opportunity to adopt new technology, while education itself subsequently improves opportunities. However, when the authors move to a multivariate analysis, their model is far from appropriate to the task, even if we ignore problems of feedback. They claim that their multiple-regression results suggest "that the antecedent variables (individual characteristics) are less influential than the intervening variables (village characteristics), and that the participation level is not much affected by variation in farm size if the extremely small holdings are excluded" (p. 976).

Unfortunately, their model does not examine this claim empirically but instead treats it as an assumption. Their specification of the path model reproduced in figure 1 actually omitted  $p_{51}$  and  $p_{52}$ , the direct effects of individual characteristics (farm size and education) on modernization! However, Sandhu and Allen did report their correlation matrix (reproduced in fig. 1); it allows the recalculation of path coefficients using IPA (Interactive Path Analysis [Nygreen 1971]) for the fully recursive model which includes  $p_{51}$  and  $p_{52}$ .<sup>3</sup>

<sup>3</sup> The fully recursive model used here also includes  $p_{41}$  and the residual  $p_{3a}$  and  $p_{4b}$ , which Sandhu and Allen omitted from their model for some unstated reason. Assuming the latter omissions to be simply an oversight, their model is still overidentified, and

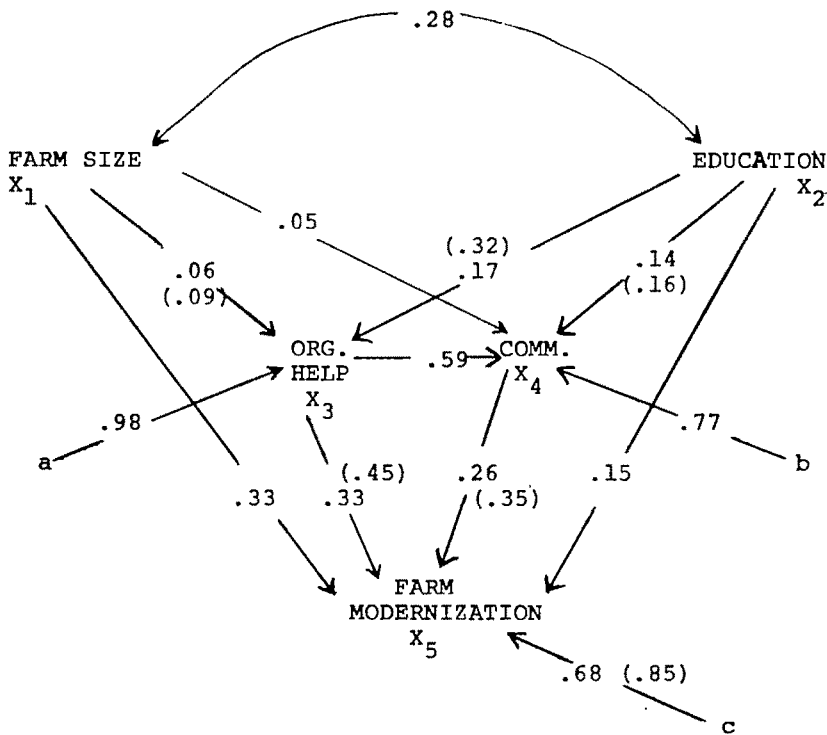


FIG. 1.—Village influences on farm modernization: a fully recursive model.

ZERO-ORDER CORRELATIONS

$X_1$ .....	...			
$X_2$ .....	.28	...		
$X_3$ .....	.11	.19	...	
$X_4$ .....	.16	.27	.62	...
$X_5$ .....	.45	.38	.56	.56

The results demonstrate that, contrary to Sandhu and Allen's conclusions, individual characteristics are indeed powerful determinants of modernization. While the addition of  $p_{41}$  makes little difference and  $p_{31}$  could be omitted without any substantial loss of predictive accuracy (both are less than twice their standard errors), the addition of  $X_1$  and  $X_2$  as direct determinants of  $X_5$  increases the percentage of variance explained from 39 to 54. Both  $p_{51}$  (.33) and  $p_{52}$  (.15) are significant. Even with such a crude measure of wealth as acres of land, resources directly condition the

many different solutions are therefore possible. This may explain my inability to recalculate their results using their specification, but the possibility remains that their original path coefficients were incorrectly calculated. In figure 1 their results are entered in parentheses.

adoption of modern farming technology. Village-level organizational help and communications do not intervene significantly between acres of land and modernization of farming, and the sum of the indirect effects of education on modernization through village-level characteristics represents only about one-third of their total correlation. Also, my recalculations suggest that the direct effects of village-level variables are somewhat less powerful than do the results presented by Sandhu and Allen (fig. 1, in parentheses).

Sandhu and Allen's focus on the comparison of village and individual effects makes the lack of a complete analysis of all variables at both levels somewhat puzzling. They do not use average land size or average education as village characteristics nor do they analyze organizational participation and communication as individual attributes, despite the fact that they were measured that way. Their analysis omits (1) explicit consideration of the between-village variance in individual characteristics, (2) the within-village component of their village characteristics, and (3) the interaction between individual and village variables. However, given their research design and particularly their sampling, such an analysis can feature an average of only eight observations for 55 villages and only one or two observations for each of the remaining 38.

The authors see the adoption of new technology as a function of such village effects as peer emulation (dropped from the multivariate analysis without mention because it correlates well with nothing!), organizational help, and communication. They write as though all action were voluntary, as though agencies did not discriminate between the rich and the poor, and as though conflict between political factions had somehow disappeared. The Punjab may be relatively homogeneous in terms of caste and religion (Jat-Sikhs), but that only makes it atypical of India and is no excuse for ignoring inequality and the struggle for power. This distortion climaxes in the labeling of the largest landowners as "subsistence" farmers. Such individuals own more than twice the all-India average acreage for households, and they have far more tractors and tube wells than elsewhere in India. If the richest farmers in the richest districts in the richest state are "subsistence," what does that make the rest of the Indian rural population?

Sandhu and Allen's study ignores agricultural laborers because, of course, they do not figure in the adoption of modern farm technology. The adoption of cash payments for hired labor is one of the changes central to the development of an agrarian proletariat, but it is not even mentioned. Such research strategies simply adopt the biases that government development programs have long exhibited.

I would reinterpret their results to emphasize that those with higher education are well connected. They possess the communications and organizational contacts which facilitate reaping the benefits from development

programs. Big landowners can take advantage of modern technology, which is often costly and relatively indivisible (particularly tube wells and tractors). Laborers' share of the produce has gone down and small farmers are driven to ruin, while large landowners receive development inputs and are taxed only lightly in the bargain (Frankel 1971). This may well be the "orientation to modernization which replaces traditionalism" in India, but it is necessary to recognize it as the development of capitalist agriculture. The Green Revolution has produced more food, but it also has produced greater inequality. As Wharton (1969) has noted, it may yet turn out to be Pandora's box rather than a cornucopia.

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#### RESPONSE TO GARTRELL'S COMMENT

We appreciate the careful attention Gartrell has given our article. He is right that 55 respondents interviewed from 38 villages may not represent their villages adequately. But, even if this part of the sample is eliminated, the findings remain unaltered. In fact, the influence of the village was evident in table 6 with a sample of 276 farmers and 38 villages (p. 978). These villages were equated in all physical and geographical characteristics to seek out the influence of social-psychological factors (pp. 969 and 978).

The village characteristics in terms of educational, commercial, and co-operative development were assessed by a number of appropriate institutions such as schools, libraries, and shops in the village (p. 970). The interactional characteristics of agency help, communication, and peer emulation were based on the series of questions answered by the respondents living in those villages. There was no overlap in the questions in these three areas. The agency questions were addressed to the source and volume of help, and the communication items were related to frequency, medium, and practicality of information on agricultural methods. The



dependent variable of farm modernization was based on a much larger area of information, and the overlap of intervening variables was negligible. It is not a tautology. The use of agricultural inspectors as interviewers did not bias the results substantially because they were well briefed and closely supervised.

We agree that the land varies in value depending on its quality; this difference was equated by the use of "standard acres." As regards the issue of his personal land versus land possessed by members of his family, each farmer was asked to indicate the land cultivated by him, whether owned or rented.

Path analysis provides a rather flexible system for evaluating a particular hypothetical causal sequence among a given group of correlated variables. It proves nothing, but it indicates a correspondence or lack of correspondence between the hypothesized order of effects and the path coefficients. As more paths are partialled out of the total variance, the proportionate "share" of each path coefficient declines within the limit of the explained variance. The unexplained or "residual" variance remains the same, approximately, for a given set of correlated variables. This fact points up an oversight in the model as recalculated by Gartrell. The sum of the squares of the four path coefficients and the residual path coefficient equals one. Thus,  $.33^2 + .33^2 + .26^2 + .15^2 + .83^2 = 1.00$ , so that the residual path coefficient is not .68 as shown in the recalculated figure but its square root, approximately .83. Gartrell is wrong in assuming that the calculation of the three additional path coefficients could increase the amount of explained variance.

Our preference in calculating the recursive path-analytic model with only six internal paths rather than the nine suggested by Gartrell was based on the belief that the simpler model was more direct and a better representation of the dynamics of the variables in the system. The focal dependent variable, farm modernization, is a process in which change and progressive development are implicit. The two intervening variables in our treatment, organizational help and communication, are also dynamic processes which relate immediately to modernization. Our path model tests these process variables in the intervening variable position. We have no quarrel with Gartrell if he wishes to cast the variable set into some other configuration and analyze the relationships with reference to some alternative hypotheses. Our hypothesis, however, treated farm size and education as antecedent variables in the full and literal sense of the term because these were relatively static preconditions which were generally beyond the near-term control of the typical Punjab farmer. We used the sequence static antecedent variables, dynamic or process intervening variables, and the dynamic dependent variable as a logical model to stress the function of the more readily manipulable factors in the system. Our treatment is

quite consistent with the relative magnitude of the correlations in the system, from which we developed this line of reasoning.

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COMMENT ON HAUSER'S REVIEW OF *EDUCATION, OPPORTUNITY, AND SOCIAL INEQUALITY*

Robert Hauser's review of *Education, Opportunity, and Social Inequality* (*American Journal of Sociology* [January 1976]: 911-28) contains useful suggestions and comments. However, my general impression is that he has not understood the very objective of my book, probably because I have not made this objective clear enough: to explain, in the sense of making intelligible, a number of apparent paradoxes produced by empirical research on social mobility. Here are some of these paradoxes:

1. Inequality of educational opportunity (IEO) decreased more or less regularly in all Western societies during the two decades after World War II. That is, the ratio of the probability of, say, a higher-class son to the probability of a lower-class son going to college declined. However, this decrease had no impact on the structure of intergenerational mobility, although educational attainment is a determinant of status.

2. It had no evident impact on income equality either, although educational attainment is a determinant of income. Obviously, these first two points are paradoxical with respect to the beliefs of many sociologists and economists for whom IEO is an essential factor responsible for other forms of inequality and for their intergenerational transmission.

3. Countries very different with respect to IEO appear as not markedly different from one another with respect to mobility. This is the classical Lipset-Bendix paradoxical finding.

4. Even in countries where IEO is lowest, it is still considerable. That is, the disparity ratio between higher and lower classes as far as college attendance is concerned can be as high as 10 when it is lowest. Is this fact immediately intelligible?

5. Mobility is generally weakly related to educational attainment measured absolutely or relatively to father's educational attainment. If not Hauser, C. A. Anderson in his pioneering paper (1961) had, before me, the impression that this fact was not immediately clear.

6. Sophisticated techniques such as path analysis have been intensively used in recent years by stratification researchers. They have led to many

interesting results, but also to some findings which are hard to interpret: why is the proportion of the variance of the usual dependent variable, son's achieved status, explained by father's SES, father's educational attainment, son's educational attainment, etc. regularly smaller than the part not explained by these factors? Obviously, explained variance is often small in the social sciences. But is not trying to know what is behind this unexplained variance an interesting question? Or should we wholeheartedly be content to interpret the unexplained variance as objective randomness?

7. Another paradox of causal modeling is that, when it is applied to a sequence of cohorts, the values of the path coefficients change moderately from the first to the last cohort. Meanwhile everything has changed in the world: the educational system has grown tremendously, IEO has decreased, etc. Why then are the path coefficients almost stable? The question is at least worth asking.

I could make this list longer. The seven points are probably sufficient, however, to illustrate what was my aim—to try to answer a set of questions, not of the *how much* type, but of the *why* type: Why does IEO remain so high in spite of all the efforts to reduce it? Why has the decline in IEO not provoked a decrease of intergenerational inheritance, even though educational attainment is a powerful determinant of status? Why has it not provoked a decline in income inequality, even though educational attainment and income are correlated? etc. In spite of the accumulation of quantitative research, I had, rightly or wrongly, the impression that with a few exceptions such as Thurow's (1972) paper, "Education and Economic Inequality," in the literature no clear answer had been given to these basic questions. Thurow's paper, which I discovered when my own work was almost completed, was a great comfort to me. It makes the point that the impact of educational equalization on other forms of inequality can be much better understood if it is interpreted as dependent on a simple process in which people are ranked according to educational attainment and allocated to the available ranked social positions. This notion, that the aggregate statistical relationship between education and status can be made more intelligible by deriving it from a simple queuing process, was already developed in my first paper on mobility (1970) and systematically used in *Education, Opportunity, and Social Inequality* (1974).

Given my objective, that is, to answer a number of questions of the *why* type, I came to the idea of building a model roughly describing the basic mechanisms responsible for educational and social inequality, to see whether it generated the "paradoxical" outcomes some of which are listed above. At one point, I thought of building a model fitted to French data but abandoned the idea—available were only the partial observation of a single cohort, some aggregate data on the composition of the student body at some levels for some points in time, and some other partial aggregate and

survey data. Briefly, the situation was far from the ideal one where data on a sequence of cohorts (including schooling and status acquisition) would be available. On the other hand, the OECD had gathered an important set of data from a number of Western countries. Hence my idea of building, instead of a model fitted to a particular context, a kind of ideal-typical model taking into account only some simplified basic mechanisms to check whether this model could account for a set of "qualitative" statements—statements of the "more-or-less" type (which could be derived from the OECD data as well as from other sources).

One additional reason why I used this strategy was that many data could not be taken at their face value. Hauser would teach me that, for instance, the assumption that fertility is the same for all classes is doubtful. I know. I realize that for countries such as Germany or France, where the proportion of immigrants in the working population increased during the last period of rapid economic growth, this assumption is even more than doubtful: a great proportion of the immigrants leave their families abroad, so that their *apparent* fertility for the French or German systems is close to zero. On the other hand, most of them are lower class, so that the OECD estimations of class disparities in educational attainment, which assume equal fertility in all classes, are probably overestimations. This means that these rates of disparity cannot be taken at their face value and that trying to build a model fitted to such data would be misplaced rigor. But since the proportion of immigrants increased during the period in question and the overestimation of disparities has probably also increased, the "qualitative" statement "disparity between lower and higher classes at level  $x$  has decreased" can be considered valid.

This is just an example illustrating why it was often difficult to consider the data collected at a quantitative level, and why I decided to retain exclusively the qualitative information of the more-or-less type they contained. This consideration reinforced my choice of building an ideal-typical, theoretical model incorporating exclusively, in a simplified fashion, some basic mechanisms responsible for inequality, so as to check whether it was able to explain the above paradoxes.

It should now be clear why my purpose was not to present a model fitted to any particular context. I did not think I would have to stress this point so heavily, but Hauser's review convinces me I must. To illustrate this point just by a simple example: some basic data on IEO, namely, that on the high degree of IEO, derive from the exponential mechanisms necessarily generated by any school system. As my objective was to build an ideal-typical model, I used the strategy of simplicity in describing these exponential mechanisms: I assimilated the ideal-typical school system to a sequence of dichotomous branching points and assumed, furthermore, that the parameters associated with a given class of students are the same for all

branching points. Hauser informs me that this is not realistic. I know. I know that in France, for instance, whether one goes on to college and whether one survives from the second to the third year of secondary education are alternatives associated with very different parameters. I know that, again, in the case of France (and many other countries), a student who has left the "royal road" leading to college can eventually come back to it. I know that the assumption that achievement is constant throughout the schooling of a student is unrealistic. In a system like the French one, selective as it was in the period under consideration and still is to a large extent, any student tends to be surrounded by peers who are, on the average, increasingly better over time, since they are selected at each stage.

All these assumptions are simplifications, which is another way of saying that they are unrealistic with regard to any concrete school system. But it must be added that a school system will always generate exponential effects, though in any concrete case these effects will be more complicated than I have assumed: one does not attend a prestigious higher-education institution at age 22 because on one occasion one decided to do so, but because one successfully met a sequence of obstacles during schooling and made at these points the more favorable decisions. Though these exponential mechanisms are omnipresent, the artificiality of the assumptions which, for the sake of simplicity, I have introduced to describe them means that when the model is confronted with data in a particular context discrepancies must occur. But, on the other hand, the model contributes to making more intelligible some basic facts. It is not able to predict the degree of IEO in a context, but it explains *why* it is high. It explains, also, why the ratio of the probability of a higher-class youngster to the probability of a lower-class youngster attending college decreases over time, while in many cases the difference of the two probabilities does not; why both the ratio and the difference decrease at earlier educational levels. It clarifies the impact of early cognitive differences between classes, as measured by school achievement at early stages in the schooling, on class differences in college attendance, to limit myself to a few points. To Hauser, these outcomes are trivial. Maybe. But if these exponential mechanisms are so evident, I wonder why linear models are so frequently used in stratification research.

One reason for Hauser's disagreement with me is probably that he does not accept the idea that there are several types of models. One type comprises what are basically descriptive models, such as path models, whose purpose is to answer questions of the *how much* type: how much does educational attainment account for status achievement, etc. Another type includes explanatory models whose aim is to answer questions of the *why* type, that is, to make a phenomenon or a set of phenomena more intelligible. Descriptive models have to be fitted to data. Explanatory models

may ultimately fit no data at all and nevertheless increase our understanding of the phenomena they consider. A good example of this extreme case can be found, for instance, in Thomas Schelling's "Dynamic Models of Segregation" (1971), which Schelling has never made any effort to fit to any particular context but which increases powerfully our understanding of segregation. His aim was to show that unprejudiced individuals can generate a segregated society, even though nobody wants segregation, as soon as everybody prefers not to be in a minority position. The important point was to show that such unintended aggregation effects were possible, using models as simplified as possible; it was to show the *logic* of the apparent paradox. My purpose in this respect was similar to Schelling's: to show that equalization of opportunity does not necessarily mean equalization of results in an ideal-typical world, one reduced to some basic mechanisms similar to those which can be observed in the real one. When Hauser says pedantically (p. 918) that my model does not represent the state of the art, he compares it with approaches aiming chiefly at description and prediction rather than explanation, showing that either he has never thought of the distinction between the two notions or thinks that descriptive models which are nicely fitted to data represent the only legitimate type of modeling. I recognize fully the importance of descriptive analysis, provided it is recognized theory also is needed.

Even assuming that a model has to be fitted to a particular context to be legitimate, it seems to me that, still, two attitudes are possible toward the model I have presented: one can either stress discrepancies between the model and data collected in some context in which one is interested and come out with the statement that the model is "wrong," or one can elaborate and modify the model so as to adapt it to this context. Hauser has chosen the first attitude. The question is whether it is the more fruitful and open-minded one. Many comments in Hauser's paper derive from the implicit statement that the first attitude is the best one: the model is wrong since it fails to predict some American data. Is that not too simple? In other words, Hauser rejects the possibility of building a general theory of phenomena which appear in various forms in a variety of contexts. Unless, instead, he believes that I should have tried to reproduce exactly all the available data, in all Western societies, before saying anything about the general phenomena in which I was interested.

I will now comment briefly on some of Hauser's particular points less directly connected to this basic misunderstanding.

On my criticism of the "value theory." What I have tried to say is that a theory which tries to account for IEO exclusively by the assumption of value differences between classes cannot account for the fact that school attainment varies with school achievement, IQ, etc. This variation suggests that the family units and/or the individuals involved take into con-

sideration the risks, costs, and benefits of spending  $x$  more years at school. A bad achiever may see that trying to follow some prestigious curriculum could be rewarding but also take into consideration the fact that the probability of being successful is low. Furthermore, I introduced the assumption that a higher-class student will consider it more "useful" to reach a given educational level than a lower-class student with similar achievement, since the former is more exposed to demotion. Why are these statements so unreasonable? I see that Hauser does not like them, not that they have to be rejected.

On interaction. There are interaction effects in the tables I presented, though they are complicated.<sup>1</sup> Taking care of this nonlinearity can be useless from a descriptive viewpoint. It is essential if the purpose is to *understand* the processes behind the tables. The nonlinear effects suggest the plausible explanation that there is a tendency in the higher class to try to maintain a high level of aspiration except when achievement is below a relatively low threshold and a tendency in the lower class to have a high level of aspiration when achievement is above a relatively high threshold with a decrease below. This type of structure appears more or less clearly in all the tables. It suggests, to me at least, fascinating questions and avenues of research. But to pursue this line requires that men not be considered as punch cards, as a set of juxtaposed variables, but that they be seen as actors, able and willing to take decisions depending on their resources and on the context. This is part of what I meant when I made the point that the kind of modeling I have tried is closer to the usual language of sociological theory than what Hauser calls the "standard representations of social mobility."

On the logical fallacy of affirming the consequent. My logic is not: if  $A$ , then  $B$ ; we observe  $B$ , whence  $A$ . It is: if  $A$ , then a set  $S$  of conse-

<sup>1</sup> For example, in the Danish table 2.4 of my book, if we consider the rows 2-4 and the columns 4-6 (reliable data), the increase in the dependent variable is greater the higher the class. The structure of the French table 2.6 is complicated indeed: when school achievement is good, workers' sons take a general course in strong proportion even if age is 12. If achievement is middle, the decrease with age in the proportion taking a general course is greater in the lower than in the higher class, etc. Using cell sizes (not given in the text) to eliminate age, the proportion in the higher class taking a general course is .98, .93, and .60 when achievement is high, moderate, and low, respectively; the corresponding proportions are .81, .43, and .18 in the lower class. Again an interaction effect appears. In the French table 2.3, the decrease in aspirations as a function of school achievement is weak among higher-class youngsters and strong among lower-class youngsters. Again, an interaction effect. In the case of table 2.1, there is a sharp increase in aspiration from the first to the second quintile for major white-collar, then a smooth increase; for middle or minor white-collar and skilled labor, there is a smooth increase, then a sharp increase from the fourth to the fifth quintile. In table 2.2, when SES is low, the decrease in the dependent variable is sharp from academic aptitude 5 to 4, but becomes smaller when SES increases. Also, going from aptitude 1 (low) to aptitude 2 makes little difference for the lower class and a great one for the higher class, though the increase with SES is not regular.

quences; this set of consequences is more or less congruent with reality, whence we can have a certain degree of confidence in *A*. This degree of confidence is the greater, the greater the number of consequences, the more counterintuitive some of the consequences, etc.<sup>2</sup> Still, I am convinced that, if not Hauser, many people have found it hard to understand why IEO can decrease without noticeable effect on mobility or income inequality; why IEO remains so high in spite of all efforts to reduce it; why mobility is not so different in countries different with respect to IEO; why education and mobility can be weakly related; why the correlation between education and status is moderate; why, although this correlation is moderate, trying to get as much education as possible is an advisable strategy; why the educational disparities between classes decrease at college level while the proportion from the higher class remains higher from one point in time to the next (this latter point is mentioned with surprise in some OECD reports); etc. Thus the theory generates a set of consequences congruent with reality, some of which at least some people found hard to understand. On the other hand, the basic axioms of the theory (cultural factors make school achievement a function of social class, people make decisions as a function of their social position and achievement, school offers a sequence of choices, the supply of social positions does not necessarily change to the same path as the social educational demand since the latter results from the combination of individual decisions, etc.) do not seem to me to be obviously wrong. I have never said the theory is true. On the contrary, I have insisted many times on its limitations. I have only said that I have a certain degree of confidence in it and that it could be considered false once a theory is produced whose basic statement is radically different in at least some respect and which explains the same set of consequences as well as some new ones.

On sensitivity analysis (p. 918). I agree it would be useful. However, given the number of parameters, exploring the parametric space would not have been a light task. I had the feeling the costs would be much greater than the benefits. I checked only some points which appeared crucial: for instance, that the structure of mobility changes little, both when the social structure does not change and when it changes to a slow path, both in the case where social background has no residual effect on status (besides its effect through education) and in the case where it has some effect, etc. Incidentally, if the model is as wrong as Hauser says it is, why is this costly exploration necessary?

On the effect of cultural deprivation on IEO (pp. 920–22). The model makes clear that the stronger the exponential effects, the more moderate

<sup>2</sup> I have tried to deal more extensively with these problems in "Théories, théorie et théorie" (Boudon 1971, chap. 6).



the effect on IEO of correcting early cultural deprivation. When exponential effects are reduced (for instance, when the undifferentiated part of the curriculum is made longer, or when disparity between classes with regard to the "survival" probabilities becomes lower), the relative effect of cultural deprivation is stronger and perceptible in the cohort for a longer time. Hence, I am not surprised to see a negative relationship between ability and SES at college level in France and a positive one in the United States, for instance. The discussion suggests, incidentally, the interesting policy point that measures to correct cultural deprivation will have a smaller or greater impact on IEO as a function of the state of the parameters affecting the exponential mechanisms. But, again, the model says very clearly that, depending on the parameters of the exponential mechanisms in a context, the correlation between ability and status can take different values.

On social structure (pp. 922-23). Hauser takes for granted that prestige scales are the last word on stratification. Some people have expressed some doubts in this respect. Hence my strategy: to use a theory of social structure as general and "empty" as possible. Indeed, the theory says only three things: there are classes, they are ordered, and the structure is roughly pyramidal. I was interested to see how far it was possible to go with such a simple stratification theory, avoiding the difficult discussions on this topic. Incidentally, when the social structure is gently reduced to a set of numerous, more or less convincingly ordered prestige categories and is no longer pyramidal, it is not surprising that the probabilities of moving up are, generally, no lower than the probabilities of moving down. But, although *de gustibus non disputatur*, I must confess that Glass's categorization of the social structure, for instance, though it probably does not represent "the state of the art" to Hauser, seems to me much more meaningful than the fine categorization based on prestige which apparently he prefers.

On IEO measurement. At several points in his review, Hauser makes the point that equality of educational opportunities can be measured in many ways. I could not agree more. Comparing the average number of branching points individuals survive to in the various social classes is a way of measuring IEO although, it seems to me, such a measure is hard to interpret. Counting branching points, even more than counting years of education, is  $\epsilon$ -ing together heterogeneous elements. Branching points are not dollars. They are even less so than years of education. On the other hand, when I ask "how many times more or less likely to go to college am I than my neighbor whose social origins are lower or higher," the meaning of the question is immediately clear. Probably for this reason IEO is in most cases, implicitly or explicitly, defined on the basis of a comparison of the conditional probabilities dependent on social class

of reaching such and such level of education. The advantage of such a definition is that it has a clear and simple intuitive meaning. Its shortcoming is that it does not easily provide an overall synthetic IEO measure.

In any case, if this definition of IEO is accepted, the model of my book generates a decline in IEO in the sense that, for each educational level, all pairwise disparity ratios between classes decline over time. Obviously, the relationship between this and other possible measures of IEO is not simple and would be worth analyzing (Hauser's comments on this point contain a useful positive suggestion). The IEO in my sense can decrease, and, for some states of the parameters, the variance of the distribution of educational attainments (to take this other possible measure) increase, while for some other states it will decrease over time. However, this does not affect, for instance, the outcome of the model that less IEO in my sense can be associated with more income inequality since, whatever the parameters, the model generates a tendency for the higher social positions to be filled by people whose educational level increases more quickly over time than in the case of those of the lower social positions. Also, IEO in my sense can decrease and the regression coefficient of educational attainment on social background remain constant. Take, for instance, the simple case where at some point in time 42% of higher-class and 3% of lower-class youngsters go to college, while at some later point in time the corresponding percentages are 49 and 10. This type of pattern over time is similar to the empirical patterns empirically observed in many Western countries. It is a consequence of the exponential logic I have tried to describe. Now, in this case, the regression coefficient of educational attainment on social background remains constant, while the disparity ratio between classes is about three times smaller at the second time point than at the first one. That is, higher-class youngsters are 14 times more likely than lower-class ones to go to college at the first time and less than five times at the second time. But the regression coefficient says no change. This says once more that it is necessary to look behind such coefficients to explore what they mean. No change in the regression of education on background does not necessarily mean no decrease in IEO. I do not feel responsible for this "contradiction."

As regards social mobility, finally, my objective was explicitly to check, with the help of the model, the belief that a decrease in the disparity of educational attainments between classes should increase social mobility. Many people considered this belief trivially true. Hauser (p. 925) finds it trivially false. This shows that the question as to whether it is true or false is not "trivial" and that an analytical device of the type I have used, even if it needs refining, can be useful in answering it.

On the box model (pp. 923-24). I know it lacks symmetry and the structure of the coefficients is not simple. I do not know whether or not

it "deserves wider application," unless symmetry is the ultimate criterion of validity.

On the adjustment of schooling and occupational distributions (pp. 926–27). I know that this adjustment involves a "delicate and volatile interplay of many social forces," which is for Hauser a delicate and volatile way of saying that he prefers not to take this problem into consideration. I understand why not. Take, for the sake of simplicity, the situation where two educational levels (high/low) and two types of social positions (high/low) can be distinguished. Imagine, furthermore, 50 people come to the status market with a high level of educational attainment and 50 with a low level. Suppose also that the status market is not delicate and volatile and that 30 higher and 70 lower-status positions are available. In such a case the squared correlation coefficient  $\phi^2$  between the two variables cannot be higher than  $3/7$ , which means that the value of the correlation (or of many measures of association) will have little meaning if the structural constraint is not taken into account. Now, can we consider the market volatile enough, so as to ignore such constraints? I know that sometimes positions are created thanks to the fact that people are available who have the competence to fill them. But the most realistic assumption is that this adjustment is true exclusively at the "local" level. At the macrolevel, the assumption of a strong inertia of the change in social structure with respect to the change in educational distribution is the only one which is compatible with the data of my chapter 9 as well as with other data—for instance, that published in Thurow's paper previously mentioned.

On miscellanea. Hauser's quotation on page 924 is ". . . explain," whereas I wrote "could explain." What I presented as a possible explanation is transformed into a dogmatic assertion. On page 920 Hauser writes: "If one takes the hypothesized time path of the model seriously, inequality of educational opportunity *will* disappear eventually." What the model says is that inequality at college level *would* disappear when (if) all went to college. A tautology. But I made clear that the model was intended for explanation, not for prediction (p. 201, for instance). On page 922: "Boudon says there is increasing equality of educational opportunity (p. 102), but the data are equivocal. While the ratios of graduation rates between the upper and lower social categories decline over time, the absolute differences between rates of college graduation increase over time." True. But I have repeatedly tried to explain (pp. 97, 100, 102, etc.) how this "equivocal" equalization resulted from the exponential mechanisms incorporated into the model and to show that it is a correct description of what happened in many Western countries. Page 11: "Boudon's conclusions (pp. 109–14, not in this order) are sweeping." They would probably appear less sweeping if the original order were conserved and if the statements referred to were not isolated from their context.

Hauser considers my work as an example which should not be followed. I have always been envious of those who know what should be done. But I always thought that "try, and see what happens" was a better though less secure line of behavior as far as research was concerned than following a well-explored trail—for instance, applying to data statistical techniques which are mechanically well-known and which always "work." Try, and see: that means not knowing in advance what should be done. Hauser does. I do not. Except on one point. I know that Hauser's review, with the exception of a few positive suggestions, is exactly what should not be done, since, it seems to me at least, the first thing to do in a review is to make sure that the author's objectives are properly understood, however deviant they might be.

Unfortunately, Hauser has understood neither the objective nor the results of my analysis. He sees it as a "reification of the notion, held by human capitalists and some stratification researchers, that schooling is *only* an investment and not a consumption good" (p. 927; my italics). What I have tried to show, actually, is that during at least two decades people have played a complicated collective game which includes many subgames. One of these subgames is cooperative: by demanding more education than similar individuals would have demanded before, each individual has contributed to his own enlightenment and to the enlightenment of all. Certainly by so doing he has also contributed to increasing his own share of the goods produced by the society and probably the share of all, if it is accepted that educational growth has had an effect on productivity. But, by so doing, he has also simultaneously played a subgame of the prisoner's dilemma type: when each demands more education than a similar individual would have demanded some time before, this has the effect of increasing the price in terms of years of education that all have to pay to get a given social status, that is, the complex life-time flux of rewards associated with a social position. Of course, this point presupposes again that the social structure (distribution of socio-occupational positions), though it may be modified by the overall increase in education, does not follow without inertia the increase in the demand for education: as stated above, without this assumption many data on the period under consideration become incomprehensible. Furthermore, the subgame leaves some players "frustrated," as game theorists sometimes say: the probability of downward mobility for higher-class people has not increased, which is good for them; the probability of upward mobility for lower-class people has not increased either, which is bad for them. Also, though everybody has become richer on the average, the fact that lower-class people have tended to become educationally less deprived relative to higher-class people has apparently not helped decrease the distance between the better off and the worse off as far as economic rewards are concerned.

So, enlightenment is certainly a desirable outcome, but it should not be "reified." Enlightenment is just one subgame of the complicated set of subgames which individuals have to play simultaneously. I know that the world becomes simpler if education is considered a consumption good. In that case, the more the better. But the question is whether education can be seriously considered as essentially a consumption good. The most profound comment I may have seen on this question is an autobiographical statement of Jean-Paul Sartre, whose exact wording I do not remember: he said that he read detective stories for his entertainment and Wittgenstein in order to write his own books. Whence I conclude that for Sartre reading Wittgenstein is perceived as an investment and reading detective stories a consumption. Now, in all universities, philosophy students are supposed to read Wittgenstein rather than detective stories. Moreover, even if we suppose that education is essentially a consumption good, "more is better" only under the condition that if I eat more of the cake education I am not deprived by so doing of some other cake which I would happen to like more.

Given the complication of the educational game in a system where the demand for education is regulated by market-like mechanisms, maybe building a simple, ideal-type model, unrealistic as it is (but is not simplification both necessary to theory and unavoidably unrealistic?), was not a totally unreasonable strategy. The simple model shows that the market organization of the demand for education has resulted in a complicated set of aggregation effects which generate, simultaneously, some outcomes desirable for all, some outcomes desirable for some, some outcomes desirable for none. I still believe that this model has something to do with what happened in the Western societies during the two decades after World War II. When I wrote (1974, p. 201) that I never intended for the model to be used for prediction, this was more than a *clause de style*. The structure of the game is complicated, probably partly perceived by the actors and partly not. Some of the outcomes are probably more easily perceived than others; the lengthening of the schooling period which results from technological progress and also from the prisoner's dilemma subgame cannot go on without limit. Also, some of the goods provided by the increase of education are collective, and the economists have shown that people are not willing to pay for their share of the collective goods. Maybe the educational crisis of the sixties has something to do with these effects of the invisible fist. All these questions invite us to explore new avenues of research, to "bring men in," to investigate the educational decisions made by individuals, to know better the significance of educational private demand.

Hauser (p. 923) has a preference for "standard representations of the mobility process" (read: causal modeling) because "they are rich in formal

properties" because the "sampling distributions of their parameters are known." Again, I think descriptive models of this kind are useful, and my purpose has never been to deprive Hauser of his methodological bread and butter, though a fear on his part in this respect seems to me a plausible explanation of the negative attitude he has adopted in his review. What I have said is only that path coefficients have an "equivocal" meaning since they are derived from linear models whereas the subprocesses underlying mobility are not linear (exponential mechanisms, queuing process, etc.), since they ignore generally the effect of structural constraints, etc. When we learn that in some context the path coefficient corresponding to the effect of educational attainment on achieved status has a value of  $x$ , the only thing we learn is precisely that it has a value of  $x$  (this value depending, of course, on the educational and occupational categorizations). And when we learn, for instance, that this coefficient was  $x$  at some time and close to  $x$  half a century later, this is surprising and requires an explanation involving some other kind of instrumentation. Only the general direction in which this instrumentation lies is known: we must go beyond the statistical relationships to explore the generative mechanisms responsible for them. This direction has a name: theory. And a goal: understanding. That is why I claim the right of deviance from "standard representations."

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#### COMMENT ON BRAGINSKY'S REVIEW OF *SCHIZOPHRENICS IN THE NEW CUSTODIAL COMMUNITY*

We would like to amplify Dr. Braginsky's exiguous account (*American Journal of Sociology* [November 1975]: 678-79) of our findings in *Schizophrenics in the New Custodial Community: Five Years after the Experi-*

ment (Columbus: Ohio State University Press, 1974). It is our modest belief that the implications of our research findings are of sufficient importance to deserve more attention than they are likely to receive by readers of his review.

It should be noted that in 1961 we initiated a study designed to determine (a) whether home care for chronic schizophrenic patients was feasible, (b) whether drug therapy combined with the personal support of a public-health nurse and some social services was effective in preventing their hospitalization, and (c) whether home care was superior to hospitalization. All Central State Hospital (Kentucky) incoming patients who met our criteria (diagnostic, age, residence, nonassaultive, nonsuicidal, and family status) were eligible for inclusion in the study. After a careful evaluation involving psychological tests, a psychiatric interview, and the taking of an extensive social history, each qualifying patient was randomly assigned by the project director to one of three groups: home care on drugs (40%), home care on placebo (30%), hospital control (30%). The hospital admission of the latter patients was simply allowed to stand. The other patients were returned to their families. No one except the project director knew which patients were on drug and which on placebo medication. Naturally, this caused great disquiet on the part of nurse, physician, psychologist, and social worker.

Nine major findings were reported, the two most significant of which were (a) that 77% of the drug home-care but only 34% of the placebo home-care patients were able to remain at home during the course of the study, and (b) that even after initial hospitalization averaging 83 days, more than half of the hospital controls who were released were rehospitalized during the study. Additionally, even those who remained at home as drug and placebo successes were mostly functioning marginally in their everyday routine activities. (See *Schizophrenics in the Community: An Experimental Study in the Prevention of Hospitalization* by Benjamin Pasamanick, Frank Scarpitti, and Simon Dinitz [New York: Appleton-Century-Crofts, 1967].)

In the restudy, *Schizophrenics in the New Custodial Community: Five Years After the Experiment*, we found no significant differences among the three, originally randomly selected groups in poststudy hospital experience, utilization of outpatient clinic care, psychological status (using the Lorr Scale), social adjustment, problem behavior (measured by a 22-item checklist responded to by family members), vocational activity, house-chore performance, and marital and interpersonal relationships.

The data analyses included analysis of variance (ANOVA) among many other statistical tests. Braginsky could have determined as much, if he was in doubt, simply by asking us before undertaking his review.

Our interpretation of these results was not that home care was inef-

fective. Rather, we argued that five years of ordinary care in the community resulted in all three groups functioning at about the same level.

Let us be very clear about the significance of our present findings. Our 1967 publication showed that home care was feasible for chronic schizophrenic patients provided that medication, home care, and social services were made available. Our restudy indicates convincingly that most chronic schizophrenic patients cannot function adequately in the community unless these minimal requirements are met.

All other reviewers' assessments have been positive. A critique by Raymond Feldman in the *American Journal of Psychiatry* (132 [May 1975]: 573) contains the following: "This book is written with such clarity and honesty that it is a delightful experience to read. . . . It is possible in a review only to convey suggestions of the unusual quality and profound implications of this unique book." Another critique, by Marvin Herz in *Hospital and Community Psychiatry* (26 [February 1975]: 107-8) goes as follows: "This is an important book about the long-term outcome of schizophrenic patients treated primarily in the community. . . . The data in this book should be known to everyone interested in the current status of treatment of schizophrenic patients in the United States." A general review article by Richard C. Erickson on follow-up research states the following (*Psychological Bulletin* 82 [July 1975]: 524): ". . . their report is recommended as a model of chronicling what really happens in this kind of research."

Clearly, Braginsky had other criteria in mind which diverted his attention from the substance of the book he was to review.

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## Review Symposium

*Time on the Cross*. Vol. 1: *The Economics of American Negro Slavery*. Vol. 2: *Evidence and Methods—a Supplement*. By Robert William Fogel and Stanley L. Engerman. Boston: Little, Brown & Co., 1974. Pp. xviii+286; xi+267. Vol. 1: \$8.95; vol. 2: \$12.50.

*Roll, Jordan, Roll: The World the Slaves Made*. By Eugene D. Genovese. New York: Pantheon Books, 1974. Pp. xxii+823. \$17.50.

### SLAVERY, PATERNALISM, AND WHITE HEGEMONY

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The debate over the institution of slavery has been dramatically elevated to center stage with the publication of *Time on the Cross* and, a few months later, *Roll, Jordan, Roll*. Indeed, few scholarly books have generated the degree of attention these works are receiving in both professional and popular journals. Although critical reviews continue to raise questions about their historical value, their relevance for the sociology of slavery and inter-group relations has not, in my judgment, been fully assessed.

Both studies focus on slavery in the late antebellum period, and both raise provocative questions and provide equally provocative answers about the slave experience. However, the most obvious contrast lies in their respective approaches to the historical subject matter: Robert Fogel and Stanley Engerman's *Time on the Cross* represents the new quantitative-mathematical approach to history; Eugene Genovese's *Roll, Jordan, Roll* embodies the traditional, qualitative approach.

Several reviewers who expressed reservations about the quantitative historical approach of Fogel and Engerman could hardly resist drawing invidious comparisons with Genovese's sensitive and comprehensive study. The bold introduction of mathematical history has had the effect of polarizing historians in a way that resembles the divisions created earlier in sociology over the increasing reliance on computer-oriented quantitative techniques. The questions raised are basically the same. Can history develop systematic methods of relation, association, and induction using essentially qualitative data, or can such methods be established more definitively with quantitative data organized by advanced computer techniques? Will the use of quantitative techniques ultimately destroy the very essence of historical data, trivialize the findings, and force historians to ignore factors that are not amenable to quantification, or will it provide a more objective and detailed basis for describing human behavior and thereby eliminate the vague categories of comparison that necessarily accompany the qualitative approach? Far more fundamentally, is history to remain a humanistic discipline whereby the analysis of data depends upon the his-

torian's sensitivity to subjective experience and his interpretative understanding, or is it to move in the "scientific" direction, relying on data that are quantified, controlled, and manipulated by mathematical techniques? Any attempt to answer these questions by relying solely on the content of these volumes would unfairly stack the deck in favor of the qualitative approach to history. For *Roll, Jordan, Roll* is an excellent, almost classic, example of good systematic qualitative history; and *Time on the Cross* is a provocative, but basically flawed, attempt at quantitative history.

But my assessment of these two studies is not based solely on their adequacy as historical documents. There are, as we shall see, equally important issues concerning their relative theoretical and explanatory sophistication. Furthermore, the supposedly more scientific *Time on the Cross* is, strangely enough, more polemical. To put the matter more pointedly, if *Time on the Cross* is the most notable example of quantitative history, it also represents revisionist history in a sensationally contentious form.

Fogel and Engerman's major purpose is to challenge the "traditional" interpretation of American Negro slavery, which "has been under intensive critical review for almost a decade and a half" (1:4) by econometric historians, or "cliometricians" as they prefer to be called. This critical reassessment has been based (1) partly on new advances in "economics, statistics and applied mathematics, together with the availability of high-speed computers" (1:4), which the authors claim have now made it possible to meaningfully apply quantitative methods to historical problems and thereby to analyze a far greater body of archival materials than was humanly possible with conventional nonquantitative techniques; and (2) partly on a body of evidence amassed by the cliometricians that provides more "information on the operation of the slave system than has been available to anyone interested in the subject during the antebellum era or since then" (1:8).

Armed with new data and quantitative techniques, they zero in on and forcibly reject the traditional view of American slavery, namely, that it was an irrational economic system, economically moribund on the eve of the Civil War, detrimental to the economy of the antebellum South, and incompatible with urban industry; that the typical slave fieldhand was inept, lazy, and unproductive; and that the planters treated slaves harshly and inhumanely by callously breaking up families for economic profit, by having insufficient regard for the material conditions of slaves, and by expropriating much of what they produced. Although many of these assumptions have already been discredited by historians, especially those that deal with the economy of slavery, none has been subjected to the kind of vigorous attack put forth by the cliometricians.

Contradicting Genovese's earlier work (1965), which portrayed slavery as an inefficient precapitalistic mode of production, Fogel and Engerman maintain that in the antebellum South slavery generated efficient organization of agriculture and promoted rapid economic growth. It was a highly profitable economic system primarily because of the diligent work of bondsmen who responded to humane treatment by internalizing the master's

Protestant work ethic, working harder and more competently than their white counterparts, and striving to achieve economic and social mobility through the limited avenues open to them. Because the typical slaveholders associated good health and high morale with efficient and productive labor, and because they were shrewd capitalistic businessmen, they provided slaves with material conditions that "compared favorably with those of free industrial workers during the first half of the nineteenth century" (1:5) (including a diet which "actually exceeded modern [1964] recommended daily levels of chief nutrients" [1:115]), actively promoted family stability, were extremely reluctant to break up family units, and even discouraged sexual exploitation of female slaves. In addition to profit maximization, the slaveholder's orientation to the slave family was reinforced by a rather strong Victorian moral code that predominated in the planting class.

In different quarters, and with increasing insistence, Fogel and Engerman have been charged with presenting their findings in a format that makes it difficult for even the most conscientious reader to evaluate their conclusions. "In order to encourage the widest possible discussion of the findings of the cliometricians" (1:12), they relegate their evidence and methodology to a highly technical companion volume, subtitled *Evidence and Methods—a Supplement*, and present in the main text a popularized account of their findings that not only is devoid of footnotes, but also provides neither discussion of sources nor bibliographic citations nor explicit comments on methodological problems. Accordingly, in order to determine the documentation for the substantive conclusions presented in volume 1, the reader must undertake the arduous task of reading pages and pages of detailed equations, integrated into dense prose, in volume 2. Nonetheless, the most penetrating and persuasive critical reviews of this study have focused on the shortcomings of the technical supplement. The arguments flowing from these reviews (not presented here in detail because they are easily accessible) indicate a series of methodological weaknesses that range all the way from biased sampling to questionable inferences from quantitative data (see Gutman 1975; Jordan 1974; Rothman 1974; David and Temin 1974/1975, 1975; Duberman 1974; Lichtman 1974; Haskell 1974; and Blassingame 1974). For example, in the scholarly reviews, critics have observed that Fogel and Engerman infer motivations, attitudes, and behavior, not from systematic observations of data collected on the verbal and nonverbal behavior of individual slaves or individual planters, but from rates of return used to compute the profitability of slavery, its relative productive efficiency, and the comparatively favorable economic condition of blacks; also, that the authors fail to develop independent quantitative measures to support their proposition regarding the comparative productivity of the slaves and free workers in agriculture; moreover, that the data used to estimate the slave's diet are based only on a sample of large plantations with 50 or more slaves, whereas only one-fourth of southern slaves actually lived on such plantations; furthermore, that the only quantitative basis for Fogel and Engerman's assertion that slaves were infrequently whipped (because negative punishment was not as effective as positive

rewards in motivating slaves to work productively and efficiently) is the diary of one plantation owner; finally, that in computing the proportion of broken marriages in slave sales, the authors ignore conflicting evidence contained in governmental records and use a technique based upon extrapolation from a possibly biased sample and upon an arbitrary estimating procedure that very likely understates the proportion of married women sold on the auction block. (Similar methodological problems are associated with the authors' procedure for estimating the extent of plantation miscegenation; see, for example, Lichtman [1974]; Gutman [1975]; and Blassingame [1974].)

However, despite all of the publicity given the elaborate quantitative techniques in *Time on the Cross*, and despite all of the methodological criticisms raised by historians, some of whom champion the qualitative approach, many of Fogel and Engerman's conclusions about slavery are remarkably similar to several observations advanced by Genovese. But there are also fundamental differences, especially in their explanations of slave behavior.

For Genovese, slavery in the southern United States was fundamentally a system of class rule manifested in a unique form of paternalism. The southern slaveholders clearly established their hegemony as a regional ruling class by the end of the 18th century, and, unlike their Caribbean counterparts, overwhelmingly resided on the plantations. Paternalism, encouraged by the physical proximity and continuous interaction between planters and slaves, was greatly reinforced by the cessation of the African slave trade, which impelled masters to concentrate seriously on the reproduction of their labor force. Stressing duties, responsibilities, and reciprocal demands and expectations, southern paternalism not only justified slave labor "as a legitimate return to their masters for protection and direction" (p. 5); it also, via the doctrine of reciprocal obligations, implicitly acknowledged the slaves' humanity. In fact, the slaves seized on this interpretation and translated paternalism into a doctrine that firmly stressed their human rights. If it was the master's duty to care for his involuntary labor force, this duty represented the slaves' rights. The slaves' acceptance of paternalism, therefore, signified acceptance of imposed white control within which they established limits, affirmed their rights, and spared their self-respect. "By developing a sense of moral worth and by asserting rights, the slaves transformed their acquiescence in paternalism into a rejection of slavery itself, although their masters assumed acquiescence in one to demonstrate acquiescence in the other" (p. 658).

However, it was the slaves' religion, a complex mixture of African and Christian beliefs, that provided the medium for elaborating this interpretation of paternalism, a religion that created an organized center of resistance within accommodation, a religion, in short, that reflected white hegemony but also established definite limits to that hegemony. It seldom challenged the slave system frontally. "It rendered unto Caesar that which was Caesar's, but it also narrowed down considerably that which in fact was Caesar's" (p. 659).

The reader who feels that an adequate discussion of slavery calls for a broad historical focus will be disappointed in Genovese's tendency to confine his analysis to the 19th century. Nonetheless, on one of the rare occasions when he ventures into the 17th and 18th centuries, his diagnosis of changing treatment of slaves in South Carolina reveals, by contrast, the limitations of the historical coverage in *Time on the Cross*, whose analysis of slave treatment is restricted entirely to the late antebellum period. Genovese notes that "the living conditions of slaves in South Carolina were considerably better at the end of the eighteenth century than they had been at the beginning" (p. 54), and that this was due in part to the closing of the African slave trade, which forced the slaveholders to plan for the future. South Carolina may not have been a representative example, for there is reason to believe that slaves in the lower South were treated more harshly in the 18th century than those in the upper South. Nonetheless, since difference in treatment between the two regions "became steadily less noteworthy" (p. 54) in the 19th century, any analysis which fails to incorporate slave experiences prior to that period not only presents an incomplete and possibly distorted portrayal of slave treatment, but also is unable to capture the probable connection between changes in treatment and changes in the structural and social relations of planters and slaves.

However, if the focus is restricted to the first half of the 19th century, Genovese, on the one hand, reinforces much of Fogel and Engerman's analysis of slave treatment. He argues, in fact, that the slaveholders began a campaign to improve the lot of slaves even before abolitionist agitation and that the material condition of slaves improved progressively throughout the late antebellum period. In material terms, the slaves were as well off as a substantial proportion of Western European peasants and workers and, "were anyone perverse enough to bother, he might find that the living conditions of a large minority or even a majority of the world's population during the twentieth century might not compare in comfort with those of the slaves of Mississippi a century earlier" (p. 59). The slaveholders learned in the 19th century that excessive driving was detrimental to slave health, leading to capital loss. "They even enlisted racist arguments to advocate a more humane course, explaining that blacks constitutionally could not work as long as whites" (pp. 59-60). The diet of slaves was in many respects similar to that of lower-class rural whites. Finally, "the widely believed notion that masters compelled their slaves to marry at a tender age is not true" (p. 463); and despite the terrible toll that involuntary servitude took, the slave family remained remarkably stable, durable, and strong.

On the other hand, some aspects of Genovese's findings and interpretations are inconsistent with those of Fogel and Engerman. For example, Genovese is unwilling to support the claim that slave families were seldom separated and joins that group of critics who insist that Fogel and Engerman's data on this subject "do not permit precise measurement" (p. 457). Moreover, his research on the slave work ethic suggests that, contrary to internalizing the Protestant work ethic, the slaves "resisted that regularity

and routine which became the *sine qua non* for industrial society and which the planters . . . tried to impose upon them" (p. 309). Furthermore, despite the fact that the 1860 census reports that only 13% of the black population had white ancestry (although some estimates are 20% or more), Genovese feels, contrary to Fogel and Engerman, "that enough violations of black women occurred on the plantation to constitute a scandal and make life hell for a discernible minority of black women and their men" (p. 415).

Yet, however different the two studies may be in their approach to historical data, and however similar some of their basic findings, it is difficult for a social scientist to overlook the difference between them in theoretical sensitivity and sociological sophistication, matters which cannot be taken lightly when explicit assertions are made about human interaction. The complicated assumptions associated with the models of human behavior outlined in *Roll, Jordan, Roll* and *Time on the Cross* cannot be fully evaluated here, but it is possible to illustrate the explanatory power of the models by deriving explanations of the relatively infrequent slave insurrections during the late antebellum period.

Consistent with their model of plantation behavior, Fogel and Engerman criticize the "traditional" explanation for the relatively infrequent slave rebellions in the antebellum South by maintaining that historians, in their exaggeration of the cruelty of slavery, shortsightedly expected slaves to be frequently or continuously forced to the point of insurrection. Perplexed by the relative absence of insurrections, historians resorted to overt or implied depiction of the South as an armed camp presenting overwhelming odds against successful attempts at insurrection. Fogel and Engerman state, however, that available evidence fails to support this proposition: "If physical power was the explanation for the absence of rebellion, one would expect to see government expenditures rise with the density of the slave population. Analysis of census data shows no such correlation. It might be argued that police power was supplied not by the state but by the planters themselves. If this were the case, one would expect to see the number of white males per plantation increase with plantation size. No such correlation exists" (1:242). In view of the racist attitudes among whites "which severely reduced the value of freedom to blacks" (1:243) and in view of the precarious economic condition of free blacks in both the South and the North, the authors conclude that the alternative for the average slave was not freedom but "quasi freedom"—thus implying that most slaves had little to gain by resisting their bondage and hence did not rebel.

One does not have to be a student of collective rebellion to recognize the theoretical limitations of these arguments. Since *Time on the Cross* presents no data on the slaves' actual perception of and feelings about freedom, there is no reason to assume that Fogel and Engerman's remarks on the objective conditions of enslavement vis-à-vis freedom correspond with the way the slaves themselves assessed the situation. Indeed, if theory and research have shown anything in the area of collective behavior, it is that revolts by subjugated groups often accompany or follow improvement in their material conditions and that definitions of what is tolerable change

from situation to situation, depending upon such factors as heightening awareness, increasing sensibilities, and changing expectations. These definitions relate to both material and psychological factors and may not be correctly derived solely from an investigator's analysis of objective conditions of life. It is not sufficient, therefore, to present an ad hoc, common-sense, and probably false, explanation that slaves did not revolt because they actually fared better economically than free blacks and that racist whites would not have allowed them to enjoy full freedom. What is necessary for a definitive explanation of slave rebellion is a theory that would specify both the structural and social psychological conditions that encourage or undermine the tendency for enslaved groups to revolt, a theory with a generality of application and not inductively developed from the limited observations of slaves in one country during a restricted time period. I believe the framework of such a theory can be derived from Genovese's analysis of the various structural and social psychological aspects of slavery, a framework providing a "deductive" explanation of slave insurrections.

The persuasive appeal and theoretical thrust of Genovese's framework is that it forces one to recognize the dynamic interaction of structural and psychological factors that profoundly affected the slaves' propensity to rebel. Unlike slaves in Brazil and the Caribbean, bondsmen in the Old South failed to forge a revolutionary tradition and failed to generate revolts of significant frequency, duration, size, and historical-political magnitude. The structural factors that help account for these differences are general antecedent conditions that could be meaningfully applied in comparative explanations of slave revolts in other societies. Basically, Genovese points out that slave insurrections flourished in areas where plantations were large, where there was a high ratio of slaves to free persons, where the ruling class was divided, where slaves had a chance to acquire military experience, and where the master-slave relationships were more business oriented than paternalistic. None of these factors prevailed in the Old South.

In the Genovese analysis, though never explicitly formulated, the psychological antecedents of slave insurrections flow directly from the structural relations between masters and slaves (which were themselves shaped by other structural factors discussed previously in this essay). As an extreme variant of paternalistic relations in slave societies, southern paternalism, regardless of how cleverly manipulated by slaves to preserve their own interests, effectively prevented the blacks from fully appreciating their individual strength. And "the intersection of paternalism with racism worked a catastrophe for it transformed elements of personal dependency into a sense of collective weakness. . . . It was not that slaves did not act like men. Rather it was that they did not act like political men" (p. 149). Even the slaves' religion, which doggedly established defensive mechanisms to combat dehumanization, proved a poor offensive weapon against class rule under slavery. In the final analysis, it "imparted a political weakness, which dictated, however necessarily and realistically, acceptance of the hegemony of the oppressor" (p. 284). The absence of structural conditions

conducive to revolts meant the presence of structural conditions that undermined the formation of a "black nation," a collective consciousness that necessarily precedes political acts of rebellion.

If there is a legacy of slavery, it is the legacy of paternalism. As long as paternalistic relations between masters and slaves existed, they held in check the racial antagonisms of the white lower class. When paternalism ultimately collapsed in the postbellum period, it paved the way for a century of Jim Crow segregation and revealed the unfortunate costs blacks had incurred for preserving their manhood through the manipulation of paternalism at the expense of collective political assertion.

*Roll, Jordan, Roll* is certainly one of the most important books ever published on American slavery. Genovese masterfully integrates historical, comparative, and theoretical materials. A review essay can treat only a partial sample of the material contained in this massive study. Every aspect of slavery is analyzed, including such infrequently researched topics as the mammy, the overseer, the driver, the house servant, the plantation preacher, the Big House, and the slave quarters. There are bound to be weaknesses in a study of this magnitude. Genovese's devotion to elegance and style is exercised sometimes at the expense of brevity, clarity, and precision. The concept of paternalism is crystal clear when related to the plantation but somewhat vague when equated with the general class of nonbourgeois relations. And, since he chose to confine his observation almost entirely to the late antebellum period, the reader is denied the opportunity to see how slavery unfolded over time, to see the extent to which slavery as it existed in the 19th century reflected slavery in previous centuries. It would not be unreasonable to say also that Genovese, a self-proclaimed Marxist historian, has presented a cross-sectional analysis of slavery that is quite consistent with modern functionalist perspectives on social systems. This is not meant to be a criticism, but simply to suggest that, except for an occasional reference to Western Marxists such as Antonio Gramsci and Eric J. Hobsbawm and a sprinkling of sentences such as "Southern paternalism developed as a way of mediating irreconcilable class and racial conflicts" (p. 6), readers will find little that relates explicitly to Marxist theory.

I have already pointed out the striking scholarly contrast between *Roll, Jordan, Roll* and *Time on the Cross*. By way of conclusion I should like also to discuss the polemical tone of *Time on the Cross*, which sets it further apart from *Roll, Jordan, Roll*.

As if to soften anticipated criticisms of their admittedly controversial conclusions about slavery, Fogel and Engerman state that they "have attacked the traditional interpretation of the economics of slavery, not in order to resurrect a defunct system, but in order to correct the perversion of the history of blacks—in order to strike down the view that black Americans were without culture, without achievement and without development for their first two hundred and fifty years on American soil" (1: 258). They further maintain that racism is the major reason for the lingering myth of black incompetence and that historians such as Phillips, whose writings imply the biological inferiority of blacks, and more recent scholars



such as Stampp and Elkins, who provide sociological explanations for the assumed inferiority of black labor, have either directly or indirectly reinforced that myth. In short, *Time on the Cross* reveals "the record of black achievement under adversity" (1:264).

However, in the final analysis, Fogel and Engerman's conception of black achievement represents little more than the slaves' internalizing the Protestant work ethic, adopting the values of a capitalist-oriented economy, and striving to achieve social mobility as efficient laborers within the plantation system. If this is the "record of black achievement under adversity," it is a dubious distinction. For it suggests that slaves obediently and respectfully assumed the role of servants in a system that affirmed and justified their permanent captivity.

There is an ironic parallel between the polemics of *Time on the Cross* and those of the "new black sociology," whose revisionist character is also designed to "liberate" history and the social sciences from the influence of racism. Both end up stressing black achievement at the expense of devoting sufficient and warranted attention to the consequences and ramifications of oppression, including the psychological damage emanating from chronic subordination. For, as W. E. B. DuBois remarked in 1935, after revealing that the physical conditions of slaves were somewhat comparable to those of free laborers around the world, "There was in 1863 a real meaning to slavery different from that we may apply to the laborer today. It was in part psychological, the enforced personal feeling of inferiority, the calling of another Master, the standing with hat in hand. It was the helplessness. It was the defenselessness of family life. It was the submergence below the arbitrary will of any sort of individual. It was without doubt worse in these vital respects than that which exists today in Europe or America" (p. 9; quoted in *Roll, Jordan, Roll*, p. 69).

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## AMERICAN SLAVERY AND THE CAPITALIST WORLD-ECONOMY

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The publication in 1974 of two major works on slavery in the United States has been an academic event. All the more so in that it has not escaped notice that the two interpretations—one steeped in the tradition of neoclassical economics, the other in Marxian historiography—are, at one and the same time, competing, overlapping, and complementary. The two books speak to, and are designed to speak to, the central sociopolitical tension of American national life: the systematic oppression of Blacks, its causes and consequences, economic, political, and social. It is therefore quite understandable that both books have been widely reviewed and have given rise to debate and controversy and many symposia. Indeed, an analysis of the reviews and commentaries would in itself give considerable insight into the contemporary American scene.

The authors of both books claim that, by reviewing historical reality, they are seeking to rescue the Black from unwarranted opprobrium. They do so, however, in strikingly different ways. Reduced to an extremely crude and oversimplified summary, Fogel and Engerman defend the Black against the charge of laziness and incompetence by arguing that the 19th-century slave in fact worked hard and efficiently, while Genovese defends him against the charge of having been weak or cowardly by arguing that the stance of "accommodationism" and "joy in life" was a subtle and efficacious form of resistance and probably the optimal political tactic for the period.

There are at least three different levels at which one could fruitfully criticize these works. One could discuss the social objectives of the authors and the ways in which their writing affects the contemporary political scene. Or one could discuss the concepts they have used to organize their material and how those are tied together theoretically. Or one could discuss the nature and quality of the evidence the authors put forward and the kinds of inferences they draw from it.

Both the first and the third kind of critical analysis have been widespread. With regard to their social objectives and consequences, both books have been heatedly attacked and passionately defended, especially *Time on the Cross*. Specifically, it has been argued (and denied) that the various "rectifications" they attempt about the day-to-day reality of slave life end up as apologia, however unintended, for slave owners and slavery as a system. It is no accident, therefore, that many Black scholars have been quite angry about the books.

As for the process of evidence and inference, the two works illustrate quite different styles. Fogel and Engerman explicitly present their book as an example of the usefulness of cliometrics, "a set of tools which are of considerable help in analyzing an important but limited set of problems"

(p. 9).<sup>1</sup> Genovese says, on the other hand, that "the subject of this book is the quality of life which largely defies measurement" (p. 676). The contrast between a "scientistic" and a "humanistic" style (which is not at all a distinction, if there be one, between scientific and humane knowledge) is an old one, and despite the brouhaha of the publishers of *Time on the Cross*, no new twists are evident in the work of Fogel and Engerman. I leave quite to the side, furthermore, the criticisms of other cliometricians who argue that *Time on the Cross* is bad cliometrics, as I do the criticisms of Genovese by other Marxists who contend that *Roll, Jordan, Roll* is bad Marxism.

I leave them on the side. I do not necessarily agree or disagree. But I do not wish to be diverted from what I think would be useful: to discuss the theoretical adequacy of the interpretations. I do this out of a sense of intellectual priorities at this particular moment in history. I have a feeling that the methodological issues have been for the moment fully explored, at least as far as our governing theoretical frameworks permit. I believe too that the debate on the social functions of ideas, science as ideology, was clearly laid on the table in the 1960s, and that to pursue the matter further at this time is to escape into pastimes rather than to attack central issues. In my view, the key intellectual bottleneck at the moment is theoretical. We are facing the need to rethink and restate the conceptual frameworks we have inherited from the 19th century in order to understand and contribute meaningfully to the long world-systemic transition to socialism which has begun and in which we are living. Both books illustrate very well the nature of these intellectual difficulties.

First of all, neither book focuses on the problems of conceptualization. Fogel and Engerman are very aware of the social function of knowledge and indeed devote a good deal of space to the evolution of what they call "the traditional interpretation of the slave economy" (passim, but esp. 2:168-247). They attack the racist assumptions they assert underlay this so-called traditional interpretation, which they trace back to the "racist myopia" of "antebellum critics of slavery" (p. 215). The racism of the abolitionists in turn is explained by "their greater physical separation from blacks," which made them more "gullible [about] racial stereotypes" than slaveholders, and their upper-class "conceptions regarding the behavior of all laboring folk," a failing "they shared with slaveholders" (p. 136).

In addition, Fogel and Engerman constantly present their empirical evidence as surprising. Indeed their prologue offers us (pp. 4-6) 10 "principal corrections of the traditional characterization of the slave economy." They promptly link these "surprising" findings to the first concern by raising the question of "how those who fashioned the traditional interpretation of the slave system could have been so wrong" (p. 6) and suggesting that the findings will "not only expose many myths that have served to corrode and poison relations between the races, but also help

<sup>1</sup> Unless otherwise indicated, all page references for *Time on the Cross* are to vol. 1.

to put into a new perspective some of the most urgent issues of our day" (pp. 8-9).

The one thing Fogel and Engerman do *not* do is to reconceptualize. It is not that they are unconcerned with theory. Rather, they assume that the theory with which they work is so adequate that all that is necessary is to do the cliometric research and then use the theory to interpret the findings. Thus volume 1 gives us the findings in narrative form and the interpretation and volume 2 the evidence and methods for the nitpickers. The epilogue to volume 1 discusses the "implications for our times." But so much is the theorizing based on unexamined assumptions that when the authors wish to outline their views on such an inherently controversial concept as "exploitation," they refer us to *Webster's Third New International Dictionary*.

This problem is less severe with Genovese. He is not smug about the theory. Indeed, he recognizes that he is on difficult, indeed precarious, ground in his argument that the "Old South, black and white, created a historically unique kind of paternalist society" (p. 4). In fact, much of his book can be taken as a plea for a reassessment of such central theoretical issues as the relationship of slaveholding to a capitalist world market, the constraints that religious and ethical world views place on the social relations of production, and the complex meanings of social interaction: "Gratitude implies equality" (p. 146). Indeed, in an earlier article Genovese was so concerned about "vulgar Marxism" which "offers us the dead bones of a soulless mechanism" (Genovese 1968) that he spent the whole article denouncing a materialist analysis of slavery (that of Marvin Harris) and showing the usefulness, once properly reinterpreted, of two idealist accounts (those of Gilberto Freyre and Frank Tannenbaum).

So Genovese cannot be faulted for unawareness of theoretical issues. But he gets swept away in the majesty and wonderful complexity of his story. His subtitle is *The World the Slaves Made*, and he really offers us this, with rich texture and impressive scholarship. Indeed, the book is virtually an encyclopedic ethnography and, like all really good ones, offers immense pickings for future scholars. They will take, I am sure, many short descriptions and perceptive insights and expand them into a panoply of derived work.

Genovese offers us balance and perspective. But the reconceptualization he knows is needed peters out in a meandering appendix on "the fate of paternalism in modern bourgeois society: the case of Japan." No doubt his way of presenting data as raising doubts about theory is far more fruitful than Fogel and Engerman's crisp assumption that they are validating theory, but it is not enough. Nor is it enough to engage in frequent and relevant comparisons to illuminate the data. For one has a feeling that the comparisons in both books are often between a concrete reality the authors know well and other situations about which myths or partial truths are perceived as empirical descriptions.

Since the bulk of each book is concerned with the actual findings, it is only fair to review them. And here we come up with a discovery that per-

haps not everyone would expect (I did not): The pictures drawn in both books are very similar. The composite picture may be divided into four main parts: the issue of the efficiency of slave labor; the description of the extent of material and spiritual oppression of the slaves; the nature of family life; and the relationship of the most educated and privileged stratum of slaves to the others.

Efficiency is Fogel and Engerman's big thing. "[The typical slave field hand] was harder-working and more efficient than his white counterpart" (p. 5). We even get statistical arguments that are quite precise. Slave farms are "28 percent more efficient" than southern free farms and "40 percent more efficient" than northern farms (p. 192).

As a result of this high efficiency, slavery was profitable. "The discovery of a high and persistent rate of profit on slaves constitutes a serious, and probably irreparable, blow to the thesis that the price of slaves was largely attributable to conspicuous consumption" (p. 70). The discovery of the profitability of slavery "throws into doubt" arguments that slaveholders were "precapitalist" or "uncommercial" or that they "subordinated profits to consideration of power, life-style, and 'patriarchal commitments'" (p. 71).

Furthermore, since slaves could be rented as well as bought and since "hiring was not a minor or inconsequential feature of slavery" (p. 56), it followed that slavery was *not* "incompatible with the shifting labor requirements of capitalist society" (p. 57).

And why was slavery so efficient? The answer is simple. The plantation was the original factory. The "crux," the "key" feature was the fact that slaves were organized into "highly disciplined, interdependent teams capable of maintaining a steady and intense rhythm of work" (p. 204). The authors go further. "[O]rdinary slaves could be diligent workers, imbued like their masters with a Protestant ethic . . ." (p. 231). Thus it was that size of unit could be made to pay off. "Economies of scale were achieved only with slave labor" (p. 194). Nor could such economies be achieved without the use of force. Fogel and Engerman find a superbly sterilized way of stating that point: "The special advantage of slavery for agricultural production, then, is that it was a very cheap way of 'compensating' slaves for the nonpecuniary disadvantages of gang labor" (p. 238).

We are told by Fogel and Engerman that Genovese disagrees, that Genovese thinks "slavery was economically inefficient," although he is "ambivalent on the issue of profitability" (p. 64). But to what extent does he disagree in fact? He does not disagree that slave owners responded to the market, reducing "the size of their slave force for reasons of efficiency" (p. 401) and for the same reasons at other points engaging in hiring. He specifically criticizes Du Bois for overestimating the "Puritan work ethic" (p. 311) of white European immigrants and agrees that slaves were willing "to work extraordinarily hard," although he does add that they resisted "the discipline of regularity" and "responded to moral as well as economic incentives" (p. 313).

Nor does Genovese disagree that slave plantations could resemble "fac-

tories in the field." But he thinks that such a description, while correct for the great slave sugar plantations of the Caribbean, did not quite apply to the tobacco and cotton plantations of the U.S. South. The system there was "a halfway house between peasant and factory cultures" (p. 286). A difference to be sure, but not as sharp a contrast as Fogel and Engerman seem to think, and even Genovese to imply. Genovese does not think that slaves acted exclusively or totally as routinized workers, but does he in fact think that industrial factory workers do so? And do Fogel and Engerman? The actual pattern of work under modern capitalism does not fit Henry Ford's dream, despite all the efforts of Taylorism, as a plenitude of monographs have instructed us.

If there remain some differences between Fogel and Engerman and Genovese on efficiency, there are far fewer when it comes to discussing the quality of life. Life was difficult, both books argue, but it was no concentration camp (as Elkins and others had implied). As usual, Fogel and Engerman are more blunt: "The material (and psychological) conditions of the lives of slaves compared favorably with those of free industrial workers" (p. 5). Less, they hasten to add, because conditions of slaves were so good than because those of free workers were so poor. And we do have to remember the previously cited "nonpecuniary disadvantages of gang labor." Conditions were not so good that anyone volunteered for the job. Genovese comes at the same theme from another entry point. He repeats on a number of occasions that in the 19th century, particularly between 1831 and 1861, the "condition of the slaves . . . got better with respect to material conditions of life" (p. 51). Earlier the slaves *had* "often suffered terribly," but in "the nineteenth century the demands for improvement sounded on all sides" (p. 550).

Fogel and Engerman say that "the average daily diet of slaves was quite substantial" (p. 113). Genovese seems to go along, as usual less fulsomely and more precisely: "From the eighteenth century onward many slaves had a better diet than rural whites because they made an effort to raise vegetables" (p. 535). For Fogel and Engerman, "the houses of slaves compared well with the housing of free workers in the antebellum era" (p. 116). Genovese makes the same comparison more negatively: "The laboring poor of France, England, and even the urban Northeast of the United States, not to mention Sicily or Russia, lived in crowded hovels little better and often worse than the slave quarters" (p. 526).

Whippings were a part of the system, but scarcely as pervasive as generally thought, say Fogel and Engerman. "Although some masters were brutal, even sadistic, most were not" (p. 146). Actually, as they see it, such whippings as occurred were a largely economic device. Free workers who shirked "could be fired—left to starve beyond the eyesight or expense of the employer. . . . Planters preferred whipping to incarceration because the lash did not generally lead to an extended loss of the slave's labor time" (p. 147). Once again, Genovese's emphasis is on improvement over time. Whipping in the 19th century replaced "branding, ear cropping, and assorted mutilations" of earlier eras (p. 67). Overseers were held in check.

"No sensible shareholder wanted a man who could not maintain a certain level of morale among the slaves" (p. 15).

Fogel and Engerman congratulate themselves on rectifying the overstatements of the so-called traditional interpretation of slavery because "it has diverted attention from the attack on the material conditions of black life that took place during the decades following the end of the Civil War. By exaggerating the severity of slavery, all that has come after it has been made to appear an improvement over previous conditions" (p. 260). No doubt a sound point. And Genovese goes one step further. In many ways, the situation was *worse* after the war. He says of family life for example: "The postbellum record should not be projected backward" (p. 501).

The family is another arena of convergence. The destruction of the Black family under slavery is a "myth" (p. 5), say Fogel and Engerman. Indeed, the family was "of central importance" to the organization of the plantation, as the "administrative unit for . . . distribution," as an "important instrument for maintaining labor discipline," and as "the main instrument for promoting the increase of the slave population" (p. 127). And the husband played "the dominant role in slave society" (p. 141). Both planters and slaves agreed that it was important to "encourage the development of stable nuclear families" (p. 142). Both Black promiscuity and widespread sexual exploitation of slave women are contentions for which there is "virtually no evidence" (p. 135).

For Genovese, what did occur was a "scandal," but the "plantations hardly emerge from the statistics looking like the harems of abolitionist fantasy" (p. 415). As for marital infidelity, the evidence cannot sustain a charge that it was widespread. This charge "may in fact largely reflect perceptions of postbellum conditions of social disorganization" (p. 467). As for family life itself, Genovese says that the data demand "a reassessment of slave family life as having had much greater power than generally believed" (p. 451). The slaves created "as much of a nuclear family norm as conditions permitted" (p. 452). The dignity of many men ("How many? No one will ever know") survived the indignities and "came out of this test of fire whole, if necessarily scarred, to demonstrate that the slaves had powerful inner resources" (p. 491). As for the children, "their early and formative years had offered a semblance of childhood, at least relative to the children of other laboring classes" (p. 505).

On the issue of the more privileged stratum of slaves, the approaches are quite different—for Fogel and Engerman, they represent the evidence of mobility, while for Genovese they pose the dilemmas of political opposition—but are the data so different? Fogel and Engerman say that slaves were "fairly well-represented in most of the skilled crafts" (p. 38) and "to a surprising extent . . . held the top managerial posts" (p. 39). Slave society "produced a complex social hierarchy which was closely related to the occupational pyramid" (p. 40). Genovese says: "The wealthier plantations resembled industrial villages, and substantial numbers of slaves ac-

quired a high level of skill in a wide variety of trades" (p. 388). He does think, however, that this phenomenon was on the *decline* in the 19th century and attributes this to "the South's growing dependence on northern manufactures and . . . increasingly exclusionist policies enforced by hostile white labor in the towns" (p. 389).

For Fogel and Engerman the politics of this middle stratum of southern society are simple. Despite higher pay and "room for upward mobility . . . the scope of opportunity should not be exaggerated" (p. 152). Therefore it was "on the talented, the upper crust of slave society, that deprivation of the peculiar institution hung most heavy" (p. 153). Thus they were the first to flee. They were seeking individual salvation. Genovese has a more complicated and generous vision. They were "men between," striving "to mediate between the Big House and the quarters, to lower the level of violence, to maintain order in the most humane way available." The driver, for example, mediated by "instilling a more modern work discipline in his people" and at the same time offering "some protection . . . against factorylike regimentation" (pp. 378-79). They both "assumed the role of accommodationist and became the master's man" (p. 386) and "provided the firmest social basis for a radical political leadership, as their repeated appearance in insurrectionary plots shows" (p. 394). Thus, for Genovese: "The house servants did not so much stand between two cultures as they remained suspended between two politics" (p. 365).

Although the facts both books present seem to converge, time and again, the authors' stances are strikingly different. Fogel and Engerman come to the data as calculating economic men. They try to figure out primarily why the firm acts as it does in terms of maximizing profit, and only secondarily how the worker operates within these parameters to extract advantage. Genovese is interested in the "frog perspective," as Richard Wright called it. For him, the primary question is how the slave coped with an oppressive system, and only second how the slaveowner responded to the *political* action of the slaves.

This contrast explains why at least three major themes run through Genovese's book which are nowhere to be seen in that of Fogel and Engerman. One is the African legacy to Afro-America—"a life-affirming faith that stressed shame and minimized guilt" (p. 247). The second is "the development of the black nation within the American nation" (p. 449). The third, and by far the most important, is Black Christianity. I will not review here the panorama which provides indeed both the framework and the leitmotif of the book. I have no doubt that it represents Genovese's most substantial contribution and the one for which the book will be remembered.

In his view, in the "dialectic of accommodation and resistance" (p. 658), the "protonational consciousness" of the Blacks was "expressed primarily through a religious sensibility" (p. 659). The book indeed closes on what I believe to be its true thesis: "Black religion, understood as a critical world-view in the process of becoming—as something unfinished, often



inconsistent, and in some respects even incoherent—emerged as the slaves' most formidable weapon for resisting slavery's moral and psychological aggression" (p. 659).

I have spent so much space outlining the presentations of data, because, as I have already said, they represent the thrust of both books, the residue left from reading them and studying them. Let me now turn to the theoretical issues that I believe are illuminated by both books but not dealt with in an ultimately satisfactory way.

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The theoretical debate underlying these books concerns the nature of capitalism as a social system. Fogel and Engerman are adherents of the basic Smithian view that the search for profit via market exchange is a "natural propensity" of humankind, operating as a kind of continuing psychological priority in all sane persons. The state sometimes interferes with this propensity, but the propensity is there nonetheless, unrestrained and unrestrainable—somewhat like the Freudian id which the ego contains but never subdues.

From this perspective, the "traditional interpretation" of slavery was very disturbing. Of the five propositions Fogel and Engerman say define that interpretation (2:169 ff.), the first four seem to indicate that slavery in the United States South represented economically irrational behavior: it was an unprofitable investment; it was economically moribund; slave labor was inefficient; slavery retarded growth.

Given their starting point, Fogel and Engerman were genuinely puzzled. Were these propositions true, why did slavery last so long, and why did it take a civil war to bring it to an end? The answer they come up with is at once simple and comforting. The propositions are not true. Ergo, there is no anomaly. This approach explains also why they do not pose other questions, such as how then to explain the outbreak of the Civil War, why slavery persisted longer in the southern states than in the Caribbean, why there was opposition to extending slave structures to the newly opening western areas of the United States. These questions are irrelevant to their purpose, which was to clear up an apparent anomaly in the data that would have called into question the basic theory.

The view of Fogel and Engerman is highly economic. The state is a mere superstructure, and analysts should not dwell on its role excessively. Thus while the legal institutions of the antebellum South may have been "pre-bourgeois," the economic structures were not. "While the South developed a highly capitalistic form of agriculture, and while its economic behavior was as strongly ruled by profit maximization as that of the North, the relationship between its ruling and its servile class was marked by patriarchal features which were strongly reminiscent of medieval life" (p. 129).

But this is only a minor variation on the theme of profit and property. People are a form of capital, *human* capital. "Viewed in this light, the crucial difference between slave and free society rests not *on the existence*

of property rights in man, in human capital, but on who may hold title to such property rights" (p. 233).

Genovese, starting from a Marxist perspective, faces entirely different dilemmas. For Marx, capitalism was a historically specific mode of production. It was not embedded in human nature, for human nature was an abstraction with no empirical reality. Furthermore, it was a mode of production that came in a world-historical sequence. Over historical time, with the evolution of the material base of human societies (in terms of accumulated past labor embodied in machines and technology), new relations of production were established, which throughout all of known history involved the division of men into classes. Within each mode of production, groups tended to polarize into two basic classes, one of which (the smaller) exploited the other, thus generating a struggle between them, which eventually ended in the transformation of the system into another successive form.

Given this paradigm, the problem (as Engels made explicit in various famous letters) was not to explain the broad strokes of history—that could be easily and intelligently done, as demonstrated by the *Communist Manifesto*—but to explain the complicated detail: the long and murky "transitions" from one mode of production to another, the workings of "uneven development," the superstructural "lags." There have been three responses historically by Marxists to these complications: to ignore them, as do the "vulgar Marxists," who turn out to be Smithians at heart; to be overwhelmed by them, as are the "ex-Marxists," who usually become Weberians; to take them as both the key intellectual *and* the key political problem of Marxists. One type of current jargon calls this last response "Gramscian," although to be sure Gramsci was by no means the only prominent Marxist thinker to make it. Both Lenin and Mao, to cite only the two most obvious, are clearly in this group. In any case, Genovese clearly states his indebtedness to Gramsci, in particular to his theories of hegemony and of political consciousness.

The problem for Genovese, given his strong evolutionary thrust, is the question of seeming "regression." "At first glance, the legal history of Western Europe represents an anomaly. The law arose in early modern times on rational rather than traditional, patrimonial, or charismatic foundations, however many elements of these remained" (p. 44). It was *into* this system, in which private property was already ensconced, that slavery was introduced. Thus it is quite different from slavery in the ancient world, as Genovese makes clear. Genovese says that Fogel and Engerman concentrate on the economic costs of "the reintroduction of precapitalist elements into the legal system" but that he, Genovese, is concerned rather with "the effect of the imposed duality created by the reintroduction as well as the continuation of precapitalist ideas of power and property into an inherited system of bourgeois-shaped rational jurisprudence" (p. 46). Thus paternalism in a capitalist world was a reality, not an ideological fiction, although, adds Genovese, as with all human institutions its contradictions undid it.

This happened because paternalism was a two-edged sword, and the slaves "acted consciously and unconsciously to transform paternalism into a doctrine of protection of their own rights—a doctrine that represented the negation of the idea of slavery itself" (p. 49).

Thus we have an irony. Fogel and Engerman, avowed liberals, write what is to my mind one of the strongest politicomoral justifications for the theories of Karl Marx since Engels's *Condition of the Working Classes in England*. They argue with remarkable clarity that capitalists use the state machinery, both the carrot and the stick, to maximize their individual profits, and that the less individual freedom accorded the worker, the more profitable the system. On the other hand, Genovese, an avowed Marxist, might be said to make a case for conservative (idealist) theories about the ways in which political and cultural institutions tend to explain the largest part of historical reality. "Only those who romanticize—and therefore do not respect—the laboring classes would fail to understand their deep commitment to 'law and order'; life is difficult enough without added uncertainty and 'confusion.' Even an oppressive and unjust order is better than none" (p. 115). Ideologies, repeats Genovese over and over, are not merely "self-serving and radically false." They can also be an "authentic world-view . . . developed in accordance with the reality of social relations," and hence embraced "without hypocrisy" (p. 86). We cannot wish away paternalism with a sneer of contempt. It constituted in fact the "rich experience" of the daily lives of slaves, who truly "expressed admiration for the aristocratic features of southern life" (p. 115), while turning such admiration into "a weapon of defense" (p. 116).

It is a striking feature of both works that they are so geographically self-contained. Not that this is so unusual in modern social science. Nor do the authors fail to make "comparisons." Indeed, both books abound in comparisons with other "entities." (I call them "entities" because sometimes what are compared are firms, sometimes states, sometimes other groupings, and frequently a melange.) But at crucial points in the analysis, the fact that the American South was an arena in a *world-economy* is left out of consideration and hence the analysis is falsified.

Consider for example *Time on the Cross*. Chapter 1 is dutifully called "The International Context of U.S. Slavery." We are told of the origins of the Atlantic slave trade and the course of emancipation throughout the world. But it is only at the very end of the book that we learn, literally parenthetically, that "most U.S. cotton was consumed not in the United States but abroad" (pp. 245–46). There is no hint, even in passing, that British interests may have played a historic role in the perpetuation of slavery in the U.S. South. Instead, we are told: "Although it may be surprising to some readers, the main gainers from the gang system were not slaveholders but consumers of cotton" (p. 244). How carefully phrased. We are led to envisage the greedy customer buying a shirt cheap somewhere out there and thereby exploiting the Alabama fieldhand, when Fogel and Engerman know quite well that the bulk of the gain was creamed off by the primary "consumers," the textile *manufacturers*.

Or take a theme which runs through both Fogel and Engerman and Genovese but which neither book explains very well: the relative material improvement of the conditions of the slaves in the decades immediately preceding the Civil War. This coincided, as Genovese notes (but Fogel and Engerman characteristically do not), with an increase in legal repression: more welfare, more "rights," but also fewer manumissions. Genovese says of this apparent "paradox" that the same men supported both trends and that their position "made perfect sense" since it combined perpetual slavery with "making it possible for [slaves] to accept their fate" (p. 51). But, if this is so, why was such a perfectly sensible solution only put into effect in 1831? Why not in 1731?

The answer gets us to the heart of the issue. In 1731 there was an international slave trade; in 1831 there was not, or at least there was none that permitted slaves to be brought from distant places to the United States. This change had come about because of both the industrial revolution of 1760-1830 and the definitive establishment of British world hegemony after 1815. Both the need for West Africa as a crop-producing area and the desire (and ability) to deny *European* competitors slave producers led to Britain's enforcement (selective, be it noted) of the abolition of the slave trade and encouragement in areas outside its own supply zones (such as the U.S. South and Brazil) of emancipation.

These facts led quite directly to a number of phenomena mentioned in both books. It seems self-evident that if you cannot import new slaves from elsewhere (the United States from 1808 on) you have to reproduce them yourself and that this fact alone will require improvement of material conditions, including the encouragement of a stronger nuclear family. (It will also lead to tightening up on manumission.) Of course, there are contradictions, since this process makes slave labor more expensive, but then that is exactly what the incorporation of the West African external arena as a peripheral area of the capitalist world-economy did do, and why indeed slavery finally would disappear entirely. It disappeared *not* because it was incompatible with capitalism (here Fogel and Engerman are far nearer the truth than Genovese), but because it was incompatible with a capitalist world-economy that no longer had an external arena to bear the bulk of the cost of slave breeding.

Fogel and Engerman talk of a "high break-even age" for U.S. planters with regard to slaves—hence medical care for slave children and encouragement of fertility of slave women "while slaveowners in other parts of the hemisphere appear to have discouraged it" (p. 155). Precisely. "Of all the slave societies in the New World, that of the Old South alone maintained a slave force that reproduced itself" (Genovese, p. 5). And why? Because in Brazil, for example, at that time they were still importing slaves from Mozambique, and it *cost less* to buy an adult slave than to rear your own (because of loss of mother's work time, costs of child rearing, etc.).

But notice how bizarrely these facts come out in Fogel and Engerman. They tell us that in Jamaica (unlike the United States), life expectation fell below the "break-even age." Let us ignore the fact that for Jamaica

they are talking of the 18th century and for the Old South of the 19th. Let us merely see what comes next: "Consequently, during most of the eighteenth century, masters in colonies such as Jamaica discouraged family formation and high fertility rates, preferring to buy adult slaves in Africa rather than to rear them" (pp. 155-56). Consequently? It was just the opposite. As long as plantation owners could buy slaves in Africa, who cared about the break-even age which was calculated on the basis of owning slaves throughout their life cycle? The "consequently" illustrates in the clearest possible way the fundamentally unhistorical nature of the analysis of Fogel and Engerman. But, of course, if capitalism is a natural propensity, why bother with mere history—inaccurate, confusing, anecdotal?

Or consider the rejection by Fogel and Engerman of the contention of Hinton Rowan Helper, made in 1857, that the South was a "colonial dependency." Not so, say Fogel and Engerman, for were it so, "how are we to characterize the states that occupy the territory running from the western border of Pennsylvania to the western border of Nebraska?" And, presumably even more telling: "The South's large purchases of manufactured goods from the North made it no more of a colonial dependency than did the North's heavy purchases of rails from England." The South (were it a separate nation) would have stood fourth in the world in wealth, exceeded only by Australia (top of the list—did you not know that in 1860 Australia had the highest per capita income in the world?), the North, and Great Britain. "Indeed, a country *as advanced as Italy* [italics mine] did not achieve the southern level of per capita income until the eve of World War II." And finally: "The true colonial dependencies, countries such as India and Mexico, had less than one tenth the per capita income of the South in 1860" (p. 249).

With so much obfuscation, where do we begin? In the uncomplex world that Fogel and Engerman construct there are a few "real colonies," presumably badly off, maybe (dare I suggest it?) even exploited, and *all the rest* of the world is composed of countries that are presumably equal trading partners in a system wherein comparative advantage reigns supreme and per capita income measures how hard you work ("efficiency"). Absent from this world is unequal exchange: peripheralization leading to a *variety* of economic roles for the peripheral areas of the world-economy, which have different modes of labor control (raw material cash-crops based on slave labor for the U.S. South contrasted with food cash-crops based on small freeholds in the U.S. "West"); the concept of a semiperipheral area (such as the U.S. North), at once exploiting and exploited and seeking to break loose and become a core nation by snapping the economic umbilical cord of the South to Great Britain. "As advanced as Italy?" An Italy just achieving the political framework with which to compete in the world-economy, an Italy whose South became a major labor reserve for the United States, Brazil, and the Italian North precisely at the moment when slavery was finally disappearing (was it so coincidental? [see Ianni 1965]).

Genovese, by contrast, never oversimplifies. But occasionally his stress on complexity prevents clarity. His book is an exercise from beginning to end in the subtle intertwinings of a "paternalist" structure. But what of the elementary question: is paternalism *in contradiction with* capitalist social relations? I have the sense that for Genovese the answer is an impatient of course—"in contradiction with," though of course not "incompatible with." Let me suggest another possible approach. The particular form of paternalism which Genovese describes is, as I am sure Genovese would agree, quite different from classical feudal social relations, and is on many continua somewhere between the latter and purely market-oriented contractual relations. Units based on this form of paternalism can be seen as units in transition, ones resisting capitalization, ones whose power to survive needs to be explained. This is essentially Genovese's approach, it seems to me, and one firmly in the Maurice Dobb version of Marxist analysis in which Genovese specifically places himself (p. 688, n. 71).

But suppose the forms Genovese is describing are not transitional or remnants or pockets of resistance but the heart and essence of capitalism as a mode of production, which could be seen as a system that contains within its economic arena *some* firms largely based on contractual wage labor and *some* (even most) firms based on one variant or another of coerced or semicoerced semiwage labor. If we make this simple switch of perspective, which must of course be argued, we see the whole picture in a very different light. The slave owners were then indeed capitalists, as Fogel and Engerman argue, not, however, because all rational men are, but because they were operating in a capitalist world-economy. And a slave owner who did not allow market considerations to loom large in his firm's operation would sooner or later go bankrupt and be replaced by one who did. That southern planters developed a different ideology from that of New England mill owners (and were they as different as Genovese implies?) is simply the reflection of differing interests within a single capitalist world-system. That they tried to use the state (whether within the Union or by creating the Confederacy) to defend their interests, that is the name of the game.

From the slave owner's point of view, the slave received a wage. He received annually the sum of the cost of purchasing him (or raising him if he was born to a slave), maintaining him during his work years and old age, and policing him (the *extra* costs of supervision entailed by the slave status), divided by the years of effective work. By measuring output in comparison with annual wage we can easily compute a productivity ratio. It is complicated but eminently feasible to compare the cost of a slave's output with the cost of the contractual wage laborer who receives regular payments plus perquisites only during his effective work time (at least until the 20th-century welfare-state measures entered into the wage picture) and must spread this money over the life cycle of the nuclear family. Employers were no doubt aware of rough equivalencies, or thought they were. Coercion (as opposed to contract) reduces the total wage, as

Fogel and Engerman clearly demonstrate, but the optimal wage (that is, the one yielding the greatest income to the slave owner per dollar spent on slave maintenance) was one that combined "force and pecuniary income." They add "*more* pecuniary income per capita than they would have earned if they had been free small farmers" (p. 239), but of course they omit the non-money income ("subsistence") of the free small farmers. Thus coerced or semicoerced semiwage labor is, and has been from the beginning of capitalism as a world-system, a phenomenon of peripheral areas of the capitalist world-economy, while contractual labor is concentrated (largely, but not exclusively) in core areas.

The slave owners responded to a market. This does not mean they were not responsive to "other" considerations: the social status of slaveholding, the guarantees of an available work force. But do these not involve also economic benefits? Certainly these "other" considerations evolved into an immense superstructure which was not a hypocrisy, but if the social relations were truly in contradiction with long-term economic rationality, could they have survived in a capitalist world?

The key methodological issue is the unit of analysis. Given their neo-classical orientation, the logical unit for Fogel and Engerman is the firm, and they tend to use it. They do so inconsistently, as do most neoclassical economic historians, because so many questions cannot be treated at the level of the firm. But they handle the difficulty—one is tempted to say, neoclassically—by blurring it. Else they would have to reopen their premises. But for Genovese, the analysis revolves presumably around a mode of production. Yet he never quite uses the term. Why not? Why does he speak of slave *society*, paternalist *society*, slavery as a *system*? In the end these are all euphemisms for "state" or the quasi-state which was the Old South—as though *states* have modes of production. A state no more has a mode of production than does a firm. The concept "mode of production" describes an *economy*, the boundaries of which are precisely a preliminary empirical question of the utmost relevance to an understanding of "class rule." The South, certainly in the years 1831–61, the years with which both books are centrally concerned, was part and parcel of a *world-economy* whose mode of production was *capitalist*, and within which owners of large-scale cash-crop plantations utilized, to the extent they could, such state structures as they could control (or largely control) to make it possible for them to extract the largest share of the surplus value being produced by productive workers.

Slavery was thus very useful to them, particularly the variety of it ("paternalism") which evolved, for all the reasons (most of which seem to me plausible) that Genovese (and even Fogel and Engerman) adumbrate. But it did not fit the interests of competing groups, who evolved ideologies to help them eliminate it.

Slavery of course did not serve the interests of the slaves, in however paternalist a form. We must never forget that no one volunteered to be a slave. Slaves organized as best they could to defend themselves and improve their lot. Here Genovese has done us an immense service in showing

what a legally repressed group of proletarian workers can do, operating within the laws and the ideological constraints imposed by their oppressors. He makes a very telling comparison of the class actions of slaves and contractual wage workers. "[Small group desertions, which were frequently designed only as a temporary action,] paralleled strikes by free workers, for they aimed at winning concessions within the system rather than at challenging the system itself. But, like strikes by free workers, they contained a germ of class consciousness and demonstrated the power of collective action. In both respects they combatted the sense of impotence that the slaveholders worked so hard to instill in their slaves" (p. 656).

This comparison, in my view valid, has great implications which are not developed, ones that go counter to the theme of a unique paternalist slave society. Genovese tells us over and over that concessions to slaves were made, in some part because of benevolence or prudence of the whites (whether slaveholders or abolitionist allies), but in large part because the slaves in some way seized their "rights." It was not easy. Genovese reminds us of Fanon's dictum: "It is always easier to proclaim rejection than to reject" (p. 701, n. 9). But it was done. It does not help, however, for us to fail to be clear that what the slaves accomplished in the antebellum South was the accomplishment of proletarian workers in a peripheral arena of the capitalist world-economy.

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## Book Reviews

*Plantation Societies, Race Relations, and the South: The Regimentation of Populations: Selected Papers of Edgar T. Thompson.* Durham, N.C.: Duke University Press, 1975. Pp. vii+407. \$12.75 (cloth); \$6.75 (paper)

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The most common approach to the study of the American South sees the region as an aberrant corner of the United States, compares it with the rest of the nation, and addresses its peculiarities. Edgar Thompson's approach is different: he views the South as "the northern part of the Gulf Caribbean region" (p. 188), sharing with the rest of that region and with other large-unit, cash-crop agricultural systems certain characteristics not evident elsewhere. One need not believe that "whatever is 'different,' whatever is special about the South appears to go back to the plantation" (p. 86) to recognize that the region was early and lastingly polarized—not to say paralyzed—around the plantation and its related institutions, and that the momentous results can be illuminated by studying the South in the context of similar societies elsewhere. And no one is better qualified to conduct that study than someone who combines wide-ranging erudition with a plantation upbringing and first-hand experience in Hawaii, South Africa, East Germany, and beyond—a combination almost unique, I would imagine, to Thompson.

The 17 papers in this volume are preceded by an introduction which essays a definition of the plantation, comparing it with such other agricultural establishments as the family farm, the hacienda, the manor, and the "military agriculture" which produced opium in pre-Communist Manchuria. The first group of papers includes two comparing the plantation with the mine and with the mission school—institutions both instructively similar to it and instructively different; a third on the climatic theory of the plantation; and a fourth which examines the institution as a setting for the emergence of the South's traditional system of race relations. The six papers in the second section of the book deal with various aspects of race relations, starting with Thompson's deservedly well-known "The Plantation as a Race-making Situation" and including splendid discussions of the connection between linguistic patterns and race relations, of school desegregation (viewed comparatively), and of the problems—and the potential for social theory—of the small mixed-blood groups found in most multiracial societies. The third section consists of seven disparate papers, all having something to do with the South, including one on the planter as a social type, one on religion in the old and the new South, and a first-rate treatment of the relation of sociology to regional studies, which (*mutatis mutandis*) could clarify many similar discussions of black studies, women's studies, or almost any interdisciplinary enterprise.

Since most of these papers either focus on or return eventually to the plantation system, a few observations about the ensemble may be more appropriate than a scattering of comments about each. Thompson treats the plantation under four aspects: "(1) as a way of settling and concentrating a population of mixed origins on a frontier . . . ; (2) as a way of producing an agricultural staple for a metropolitan market . . . ; (3) as a way of disciplining a population for labor under the authority of a planter; and (4) as an institution which develops in time through collective activity a distinctive style of life or culture" (p. 39). The last emphasis is the strongest and, to my mind, the most interesting.

Some 40 years ago (about the time the first of these papers appeared), Carl Carmer wrote of Alabama that "the Congo is not more different from Massachusetts or Kansas." One of Thompson's successes is to show the fundamental sense in which Carmer's observation is not fatuous—not in emphasizing how peculiar Alabama was under the plantation regime, but precisely in showing that neither it nor the Congo is at all peculiar, that certain consequences follow from the attempt to realize certain goals in particular settings.

In addition, the reader is led to understand the institution not just intellectually, as an inevitable development from a set of givens, but, so to speak, from inside. In several papers Thompson manages, by able use of documents, to convey a sense of the sheer *ordinariness* of everyday plantation life for its participants (surely the first requirement of a going social system)—no mean feat at a time when many aspects of that life look bizarre, not only to readers of *Stars Fell on Alabama* but also to many who live on or near what are still called plantations. These essays are not only contributions to sociology and history; many of them are literary achievements.

While Thompson's approach complements that which emphasizes regional differences within the United States (which may too often stress the merely odd), it has its own drawbacks. My one reservation about these papers is that, viewed as a whole, they seem to underemphasize the fact that the South has been bound (inextricably after 1865) to the rest of the United States—which looks not at all like the Congo. There is a tendency here (understandable in a collection of papers although not in a comprehensive work) to treat the South as a more or less closed system, without much attention to the terms of its trade with the national market or to the reaction of the rest of the country to its emergent culture and institutions. But the South has had not just an economy and a resulting culture but, rather, a *colonial* economy and a *minority* culture—fateful qualifications.

For all that the southern plantation is going with the wind, this book is timely. As it reminds us, similar agricultural systems still exist in many parts of the world, and they can be studied profitably in the light of the southern example. Besides, the legacy of the plantation is still with the United States, now transplanted in part to its cities. For scholars, too, the book is well timed. A glance at some bibliographies will reveal that Thomp-

son has contributed to the burst of new work on the plantation by social and economic historians, and it is good to have these papers—some by now fugitive—assembled conveniently for the interested but not highly motivated reader of Eugene Genovese or John Blassingame. Moreover, the kind of comparative institutional analysis Thompson does so well—if not the clear and vigorous prose in which he does it—is emerging from a period of eclipse among sociologists, and this book should find a larger professional audience than it would have had even 10 years ago. It deserves it.

Duke University Press has produced a handsome volume, pleasingly illustrated, and should be commended for bringing out a reasonably priced paperback edition.

*The Concept of Social Change: A Critique of the Functionalist Theory of Social Change.* By Anthony D. Smith. London: Routledge & Kegan Paul, 1973. Pp. ix+198. \$11.50 (cloth); \$6.00 (paper).

Wilbert E. Moore

*University of Denver*

The tardiness of this review of an important book has many reasons, which do not, severally or in combination, constitute an adequate excuse. Apologies are due to author, editor, and potential readers, and they are freely offered. The closest approximation to an excuse that I can find is that the book at hand is not for the quick scanner or other lazy reader. The argument is closely reasoned, at times subtle, and is mainly lacking in the redundancy that gives aid and comfort to those who read casually.

What Anthony D. Smith has undertaken is not another critique of functionalism on grounds of insensitivity to power, conflict, and intrinsic dynamics. Instead, he has attempted to show that the way latter-day functionalists have attempted to cope with that criticism without abandoning their concern for bounded systems has led them to espouse a particular formulary of explanations of change that fits a neo-evolutionary framework.

Beginning with a *wissensoziologische* interpretation of functionalist confrontation with criticism for being silent on the phenomena of change, Smith argues that the theorists had a choice between admitting to incompleteness or finding a change model (or metaphor?) consistent with the systemic properties that had been their claim to rigor and consistency. The theorists proved unwilling to abandon claims to being comprehensive and found in a revived social evolutionism the framework for making change a property of social systems rather than a mere aberration or external accident.

The principal device that the functionalists adopted to perform this transformation, according to Smith, was concentration on differentiation, which in the hands of the functionalists—Smith particularly examines the work of Talcott Parsons and Neil Smelser here—becomes a kind of unexplained prime mover, a sort of evolutionary unfolding of the potential

within the system: an explanatory principle that would seem to deserve the derogatory designation "animistic" as used by William R. Catton, Jr. (*From Animistic to Naturalistic Sociology* [New York: McGraw-Hill, 1966]).

A select company (but still a considerable variety) of sociologists have been given fairly close attention, including, besides Parsons and Smelser, Robert Bellah, Chalmers Johnson, S. N. Eisenstadt, and myself; Smith also makes the requisite references to Émile Durkheim, Bronislaw Malinowski, A. R. Radcliffe-Brown, and other founding fathers. It is probably fair to symbolize the commonalities among this company by calling them functionalists, although their degrees of orthodoxy are variable, and I find myself resistant to categorization. Indeed, there are always dangers in setting up a sort of "ideal type" of theoretical position, finding different quintessential representatives for various component assumptions within the type and then labeling the scholars by the name selected for the type. Thus, Smith's critical target comprises the "functionalists," who, in the course of the argument, also become the "neo-evolutionists" and the "endogenists" (that is, theorists attentive only to sources of change internal to social systems). This procedure leads to some anomalies. For example, the self-affirmed functionalist Kingsley Davis gets into the conversation only by an indirect reference to his and my slightly time-tarnished "functional theory of stratification"; Smith is apparently ignorant of Davis's major works in demography and comparative urbanization. Robert K. Merton is noted only as a within-the-circle critical codifier and for his discussion of dysfunctions, and not, for example, for his revival of the concept of serendipity as a component of change in the dynamics of science (a specialized field of study in which Merton is certainly not an "endogenist"). In my own case, which I know a bit better, my attention to diffusion and to the combination of diffusion and of deliberate, though partially autochthonous, change in the course of "modernization" is simply ignored. Or perhaps I was not meant to be accorded membership with the neo-evolutionists and endogenists, but that (not really gnawing) uncertainty precisely illustrates the problem of such less-than-homogeneous groupings.

Smith concludes with a plea for attention to exogenous sources of change if sociologists truly wish to cope with historical reality—and, I should add, with contemporary reality. As I have previously argued in this *Journal* ("Global Sociology: The World as a Singular System," *AJS* 71 [March 1966]: 475–82), for many sensible analytical purposes, such as the significance of the world pool of technologies and political ideologies, "societies" are so deficient in the criterion of self-subsistence and so permeable at their boundaries that their utility as analytic constructs is impaired (though, in the form of national states, not destroyed). Yet the characterization of functionalists as endogenists reveals a paradox that Smith notes only casually. It was precisely the lack of attention to endogenous change in some standard versions of functionalism that made some of us poor sinners repentant. System-eluding uncertainties and fluctuations and system-induced dysfunctions and conflicts and innovations would not dis-

appear by theoretical fiat and required conscientious attention. Smith gives fair credit for the attempt to cope with these problems. His plea for attention to exogenous sources and interactive patterns of change is also appropriate. Yet the critic gives little more attention than do those criticized to the apparently growing importance of deliberate and politically sponsored change in the contemporary world. Surely we must move beyond either organic or mechanical models of coherence, breakdown, and movement. Metaphors, like stereotypes and cherished ideologies, are only useful when not taken too seriously.

*Pyramids of Sacrifice: Political Ethics and Social Change.* By Peter L. Berger. New York: Basic Books, 1975. Pp. xiv+242. \$10.00.

Don Martindale

*University of Minnesota*

Peter Berger's method in *Pyramids of Sacrifice* is mytho-poetic. He opens with a prelude on the pyramid at Cholula, whose human sacrificial cult he takes as a paradigm of the relations between political theory and human suffering in the contemporary world. He provides novelistic interludes between analytical chapters and closes his book with a postlude. These vignettes are presumably intended to illustrate the human consequences of the ideas contained in the chapters proper.

Moreover, the writing in the expository chapters is more poetic than logical. The argument is largely carried by images. For example, the curious "cargo cults" of Pacific Islanders which undertook to bring into being by magic the types of material goods once brought by American GIs are repeatedly utilized as a symbol of the meaning of capitalism for the Third World. Berger loves arresting images which substitute for clear analysis. "Modernization," he asserts, "operates like a gigantic steel hammer" (p. 22). He urges us to reject "the libidinal intoxication with violence" (p. 72) rather than violence itself. He finds "cognitive imperialism" (p. 117) to be a crucial component of modernization. We are warned that "all men are vultures in that they live off the agonies of the past. At the foundations of every historical society there are vast piles of corpses, victims of the murderous acts that directly or indirectly led to establishment of that society" (p. 138). Contradictory positions are at times taken and then disposed of with an arresting figure of speech. The movement of thought is often analogical, from image to image, rather than logical, from proposition to proposition.

Berger takes the mainspring of social change to be myth. His work is in the tradition of Comte, who argued that "ideas rule the world or throw it into chaos," and Hegel, who saw world history as the actualization of ideas. Crucial to Berger's appraisal of the chaos and human suffering that accompany the westernization of the world is his view that they are produced by the ideological competition between the capitalist and Communist blocs of nations. This competition is fired by two myths of development:

the capitalist myth of growth or progress, which embodies the "Western tradition of messianism" and "represents a secularization of Biblical eschatology" (p. 19), and the Communist myth of revolution, which also has Judeo-Christian roots and conjoins "themes of progress, technocratic control and productivity" (p. 22) to the "quest for redemptive community" (p. 26).

Since the problems of the Third World are seen primarily as products of development myths, Berger proposes to examine them critically. He does so by accepting the Marxist-Leninist critique of capitalism as essentially correct and the critique of Marxism-Leninism by capitalist writers as essentially correct. Berger admits that there are differences in the models of society advocated by these two groups: "There is the primacy of the individual as against that of the collectivity. There is freedom as against belonging. There is acceptance of an 'adversary model' as against the ideal of 'harmony'" (p. 99). However, he finds little to choose between the social consequences of the two systems: "If socialist systems have been erected at the price of great human suffering, so have the capitalist systems. Not only revolutions cause suffering and death; the status quo can also kill, often by hunger rather than bullets" (p. 76).

Berger carries out a special critique of "consciousness raising," a euphemism for indoctrination of all types, on the principle that all empirically available worlds of consciousness are equal. He urges that policy-making should rest on a calculus of pain and, utilizing Brazil and China as examples of capitalistic and Communist modes of change, respectively, concludes that neither can be justified by the cost. He further recommends that policymaking rest on a calculus of meaning, that is, an assessment of whether modernization can be justified in the teeth of its destruction of traditionalistic perspectives. Finally, he traces the fall into disrepute of the American creed. He recommends Max Weber's essays "Science as a Vocation" and "Politics as a Vocation" as presenting the only defensible position of the social scientist who accepts his responsibilities as a citizen.

For all the brilliant writing that occurs periodically along the way, *Pyramids of Sacrifice* leaves one with many questions. If anything, ideologies are more often the shadows and reflections of economic and political power than their substance. The proposals to test policy on the basis of calculi of pain and meaning are moral injunctions: do not inflict unnecessary pain; respect the other's point of view. They are, however ethically worthy, as little likely to influence the course of events as the Christian injunction to "love thy neighbor." Having traced most of the Third World's problems to the myth of development, Berger ends up by urging the revitalization of the American creed, which is its primary embodiment.

In the end, the import of *Pyramids of Sacrifice* is deeply conservative. The Third World nations are urged to limit their relations to the great powers as much as possible, to think twice before imitating either type, and on the basis of a calculus of meaning to show greater respect for their traditional cultures—in short, as far as possible to erect a wall against

the tides of change. America is urged, as far as is feasible, to mind its own business and tend its own garden.

*Sociology as Social Criticism*. By T. B. Bottomore. New York: Pantheon Books, 1974. Pp. 217. \$10.00.

Jeffrey C. Alexander

*University of California, Berkeley*

T. B. Bottomore has emerged as one of the major representatives and interpreters of Marxist sociology in the English-speaking world, and *Sociology as Social Criticism*, a collection of 14 recent articles and review essays, indicates the reasons for his position. The book is formally divided into three sections: "Social Theories," in which Bottomore comments generally on the work of central sociological figures; "Classes and Elites," in which he continues his specific empirical studies in political sociology; and "Social Movements and Political Action," in which he pursues more topical political issues. A second, more informal division should be made between articles in which Bottomore remains within the radical topography (roughly, sections 2 and 3) and those in which he steps outside it: only thus, I believe, can we understand the different qualities of his thought.

What most distinguishes Bottomore's work within the radical tradition, and in parts 2 and 3 of this book, is his ability to maintain what usually proves to be a difficult tension. He explicitly places a certain moral and political commitment at the center of an empirically oriented theory; yet he also strives constantly, and successfully, to separate ideological from theoretical and empirical issues. As a result, he is both a "Marxist" and one of Marx's harshest critics.

Bottomore's radical ideological-political commitment manifests itself in the question to which he devotes himself: Is the manual working class in fact the vehicle to bring about a revolutionary transformation? His commitment to the autonomy of the theoretical dimension manifests itself in the fundamental critique of economic determinism that he makes in pursuing the answer. Although many of the noneconomic factors he discusses have been noted by observers in other paradigms, his thought is impressive precisely because, while acknowledging such factors, he will not abandon his commitment to the basic Marxist paradigm. It is, paradoxically, in pushing this paradigm to its limits that Bottomore's work becomes theoretically significant.

The essays in sections 2 and 3 can be seen as a frontal empirical assault on the two most central issues in Marxist logic: the positions of politics and of culture in relation to the economic base. Bottomore's discussion ranges from the most specific factors to the most general. He presents a number of specific reasons for his belief that the traditional proletariat has not generally evidenced revolutionary potential. From a broader perspective, he raises and tentatively confirms the possibility that Marx's

very identification of the proletariat as the revolutionary class was based on a faulty analogy to the revolutionary bourgeoisie, which was not, as the proletariat is, a directly oppressed class but a "third" estate. Furthermore, it appears probable, according to Bottomore, that, contrary to Marx's basic contention, the growth of capitalism tends to diminish rather than enhance the revolutionary nature of the working class. With the growth of science and corresponding changes in the mode of production, the middle class is the new and expanding social unity.

Bottomore also explores the issue of cultural autonomy, though less thoroughly. In his most important discussion, he analyzes the independence of political theory by developing the distinction, in reference to both the revolutionary bourgeoisie and the proletariat, between intellectual theories about class consciousness and empirical class consciousness itself. There are also brief remarks about the significance of tradition, intellectuals, and the culture of youth.

One might summarize this two-pronged critique by relating it to Bottomore's overarching ideological and political concern. Although the traditional proletariat is not dead as a political force, Bottomore concludes that in the present historical period the crucial issues for revolutionary change are the direction of consciousness of the new middle classes and the development of an alternative, more explanatory radical theory to replace the original version of Marxism.

This critical agenda informs the two really outstanding essays in the volume, originally contributions to festschrifts for Georg Lukacs, the great Marxist theoretician of working-class consciousness, and C. Wright Mills, who attempted a radical alternative to Marxist theory. In "Class Structure and Social Consciousness," Bottomore deals sharply with one of the crucial, and most puzzling, issues in Lukacs's thought, the assumption that the growth of capitalism will inevitably produce radical consciousness in the working class. After suggesting that the growth of capitalism serves instead to obscure that consciousness, Bottomore proceeds to undermine what might be termed Lukacs's residual alternative, that radical consciousness will be brought to the proletariat from outside by the vanguard party. He points out that, because Lukacs makes no reference to the different class basis of such party leadership, his attempt to develop a valid theory of the historical development of proletarian consciousness is doomed to failure.

The essay on Mills, "The Administrative Elite," is typical of Bottomore's intellectual style at its best. Though rarely mentioning Mills and never directly criticizing his work, Bottomore quietly undercuts the theoretical basis of his power-elite thesis. He does so by analyzing the phenomenon of cross-national variation in the power of administrative elites, according to three criteria to which Mills paid no systematic attention in the development of his famous argument.

Despite their overall high quality, however, these essays, within the radical topography, betray certain flaws which become more important in Bottomore's efforts to step outside that framework. In the first place,



his critique of determinism has a tendency, on occasion, to develop into an atheoretical indeterminacy; and what is for the most part a refreshing willingness to admit that all the facts are not yet in can become instead a frustrating ad-hoc-ism. The most striking example is the longest and most ambitious essay in the volume, "Conflict and Social Change." Instead of producing general conceptualization, it is vague and undirected. And the argument, though rooted more in Simmel than in Marx, lacks significant effort at theoretical synthesis or integration.

Closely connected to this weakness is a second, which might be characterized as a lack of concern for, or perhaps a certain insensitivity to, more general abstract theoretical issues. Despite his own theoretical insight into the pressure points of Marxist thought, Bottomore persistently takes a self-consciously positivist perspective on the tasks of sociology. For example, in "Karl Marx: Sociologist or Marxist?" he makes the implausible contention that Marx's work must be considered purely as a body of inductively arrived-at empirical generalizations which relied not at all on any a priori "metaphysic" (p. 78). Indeed, if there is one major fault in Bottomore's practice and interpretation of Marxism, it is his failure to appreciate the sociological significance of the metaphysical debates within Marxism today. This problem affects even his most successful essays, for example, the excellent critical essay on Lukacs.

The real significance of these weaknesses, however, is manifest only in the first section of the volume, where Bottomore confronts liberal sociology in three highly polemical essays, the most important of which are directed at Lipset and Parsons. What he utterly fails to comprehend here is any congruence between Lipset's and Parsons's concerns, that is, those of liberal sociological theory, and his own criticisms of Marxist theory. For example, more than any other liberal sociologist, Lipset has explored the reasons for variations in working-class radicalism. He has done so in exactly the terms that Bottomore himself has articulated concerning various effects of "autonomy" of the political dimension in democratic and bureaucratic states. Yet Bottomore's essay "Conservative Man" makes no mention of these aspects of Lipset's work. Even more surprising, what aspects Bottomore does refer to reveal a distortion of Lipset's entire theoretical project. Two examples are Bottomore's attack on *The First New Nation* (New York: Basic, 1963) as idealist and his failure to recognize Lipset's emphasis on democracy as a "democratic class struggle."

If lack of a certain kind of theoretical concern, or sensibility, handicapped Bottomore in his effort to mount a radical criticism of Lipset, it proves positively insuperable in his attempt to deal with Parsons's work. Though the uncharacteristically sardonic and condescending "Out of This World: The Sociological Theory of Talcott Parsons" manages to cover a few of the rough spots, this essay is a serious embarrassment. After a timid attempt to present Parsons's work, revealing barely a nodding acquaintance with its most basic concepts, Bottomore falls back on reproducing the *simplist*e "readings" of C. Wright Mills and A. R. Louch. The

most extended discussion which the author himself contributes condemns Parsons for never having thoroughly discussed the concept of action or its implications for scientific explanation. Since Parsons has, in fact, discussed these matters at great length and with great effectiveness, we are forced to conclude that Bottomore has failed to come to grips with Parsons's work.

As this volume of essays makes abundantly clear, there are in a certain sense two T. B. Bottomores. Within the context of radical sociology, Bottomore probes society and thought in a flexible and theoretically informed effort to resolve what he, along with Birnbaum, views as the pervasive crisis of Marxist theory. When addressing what he considers to be nonradical thought, however, Bottomore becomes a rigid polemicist with a theoretical ear that is decidedly unmusical. Although I have pointed to a certain theoretical uncertainty in his thought as one explanation for this paradox, another factor is perhaps equally important. In evaluating nonradical sociology, Bottomore inflates theoretical and ideological issues. In doing so, he commits the very error which he so studiously avoids in the rest of his work and upon which the success of that work depends.

*The Impact of Crime.* By John E. Conklin. New York: Macmillan Publishing Co., 1975. Pp. ix+294. \$4.95 (paper).

Edward Sagarin

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Certainly a major concern in the area of criminology, and one to which increasing attention has been given during recent years, is the study of the impact of crime, particularly that of the fear and perception of crime, on the quality of life in America and elsewhere. To this, John Conklin has turned his attention in a doctoral dissertation expanded and transformed into a book, a metamorphosis more successful than most such efforts. Written for the student rather than for the public or scholar, the work summarizes neatly, competently (with many reservations), but hopelessly incompletely many facets of criminology which may well be worthwhile supplementary reading: the problem of crime statistics and the dark figure of unknown and unreported illegal behavior, the cost of crime in dollars and its effect on life-styles, and collective and individual responses both to crime and to its anticipation. Although there is nothing original in this material, there is generally a balanced presentation of various views, instead of the one-sided polemical statements so frequently encountered. Particularly well developed is a discussion of whether crime unites or disrupts a community. If at first glance the reader is put off by the juxtaposition of Émile Durkheim and Truman Capote as representing the two points of view, that difficulty is overcome by the balanced discussion. Perhaps it is just this balance that may leave a student unconvinced, feeling that much of what is being written is inconclusive, but he may



also be struck by the realization that there are no final answers yet, that there are valid arguments and counterarguments for each position taken. All of this is predicated on the assumption that the reader can wade through the deadly prose, but for college students this should not be too great a task, used as they are to sociologists capable of turning the most exciting material into soporifics.

What is new in Conklin's rather skimpy book is a study of how people in two communities react to crime, law, police, and strangers. Here the material is at best superficial and made only slightly germane to the body of the book. There are numerous references to crimes by blacks, but their relevance to a study of two all-white communities is hardly apparent. The communities themselves are supposed to be similar in many respects, except that one has a high and the other a low crime rate. But when a huge figure of automobile theft is removed (reasonably so, because theft of insured property seldom involves a threat to personal safety), the two cities become almost identical in their crime rates—so nearly so that this might well have been a study of reactions to crime in cities with differential automobile theft rates. The studies in the two cities sometimes sound like a caricature of sociology: the student or his wife, knowing nothing about a community except what is published in census reports (or at least this is the impression they convey), goes, questionnaire in hand, and rings bells to obtain interviews from 138 people in one city and 128 in the other, out of 200 names randomly chosen from each. The interview lasts an average of 45 minutes, during which time Conklin is able to put his respondents on an authoritarianism scale, work out the Likert scale, and get at their beliefs on myriad questions pertaining to crime. Sometimes he stops and notes that the respondents may be dissimulating. Except for that, he continues to cite the results. Even this is done without tests of significance (only once is it mentioned that a difference is statistically significant), and it appears that many of the conclusions drawn from this ill-chosen sample and hastily administered questionnaire are based on differences that are not significant by any imaginable test.

Some of the questions are simply absurd. Does one need grants to be able to tell us that people believe murder should be against the law? Or forcible rape? There might have been some interesting material here, but Conklin has not tapped it. One of his communities has a very high Italian population; the other is mixed white, ranging from Yankee-English, to Irish, to Italian, to Jewish. The questions on whether certain offenses would be reported to the police might have brought out the entire issue of *omerta* ("connivance"): does one remain silent, in the Italian (and largely Sicilian) subculture, rather than report a matter to authorities? How pervasive is the fidelity to silence?

One of the communities is known as Belleville, the other as Port City. The tradition of using pseudonyms in describing cities and towns that have been researched can probably be traced to the transformation of Muncie into Middletown, one of sociology's worst-kept secrets. Yet it may be highly objectionable to use pseudonyms that presumably protect con-

fidentiality and then tell us that the 1970 population of Belmont, I mean Belleville, was 28,285. No undergraduate need spend more than five minutes in a library to discover the names of the cities that were so flimsily concealed. If there is no issue of confidentiality, then cities should be named so that scholars can utilize the information to advantage. The system used by Conklin and by innumerable others is dangerous because it lulls one into a promise and a belief that confidence is being protected when it is not, and the dangers to the respondents (and hence to sociology as a discipline) are aggravated. Students do not have to conduct their work a stone's throw from where they are living (the stone may be thrown back by someone who has been hurt), and they can put demographic information and other identifiers in round numbers and use misleading but methodologically harmless disguises.

Methodology aside, some of the material in this book deserves sharp criticism. To start with, the author's discussions are often based on extremely limited knowledge of a field: he cites one source many times, as if no one had written on the theme other than a single author with whom he is acquainted. For environmental defense, there is only Oscar Newman, with eight discussions or citations; Conklin has apparently not heard of the superior studies of C. Ray Jeffery and Tom Reppetto. On rumor, Edgar Morin is cited seven times; one would not know that Tamotsu Shibutani, Gordon Allport, and James Patrick Chaplin had done work in this area. And so one could continue.

One of the disturbing sections of the book deals with vigilante groups. With assurance that I do not share, Conklin is certain that the difference between a lynch mob and a vigilante group is very important, because the latter has a continuing and the former a temporary existence. The dichotomy is not so clear-cut, as a history of the Klan will show. But he refers to vigilantes who seize criminals—not suspects, not alleged criminals, just criminals in the language of Conklin, and the language is not sloppy but, rather, reflects his own image. Further, the vigilantes even give these people trials—not judicially sanctioned ones, but trials—before they inflict punishment. Trials? With due process, *Miranda*, *Escobedo*, *Gideon*, etc.? This is dangerous tripe, and I cry for America if Conklin's concepts of vigilantism are ever institutionalized as part of the American way of life (and death).

Let the reader beware. Secondary sources are not to be trusted, even when they are footnoted. Conklin refers to "a Brooklyn neighborhood which is controlled by organized crime" (p. 168). Now, just what constitutes control of a neighborhood or a community is much debated in contemporary political sociology. The citation is to an article written by a member of the U.S. Department of Justice, with the usual disclaimer that the opinions expressed are those of the author and not necessarily the Department. It happens that I have lived in Brooklyn for 40 years, and though the love affair between my city and myself has often been put to the test, it never dies. Further, as a criminologist, I do not believe organized crime is a myth. However, there are no such things as communities

or neighborhoods in Brooklyn controlled by organized crime. Monistic control of a part of a city like New York or Chicago is impossible. Areas are controlled, if that word has any meaning, by police, criminals, city hall, churches and synagogues, influential residents, voluntary associations, political clubhouses, playground directors, storekeepers, local union figures, important ethnic leaders, demagogues, and many others.

But the original source did not even say what Conklin reports: the Justice man (in describing the ethnic control of the Gallo brothers in the neighborhood of Market Street in Brooklyn, which street happens to be in Manhattan) referred only to ethnic solidarity, which led neighborhood people to be "friendly to the organization and its activities and antagonistic to law enforcement officers and their efforts against the Gallos." Could Conklin have confused control of a neighborhood with control of crime in a neighborhood, just as his source confused Manhattan with Brooklyn (a small matter, even to a Brooklynite, but an indication of sloppiness and unreliability)?

In case that is not enough criticism for a book that is not as bad as my comments may imply, I have reserved my most serious difference for my conclusion. I wish Conklin had not entitled his book as he did, for his vision of crime is street crime, ordinary crime (whatever that may mean), crimes that would not be committed by self-respecting and middle- to upper-class people. He tells us that shoplifting has an impact on the prices we pay. But what about price fixing, illegal trusts, illegal lobbying, criminal contributions to political funds, the milk deal, the wheat swindle? He tells us that there is an erosion of morality when people hear that so much street crime is being committed. But what of the damage to morality when the highest persons in the land burglarize, lie, obstruct justice, and commit innumerable other illegal and immoral acts? He discusses what occurred when 38 witnesses stood by while a girl was being murdered. But how many good Americans, like the good Germans, stood idly by while a quarter of a million Vietnamese and Americans were being murdered? The greatest impact on morality of any criminal act that I can imagine was a little piece of attempted larceny when someone almost ran away with my Constitution, to be foiled only because he was hoist by his own petard. And for those whose French, etymology, and recent American history are faulty, this means to be lifted off one's seat by the force of one's own flatulence. In a book on the impact of crime, I think one should make a big stink about this, and Conklin's omission leaves me dismayed.

*Schoolteacher: A Sociological Study.* By Dan C. Lortie. Chicago: University of Chicago Press, 1975. Pp. vii+284. \$9.95.

Elizabeth G. Cohen

*Stanford University*

Lortie's description of teaching centers on three themes: the isolation and autonomy of the classroom teacher, the self-perpetuating character of this

inherently conservative occupation, and the uncertainty of its technology. The causes and consequences of these features of teaching are studied in turn by historical analysis, by comparative analysis of recruitment, socialization, and rewards, and, finally, by examination of what teachers have to say about their work.

Lortie uses data from intensive interviews with 94 elementary and secondary teachers and the results of a survey of all teachers in Dade County, Florida, anecdotally in order to illustrate phenomena.

The chapter on socialization is one of the best in the book. Unlike those for whom special schooling is critical, teachers do not rate formal pedagogical instruction as a major means of learning. Nor does teaching resemble a craft occupation, because practice teaching is, at most, a minor apprenticeship. Teachers rely on years of unformulated experience; they do not claim to have a body of specialized knowledge or to be contributors to the state of the art.

The second highlight of this study is Lortie's documentation of teaching as a highly uncertain technology. Teaching has a greater degree of uncertainty in work assessment than perhaps any other field of work. Teachers never seem to know whether they are making any impact on students except in isolated dramatic instances. Despite chronic anxiety and guilt over their effectiveness, they rarely mention generalized outcomes concerning an entire class, such as achievement tests. Uncertainty and frustration over assessment is exacerbated by high and idealistic goals. Common responses to the question on teacher goals are: making every student learn, relating affectively to each student, and moral socialization.

In connection with this analysis of uncertainty, Lortie captures the dilemma of autonomy in teaching. Teachers want to be left alone because they see themselves as critical in the instructional process; they need to manage an affective relationship with students in order to teach them. Their chronic complaints are of clerical duties and intrusions of other adults on the classroom. Yet they are dependent on readiness of administrators, fellow teachers, and parents to grant them the work conditions they desire; they cannot withdraw but must seek the support of others whom they need in order to make autonomy work.

A persistent blind spot in the analysis is the comparison of orientations of male and female teachers. The author is quite unaware of the impact on teachers of a predominantly female organization with few advancement opportunities for women: "Men and women react differently to the nature of the career line. The steps upward within teaching are too small to satisfy the ambitions of most male entrants; they want the greater rewards associated with administrative positions. But the gentle incline of teaching fits the aspirations most women bring with them; it facilitates their in-and-out plans" (p. 88). Instead of relating the temporary character of women's commitment to teaching to the lack of opportunity in a male-dominated organization for advancement into administration, Lortie attributes sex differences solely to sex roles and the demands of family life.

The recommendations for practical action in the final chapter do not

flow from the data. The author's attempt to foresee the future is weakened because his data are old (1963-64); he could not anticipate the increase in structural changes such as teacher aides and team teaching. Nor could he envision the impact of rationalized programs of individualization sponsored by federal and state programs, or even the simple proliferation of curricular materials in a single classroom.

Lortie expects sterner accountability for teachers to arise from increasing power of the state. This overlooks his own previous analysis ("The Balance of Control and Autonomy in Elementary School Teaching," in *The Semi-Professions and Their Organization*, ed. A. Etzioni [New York: Free Press, 1969]), which indicated that the act of teaching is at present uncontrolled by a highly centralized school bureaucracy. There is no reason to think that state control will be more effective.

His recommendations on the selective recruitment of teachers are worrisome. He finds that the teaching profession recruits those who are initially favorable to the existing school system, and he proposes screening out some of these conservatives. Moreover, he recommends screening in order to select individuals who will be more oriented toward colleagues instead of those who like to work by themselves. Does this imply some sort of ideological test? Although his is a structural analysis, he does not seem to envision that changes in the work structure might be the best way to change individual attitudes.

There are many useful abstractions and fresh formulations in this volume. The many quotations from teachers are an excellent source of naturalistic wording for survey items. Despite my criticisms, it is a useful source book for the researcher in sociology of teaching and recommended reading for professors of teacher education.

*Class, Culture and the Curriculum.* By Denis Lawton. London: Routledge & Kegan Paul, 1975. Pp. ix+117. \$9.50 (cloth); \$4.00 (paper).

Michael H. Walizer

*Western Michigan University*

This small book is a scholarly attempt to intellectually and academically evolve and justify a theoretical basis for curriculum development. Being ever mindful of the current interest in the equality of educational opportunity, the author begins by specifying four historical stages of the movement toward equality. The focus of this work is the fourth stage, the 1970s, where the emphasis is purported to be on the nature and content of the curriculum. According to the author, the basic question confronting education is whether to provide better opportunities to become unequal or to provide equalitarian education through the provision of a *common culture curriculum*. Curriculum is defined as a selection from the culture of a society, including way of life, knowledge, attitudes, and values, and therefore curriculum planning involves making selections from a culture.

The author reviews leading theorists' views on the relationship between

culture and education, providing an analysis of the meaning of culture which would satisfy most sociologists and anthropologists with its brief thoroughness. He makes the somewhat obvious assertion that there is an essential link between one's perspective on culture and one's attitude toward education and curriculum. It is admitted that there are several problems associated with selecting a curriculum from a culture, including the extent to which it is possible to identify a *common* culture in a pluralistic society and the extent to which subcultures should be reflected in curriculum planning. In this connection, the relationship between social class and culture is examined. After a rather long, and somewhat distracting, presentation of a history of the evolution of a working class in Britain, the author sets up several straw men and attempts to demonstrate that there is no convincing argument against the existence of a common culture. This is done by debunking the extreme relativist position that it is as desirable to teach one subculture as any other, and also by offering some specific examples of commonalities that exist for British and American cultures.

Midpoint in the book, the author launches into a discussion on the sociology of knowledge by exploring works of Marx, Mannheim, Berger and Luckman, and Young. This is perhaps the most awkward part of the book and does not offer a smooth transition between the foundation previously developed and the conclusions which follow. However, several key points are made. These include the notions that: classes vary in access to knowledge; different perceptions of reality result from sub-cultural socialization; the nature of knowledge is somewhat problematic; and the sociology of knowledge has not previously been applied to curriculum planning. Focusing on knowledge as the key to curriculum planning, the author defends the organization of knowledge and curriculum around disciplines as opposed to the more traditional subjects. Curriculum planning is now viewed as concerning the selection of knowledge for transmission in schools and the principles by which this selection is made. Differences of opinion which exist as to exactly how many disciplines there are (ranging from five to 12) are reviewed, but the main contention is that the curriculum ought to cover all the disciplines as well as offer a balance among them—including how they are divergent and convergent.

In anticipation of objections to a common culture curriculum, the author specifies three potential areas of disagreement. These include feelings that not all students can benefit from such a high-level curriculum, that it is not practical where there is a wide range of ability among students, and that this approach will lower standards and hold back bright children. These objections are addressed in a very brief manner, perhaps owing to the short length of the book, which does not provide sufficient rebuttal to satisfy the skeptic. A final caution offered is that it will be essential to change teacher attitudes to successfully implement a common culture curriculum.

Three outlines of curricula at different types of British schools are offered as evidence that common culture curriculum planning can be



effectively instituted, but the information supplied is so superficial that an independent judgment about this claim is difficult. A final chapter gives a synopsis of each preceding chapter while stating the hope that the cause of social justice in education can be advanced with common culture curriculum planning.

If this essay is judged solely as an attempt to present a theoretical basis for curriculum planning, it is impressive in its scholarly scope but incomplete in two ways. First, sufficient attention is not paid to philosophical, social, economic, and psychological objections to such a strategy, and second, there really is no specification of the strategy by which specific selections could be made from a culture for inclusion in a curriculum. While the first limitation could possibly be overcome in a larger treatise, the reader is left with the impression that he has not been provided with a theoretical foundation which would allow for the development of practices for identifying a common culture or selecting a curriculum from it. Specifically lacking on the practical side are considerations of the political squabbles which would undoubtedly arise from such a proposed change and the problems which any systematic, overall curriculum plan would encounter in a system where responsibility for such decisions is often very diffuse.

Notwithstanding these criticisms, it is a joy to see a text on curriculum planning which contains such a rich sociological foundation. Even if one is not fully persuaded to embrace common culture curriculum planning, this book would be a valuable addition to the reading lists of most sociology of education courses, as a clear illustration of the application of sociological theory to an educational problem. In addition to the obvious relevancy for courses in curriculum planning, other educational courses could utilize this work as an example of the careful application of various disciplinary theories to one aspect of educational activity.

*School, Family, and Neighborhood: The Theory and Practice of School Community Relations.* By Eugene Litwak and Henry J. Meyer. New York: Columbia University Press, 1974. Pp. xiii+300. \$11.00.

Paul Ritterband

*City College of the City University of New York*

Time was when conventional wisdom told us that schools were an (the?) instrument for generating opportunity and creating a more equitable society. The schools, with their expertise, universalism, and rational bureaucratic norms, would displace the family as the key socializing unit in society. Rationalization of society would inevitably reduce the scope and significance of familial activity. Time was, but that time is no more.

Since the work of Coleman and his colleagues, we have been learning just how little impact schools have independent of family, peer group, neighborhood, and other social "anachronisms." Yet few suggest that the family and neighborhood attempt to take over the basic teaching func-

tions; the formal structure of the school *can* facilitate learning. The problem is that of articulating the formal and informal organizations and institutions, not that of one displacing the other.

This is the problem which Litwak and Meyer face and analyze: How can the special competences and energies of school and community be harnessed in tandem? Their formulation of the issues leads to a beautiful mixture of theoretical and policy analyses in language accessible to both social scientist and social practitioner. They lay out for us the nature of the collectivities and individuals concerned with education, the tasks involved in teaching and learning, and the sources of strain and accommodation which emerge around these tasks.

The authors recognize that there is no one structure or administrative style which offers a perfect solution to the problems of school-community relations. Each structural form or administrative style carries both advantages and disadvantages. There is none which is equally productive and useful for all the tasks to be done. Thus, uniform tasks are best performed within bureaucratic structures while nonuniform tasks are best performed within collegial or primary groups. The problem of administering schools lies in their requiring both uniform and nonuniform tasks, often performed by the same persons within a short time span. Teachers, principals, and paraprofessionals engage in a delicate balancing of essentially contradictory demands. Some resolve the problem by playing it by the book (rational bureaucratic style), some by a highly personalized involvement in the lives of their students and colleagues (human relations style). For them to be fully effective, however, the dilemma cannot be resolved. Those who run schools must be both bureaucrats *and* "buddies," moving from one mode to the other as the task and context require.

The first half of the book can be viewed as an extended theoretical prologue to the linkage problems which are the focus for the second half. Given the differences in norms, values, structures, and resources, how can school and community be most effectively linked? The major linkage mechanisms the authors discuss are the detached worker, the opinion leader, the settlement house, the voluntary association, and the common messenger. For each of these we are given a theoretical analysis of its utility (and disutility) and programmatic guidelines for its use. All too often, the linkages remain instruments of the professional, educational bureaucracy, designed at best to co-opt family and neighborhood into programs and goals generated by educationists. Here we get a sense of how the school's clients as well as staff can initiate messages and how the will of the community can make itself felt in a rational coherent manner. Given the recent history of professional-communal confrontations, the development of usable communication links becomes particularly significant. The richness of detail precludes easy summary of this book's many propositions. Here, too, Litwak and Meyer speak to social analysts *and* social activists.

During the past few years, sociologists have renewed their interest in policy-oriented research. As one of our colleagues put it, a little theory

goes a long way. In this volume, Litwak and Meyer bring together a large body of theoretically oriented research and thinking and focus it on one of the country's key policy issues. Their book should be read and reread by citizens concerned with the operations of schools and by social scientists interested in knowledge for human(e) purposes. They have given us an excellent example of how social theory and social concern can be integrated to the advantage of each.

*Black and White Self-Esteem: The Urban School Child.* By Morris Rosenberg and Roberta G. Simmons. Washington, D.C.: American Sociological Association, 1971. Pp. ix+160. \$5.00 (nonmembers); \$2.75 (members).

Melvin Seeman

*University of California, Los Angeles*

A series of interesting puzzles about self-esteem constitutes the core of this monograph, a study of some 2,500 elementary and secondary school pupils in Baltimore. The major puzzle is presented at the outset (p. 4): black children are not lower in self-esteem than whites; indeed, if anything, they score higher than white children of comparable age and socioeconomic status. That result can be counted as a puzzle in view of our assumptions about the serious personal consequences of minority position—assumptions that go back to the early studies by Kenneth Clark and that have been incorporated into our legal and social history by way of *Brown v. Board of Education*.

If black children ought to have lower self-esteem according to our theories, but do not according to the present evidence, what accounts for the discrepancy? The authors examine and dismiss (rightly, I think) some obvious technical possibilities (e.g., that their six-item Guttman scale is at fault, that the samples are not comparable, or that the controls are inadequate). They then proceed to follow the puzzle's thread through peer-group, neighborhood, and school-related influences on self-esteem. Unhappily (for those who like the plot kept neat and tidy), new puzzles occur and multiply. For example: (1) racially segregated black children do not show the expected disadvantage in their self-esteem; (2) despite the activities and slogans of the sixties, the historic preference for light skin color is redocumented among these black children—yet the darker children do *not* have lower self-esteem; (3) children who do well in school have higher self-esteem, and the integrated black children do perform better (have higher grades)—yet their self-esteem is relatively low (the performance-esteem connection just does not materialize in this instance; (4) both social class and family structure supply further anomalies: among whites, lower-class children and those from "separated" or "never-married" families are low in self-esteem, but these family and class connections do not hold in predominantly black schools.

Happily, these puzzles (and others like them) are not too resistant to explanation when the authors supply both the requisite skill in data man-

agement and a consistent viewpoint which involves the conviction that social context makes a great difference. Thus, in the last analysis, the story really becomes quite simple, and the puzzles are seen to be so only because the *a priori*s we have inherited about race and self are so flat and linear.

Three main themes dominate the authors' efforts to unravel the skein. First, the dissonance or consonance of the *social context*—for example, the black child in a predominantly white versus a predominantly black school—will have a considerable effect on individual attitudes and emotions. Second, among the chief reasons that context is important is that it generates differences in *reference group* processes, so that the black child is sometimes comparing himself with his "membership group" (other blacks) and sometimes with the nonmembership group (whites) with whom he is in close interaction. Third, the judgment of *significant others* (including teachers, parents, and peers) is crucial for self-esteem.

These are surely familiar themes, and they allow the flexibility needed to make the results scarcely a puzzle at all. Thus, desegregation has some positive effects for black children (e.g., interracial friendships and academic achievement), but the effects do not include positive self-esteem, since there are too many opportunities for negative comparison with higher-achieving whites. In the same way, black children in segregated settings, where the norm with respect to family structure is more supportive, do not suffer the consequences of identification with a "never-married" home (but those in integrated settings do). Likewise, though black children value the "light" physical type, they do not consider themselves less good-looking than the white children, they do not compare themselves with whites in assessing the importance of dark skin for their self-esteem, and they find many ways to selectively perceive color and the values associated with it (for themselves, their peers, and their parents), so that ultimately the expected esteem differences between lighter and darker black children are thoroughly muted. On the whole, the existence of a consonant black social context, as well as favorable social comparison processes and support from significant others within that context, operates to counteract the forces that have made the attribution of low self-esteem seem so reasonable—the forces of poverty, low achievement, broken homes, the domination of white aesthetics, and the like.

This is a well-done research monograph, with some untypical strengths of the genre and some typical limitations in scope. Two points in particular (one practical, the other theoretical) may be disconcerting. There is, first, the danger that some will find here a warrant to minimize the importance of racial discrimination; after all, if subordination does not translate into low self-esteem, things may not be so bad (in any case, "these folk can cope"). The argument, of course, is as specious as it is dangerous. Second, this work, like many which rely on the reference-group argument, persuades in a *post hoc* way more than it demonstrates. In that sense, the reader does not really get the key to the puzzle; he gets a reasonable-looking diagram of it.

*Deviance, Selves, and Others.* By Michael Schwartz and Sheldon Stryker. Arnold M. and Caroline Rose Monograph Series. Washington, D.C.: American Sociological Association, 1971. Pp. v+128. \$5.00 (nonmembers); \$2.75 (members).

Jackson Toby

*Rutgers University*

The theoretical point of departure of this monograph is a symbolic interactionist approach to delinquency: "The self is an object to which individuals attribute meaning, and one can expect variations in behavior based on that meaning" (p. 36). "No less than others, the deviant is a role-player in search of an identity which he seeks to create and to keep stable" (p. 24).

The population used to test hypotheses generated within this theoretical orientation consisted of 701 teen-age boys, 582 from a large, lower-class high school in a medium-size Midwestern city and 119 from a high school in an upper-middle-class community. Professors Schwartz and Stryker classified the boys as "bad" if their teachers regarded them as having a high potential for deviance; the rest were considered "good." This use of teacher ratings to predict delinquency has a long history in delinquency research.

To summarize the results, despite top-notch research methodology applied to the difficult problem of measuring the self-concepts of "good" and "bad" boys, the "bad" boys did not seem to have worse self-concepts than the "good" boys. I am oversimplifying complex data that pointed in somewhat different directions for white and for black boys. Nonetheless, the results failed to provide much support for the hypothesis that a poor self-concept attracted boys to a delinquent life-style. Why not? One possibility is invalid research instruments, but this seems unlikely, first, because the research methodology was so clearly superior and, second, because substantive findings did emerge from the data that confirmed results of other investigators using quite different methods. Such findings would not have appeared had the instruments been invalid. The most interesting of these substantive findings was the unexpected one that black boys had *better* self-evaluations than white boys. It was unexpected because the authors probably were unaware of the superb study by Morris Rosenberg and Roberta Simmons which demonstrated that the widespread assumption of low self-esteem of black schoolchildren was mistaken (*Black and White Self-Esteem: The Urban School Child* [Washington, D.C.: American Sociological Association, 1971]). Another possibility, considered by the authors, was that a good theory was not confirmed because of special circumstances pertaining to the sample (most of the cases come from an atypical lower-class school with a charismatic principal who had unusual rapport with black students). Perhaps.

Still another possibility is that the theory itself was at fault. I lean toward this interpretation. Specifically, I do not believe that "bad" boys

permanently maintain a poor self-evaluation. They may have gravitated toward delinquent friends and toward antisocial activities because of anticipated failure in legitimate activities, as I have suggested elsewhere. That is, delinquency may have functioned to *raise* their self-evaluation, if they chose an activity in which they anticipated success as opposed to activities such as schoolwork where failure seemed highly probable. In short, although a *threat* to their self-esteem may have been responsible for the initial attraction to the "bad-boy" role, their self-esteem was only jeopardized temporarily; becoming delinquent became an alternative source of self-respect. The trouble with this line of reasoning is that it is difficult, if not impossible, to test. It assumes that many individuals are so sensitive to threats to their self-esteem that they seek out a deviant role in which they can feel successful in preference to a legitimate one in which they anticipate failure. But if the search for an ego-gratifying deviant role begins before much failure accumulates in the legitimate role, we cannot find low self-esteem among "bad" boys. Neither can we know definitely whether the expectation of failure in legitimate roles led to their delinquent style of life.

A similar argument can be made concerning the authors' failure to find doubts about masculinity among the "bad" boys. Some boys may feel emasculated if they attempt to conform to the expectations of teachers and other conventional adults. By choosing a delinquent role, they may regain a sense of potency. In a theoretical article I argued that "badness" and even violence serve to bolster a primitive masculine self-conception (Jackson Toby, "Violence and the Masculine Ideal: Some Qualitative Data," *Annals of the American Academy of Political and Social Science* 364 [March 1966]: 19-27), but I did not predict that male delinquents would have a weaker sense of masculinity than nondelinquents. Whether they did or not would depend on the success of a compensatory mechanism.

To sum up, the failure of this technically sophisticated study to show that "bad" boys tend to have low self-esteem does not necessarily invalidate the symbolic interactionist perspective on delinquency. Perhaps the social-psychological process is more subtle; perhaps negative responses from significant others are sufficient to *threaten* self-esteem and thereby trigger a delinquent adaptation but not lower self-esteem appreciably. In any case, this monograph will not permit us the luxury of a simple answer.

*Education, Occupation, and Earnings: Achievement in the Early Career.* By William H. Sewell and Robert M. Hauser (with others). New York: Academic Press, 1975. Pp. xviii+237. \$16.50.

*Attitudes and Facilitation in the Attainment of Status.* By Ruth M. Gasson, Archibald O. Haller, and William H. Sewell. Arnold and Caroline Rose Monograph Series. Washington, D.C.: American Sociological Association, 1972. Pp. vi+37. \$5.00 (nonmembers); \$2.75 (members).

James S. House

*Duke University*

The volume by Sewell and Hauser is the most complete report available on the Wisconsin study of status attainment and constitutes a major empirical contribution to our understanding of socioeconomic inequality and achievement in American society. The brief monograph by Gasson, Haller, and Sewell reports some additional analyses from the same data set, but the theoretical discussion and empirical results in it hardly merit a separate monograph, however brief.

Sewell and Hauser's book is dedicated appropriately to Otis Dudley Duncan, as it is the latest in a line of work initiated by Peter Blau and Duncan's *American Occupational Structure* (New York: Wiley, 1967). For those familiar with this body of research, the present work provides new empirical insights; for the less familiar, it offers a clear and up-to-date presentation of what we know about the determinants of occupational, educational, and income attainment among males from nonfarm backgrounds in American society. The book assumes only a basic knowledge of multiple regression and its application in simple recursive path models, and thus should find a wide readership among graduate students and professionals in a broad range of the social sciences.

The Wisconsin study is relatively unique in the status attainment literature in several ways. First, it is a continuing longitudinal study (of a random one-third sample of all graduates of Wisconsin high schools in 1957) with very impressive response rates. Second, it includes significant social-psychological variables as well as data on socioeconomic origins, attainment, and ability. Third, it includes good measures of the income of both the respondents (from Social Security files) and their parents (from Wisconsin state income tax returns), though the nature and validity of these measures should have been more fully discussed in the book. These advantages are offset somewhat by the restriction of the sample to predominantly white Wisconsin students who finished high school, though Sewell and Hauser rather convincingly document that their data yield results quite similar to those of studies on more heterogeneous national samples.

After a brief introduction and a thorough examination of the adequacy of the sample, Sewell and Hauser begin a careful analysis of the determinants of levels of education, occupation, and earnings among *males of nonfarm origin* seven to 10 years after high school graduation. Their first

major theme is the relative importance in status attainment of achievement versus ascription (or ability versus socioeconomic background). Parental status characteristics and mental ability measured in high school explain together about 28% of the variance in educational attainment and 20% of that in attainment of occupational prestige, with ability slightly more important than background factors in both cases. But the same variables explain only about 5% of the variance in sons' earnings in 1967, with socioeconomic background slightly more important than ability. Adding the sons' education and occupation to the list of predictors increases the explained variance in earnings to only 7.4%, although the age of the sample undoubtedly accounts for some of the lack of explanatory power. Sewell and Hauser have data on parental income that are critically lacking in many status attainment studies. Parental income has a clear, though modest, impact on educational, occupational, and especially earnings attainment; in the latter case, parental income accounts for virtually all of the variance in sons' earnings which can be explained by family background.

Sewell and Hauser then examine a set of social-psychological factors (high school grades, parental and teacher encouragement of higher education, friends' plans, and respondents' college plans and occupational aspirations—all measured just prior to high school graduation). These variables both account for additional variance in attainments and explain how and why prior family background and ability affect attainment. The variance explained in both educational and occupational attainment is almost double that of the model including only socioeconomic background and ability, but there is almost no increase in explained variance in earnings. Most of the effects of socioeconomic background on occupational and educational attainment are mediated through these social-psychological variables. A chapter by Sewell, Hauser, and Alwin considers whether type of college attended can account for additional variance in income attainment and explain why wealthier parents have wealthier sons. The effects of college type are evident but quite modest (e.g., 2% increment in variance explained by income).

The final chapter (no. 6) of results, prepared by Janet Fisher, Kenneth Lutterman, and Dorothy Ellegard, seems out of place in both style and substance. A potpourri of data regarding the additive and interactive effects on earnings of ability, education, military service, and farm versus nonfarm background are presented, but to what end is never clear. Sewell and Hauser make no effort to clarify the meaning of their concluding chapter but, rather, merely repeat almost verbatim the chapter summary.

With the exception of chapter 6, this book maintains the highest standards in the collection, analysis, and presentation of data. One would only like to see more discussion of the implications of the findings. Although the authors anticipate more definitive results from later waves of data, their present results clearly replicate and extend earlier work. The authors, like Christopher Jencks in *Inequality* (New York: Basic, 1972), are struck by the degree to which educational, occupational, and



especially income attainment remain unexplained after socioeconomic background, ability, and a variety of other factors are considered. These rather consistent findings call for a reworking of both radical and conservative views on the nature of stratification in America. Ascription and achievement seem about equally operative, but neither is the primary determinant of achievement in American society. The process is in large part a lottery, but the cruel hoax is that most people continue to feel the outcome is their own fault, as Richard Sennett and Jonathan Cobb so poignantly suggested in *The Hidden Injuries of Class* (New York: Basic, 1973). The data which Sewell and Hauser summarize and present bear on important theoretical and normative issues. The implications of the data, not entirely clear, deserve to be more fully considered, especially by those who know the data best.

Using the same data set, Gasson, Haller, and Sewell ask whether "facilitators" (socioeconomic origins, mental ability, and significant-other influence) and "attitudes" (educational and occupational aspirations) tend (1) to "cluster" (intercorrelate), (2) to relate curvilinearly to occupational and educational attainment, and (3) to interact with each other in predicting status attainments. As previous research had already shown, the attitudes and facilitators are indeed moderately intercorrelated. Although plausible, the theoretical justifications for the interaction and curvilinearity hypotheses are not overly compelling. This fact, coupled with the intercorrelation of the attitudes and facilitators, makes it quite unsurprising to both the authors and the reader that there is little evidence of curvilinearity or interaction. This analysis would have made a useful and interesting chapter or appendix in the Sewell and Hauser volume or an article in a journal. But as a separate monograph it seems pretentious and labored even in 37 pages (11 of which are devoted to figures, the size of which is vastly out of proportion to their informational value).

*Workingmen of Waltham: Mobility in American Urban Industrial Development, 1850-1890.* By Howard M. Gitelman. Baltimore: Johns Hopkins University Press, 1974. Pp. xvi+192. \$10.00.

Lawrence E. Hazelrigg

*Indiana University, Bloomington*

Howard Gitelman's research monograph is another in the growing series of the so-called new-history case studies of 19th-century U.S. cities. Here the setting is Waltham, Massachusetts, founded in 1738 10 miles west of Boston on the banks of the Charles River. Still a village of 882 in 1790, Waltham grew to 4,464 in 1850 and then to 18,707 in 1890. The main economic forces during the middle and late 1800s were contributed by the Boston Manufacturing Company, famous for its then innovative policies of labor recruitment and management (the hiring chiefly of young,

"literate, pious, and healthy country girls" who were quartered in company boarding houses [p. 3]), and the Waltham Watch Company.

The monograph begins with general remarks about the research, and a sketch of early Waltham, following which Gitelman takes us through analyses of migration, occupational mobility, social mobility (i.e., assets, power, organizational roles), government and power, and neighborhoods and social life, and then to a final, recapitulative chapter. A brief outline of methods and a presentation of the classification of occupations form two appendixes.

Predominantly, the analyses are of numerical data files constructed from such archival sources as the manuscript federal census schedules, state census returns, municipal tax rolls and registers of births, deaths, and marriages, city directories, and company files. It is a history not of the "notables" of Waltham but of aspects of the recovered male population. Its focus is not on events, attitudes, decisions, etc., that constituted day-to-day life in Waltham, but on certain of their structural outcomes. The authoritative assertion of the referent (i.e., the reality of 19th-century Waltham) resides less in the traditional structure of narrative discourse than in that of numerical juxtaposition and analysis.

For anyone already familiar with the new-history case studies of 19th-century U.S. cities, conclusions drawn in this volume will hold no surprises. Gitelman does give special attention to the labor-force populations of the two major employers, Boston Manufacturing and Waltham Watch, and interesting contrasts between them are noted, but the results of these subpopulation analyses mostly parallel and/or add detail to conclusions from the larger population. I shall not recite the whole list of Gitelman's findings, only some of the more central ones. Waltham, like other towns and cities that have been researched, experienced an enormous turnover of people between 1850 and 1890: "at least 10,987 and perhaps as many as 17,000 males in-migrated, and at least 6,158 and perhaps as many as 12,000 males out-migrated" (p. 165). Apparently, occupational mobility became increasingly frequent during the four decades—although Gitelman's choice of designs makes the estimation and analysis of trend unnecessarily obscure and the just-mentioned conclusion doubtful. Catholics (especially Irish Catholics), who constituted an increasingly larger proportion of Waltham's inhabitants, entered the labor force earlier, at lower levels, and with fewer avenues of upward mobility. Inequality of asset holding grew, though perhaps at a reduced rate during later decades, primarily because the principal asset for most asset holders was homeownership, a low-income-bearing property, while an ever-smaller minority of the population enjoyed high rates of return on investments and business properties and held occupations that experienced a disproportionately high rate of increase in direct income. "The high degree of correspondence between persistence, asset accumulation, homeownership, and fairly high occupational standing produced a situation in which the men most likely to know one another and to be known in the community were the materially successful" (p. 168); they were most likely also to occupy positions of

social and political leadership in voluntary associations and local government. Residential and associational segregation increased, along socioeconomic but also religious-ethnic lines. Overall, Gitelman concludes his examinations of the various aspects of Waltham social structure with the singularly unsurprising observation that "Waltham society became more markedly stratified" as the town of less than 2,000 people in the early 1800s became the city of nearly 20,000 in 1890 (p. 169).

The monograph has serious shortcomings. For one, Gitelman's always spare discussion often strikes one as more like undernourishment than muscular trimness. The list of specific findings is quite short indeed, not for reasons of a methodological cautiousness but because the data were utilized incompletely. Given the labors that went into the production of data (a process that Gitelman does not always describe adequately), one would have wished for a greater return through applications of more informative techniques of analysis. Likewise, one would have wished for a comparative thread in the presentation, that is, direct comparisons with information constructed in studies of other 19th-century cities. As these case studies accumulate, careful attention must be given both to the particularities of this or that city and to comparability of research design (including the mode of data construction, analysis, and presentation) in order that gains from the large investments required by the studies may be maximized.

*Ernest W. Burgess on Community, Family, and Delinquency: Selected Writings.* Edited by Leonard S. Cottrell, Jr., Albert Hunter, and James F. Short, Jr. Chicago: University of Chicago Press, 1973. Pp. ix+337. \$13.50.

Ralph H. Turner

*University of California, Los Angeles*

It is a tribute to the breadth of Ernest Burgess's sociological interests and to the range of his contributions that four editors, representing different specialties, have collaborated to prepare this collection from his work. Albert Hunter, assisted by Nancy Goldman, extracts the principal themes and contributions from Burgess's work on the community. The central importance of the concentric-zone theory in this work has often led students to underemphasize two features of Burgess's approach. First, Burgess saw concentric zones as a framework within which to study the community as a dynamic, changing phenomenon. And second, Burgess always used the ecological approach in conjunction with investigation of the way social definitions and local cultures develop differentially in the community. Leonard S. Cottrell, Jr., assembles six selections that reveal the range of Burgess's interests in the family. From his classic theoretical exposition (1926) of what we have come to call the nuclear family, to the radically atheoretical work on measuring and predicting adjustment in marriage, to his later efforts in staking out the field of social gerontology, Burgess

touched on nearly all phases of family study. James F. Short, Jr., outlines the contributions to the study of crime and delinquency. Again, there is the characteristic range from atheoretic prediction of parole success to efforts to establish a theoretical foundation for understanding delinquency as an aspect of the total person. Finally, Morris Janowitz presents Burgess in his role as a methodologist concerned with making social research useful in human betterment and exploring the interface between research and values.

Burgess was so distinctive a human being that it is difficult for those who knew him to read his work without seeing and hearing the man. His gentleness and his attentiveness to the feelings and concerns of others led some of his admirers to accept uncritically his sometimes oversimplified formulations and naive observations about the real world. Indeed, Burgess's work does abound with overly simple distinctions—individual and person, personality and social type, criminal and noncriminal self-conceptions, traditional and companionship family—and his case studies often read more like parables than like the confused and contradictory documents that most of us elicit from unsensitized subjects. On the other hand, the conspicuous propriety of Burgess's manner led many who considered themselves sophisticates to deny his work the serious attention it deserved. His simple and direct mode of presentation, which credited his auditors with sufficient intelligence to recognize and make standard qualifications and which eschewed the use of catchy or obscure terms to popularize his observations or mystify his readers, often meant that the profundity of his thought went unrecognized.

Some of Burgess's work is dated and of chiefly historical interest. This is most true of his methodological work. In one sense, the pioneering use of "gut bucket" prediction methods in the areas of parole and marriage stimulated the development of more refined methods of prediction and evaluation in sociology. Burgess's method produced a storehouse of dissociated findings which provided leads for more refined analysis by himself and other investigators for a generation. But the point of diminishing returns has been passed for this method of prediction without its being demonstrated that the use of these procedures improved marriages or increased the efficacy of the criminal justice system. Burgess also expressed a conviction that standardized methods for analyzing case studies would displace intuitive approaches. Although methods of content analysis have flourished and the "general inquirer" carries dissection to its ultimate limits, case study method is still principally intuitive or even simply illustrative. Substantively, today's scholars can hardly accept Burgess's comfortable assurance that yesterday's indications of family disorganization were not harbingers of Zimmerman's atomistic nonfamily but the birth pangs of a new family relationship securely bonded in companionship. Burgess recognized intellectually the essential ambivalence of family relationships, but his heart kept him from taking these centrifugal forces seriously, as Waller and Folsom did.

But certain of Burgess's writings are gems that should be on contem-

porary reading lists, not for their historical value so much as for the currency of their ideas. My personal favorites are three—all marked by the disarmingly simple exposition of a profoundly useful approach to problems of great current interest. In "The Growth of the City" (1925) Burgess outlines the dynamic yet patterned character of the urban community, examining organization and disorganization through the metaphor of metabolism. "The Delinquent as a Person" (1923) outlines a generally applicable framework for the sociological study of personality, clarified with interesting case materials. "The Family as a Unity of Interacting Personalities" (1926) develops the assumption that "the family as a reality inheres in the conception which society and its members have of it and of their roles as husbands and wives, parents and children" (p. 94).

*A Family Album: Portraits of Intimacy and Kinship.* By Thomas J. Cottle. New York: Harper & Row, 1975. Pp. xiv+224. \$7.95 (cloth); \$3.75 (paper).

John Moge

*Boston University*

This is a book of short stories centering on themes that the author and his informants thought of as universals of the human life cycle: family, intimacy, kinship, living, and dying. I place it in a literary tradition because the author says "I have . . . recreated conversations and then have had them examined for accuracy by the participants" (p. xiv). Only the latter process keeps the book from being fiction and qualifies it for review in a professional journal.

The use of recall as a technique for the reconstruction of the content of a conversation is known to be inaccurate even when handled competently, as in this instance. Asking informants whether a written test corresponds to their recollection of a conversation tends to produce only a small increase in accuracy, for the replies are likely to be colored by all sorts of extraneous considerations. No systematic attempt was made to be sure that informants concentrated on any sequence of controls over their processes of recollection. No tape recording of a conversation is like this text: these people speak in sentences and paragraphs, with none of the vagueness, opacity, and meaningless connectives of actual speech. The conversations are transformations of actual empirical events, the transformations being accomplished through the application of rules of grammar and syntax and the conventions of a literary style. The book falls into a sociological tradition that existed before tape recorders and questionnaires were used, a tradition still used in some types of community or family studies where the evocation of mood, or the recreation of the affective aspects of a particular event, episode, or ceremony is the important social fact to be communicated.

Another standpoint from which to judge this book is that of ethnography. Because informants are given fictitious names and neither places nor dates

are provided, accuracy can never be assessed, but the whole volume concerns the way individuals describe segments of their patterns of life in America today.

The reports in "Childhood" include an episode in which the interviewer sees and hears a happy, united, successful marriage but the child tells all about the anger, fighting, and drinking the parents engage in in private. The section "Adulthood: A Place between Living and Dying" describes various feeling states such as the loneliness of a young college girl, the anguish of a first sexual experience, an encounter with racism and rebellion. "Parenthood: No Place to Look But Back," the third section of the life cycle, reports a mother's feelings when she has battered her child almost to death and a father's feelings when a wife takes her revenge for a divorce by interfering so cleverly with his access to his own children that eventually he loses a court appeal. Some grandparents look backward down two or three generations of change in America.

The writing is beautiful. Once the fact is accepted that, on the spectrum from the purest literary fantasy to the most concrete, dull, empirical attempt to record all the facts about one social encounter, this book lies nearer to literature than to sociology, then it is highly agreeable to read. The last 18 pages, the "Epilogue," are an attempt to introduce and apply some analytical concepts; this section too is best considered primarily as literature. The other bow to scholarship is a workmanlike bibliography of around 400 items, mostly published since 1946; however, no systematic use is made in prologue, text, or epilogue of any concepts, theories, or measurements in any of the reference items.

Yet, having read the prologue as a positivist and therefore in a hostile mood, I found myself charmed by the text, carried along from story to story, and, finally, as you can see, persuaded that although it is nothing any conventional professional sociologist would sign his name to, the book has both merits and uses. The merits have been sufficiently extolled. The uses are for courses on the family. For the course that takes general sociological concepts and theories and tries to use the family as a unit in which they might be tested, the book would be useful as descriptive material on affective states that students often ignore. For the functional course on the family which has no relationship to structural functionalism as a theoretical orientation, the book provides material for discussion over the whole life cycle. I ought to qualify even this moderate recommendation, for although the book is ostensibly about the life cycle, there is nothing developmental about it. It is firmly episodic, discursive, descriptive, and delightful.

*Mathematical Models of Group Structure.* By Thomas A. Mayer. Chicago: Bobbs-Merrill Co., 1975. Pp. 79. \$2.25 (paper).

Maureen T. Hallinan

*University of Wisconsin*

Mayer's monograph brings together and explicates models of small-group structure. His stated rationale for this endeavor is the importance of the study of group studies reveal uniformities in social phenomena, information processes, and identify causal agents in the production of social phenomena. All the models presented, with accompanying frameworks and mathematical definitions and theorems elsewhere, specifically in the journals and books reviewed, the value of Mayer's work, therefore, is that it allows a sense of what has been done in this area, to evaluate the models, and to think about alternate approaches to group structure.

Mayer organizes the models into three categories: solidarity, structural equilibrium, and structural hierarchy. In the section of structural solidarity he includes the traditional model of clique structure defined by Luce and Perry in 1949, the algebraic model of cliques, based on the notion of a matrix, and Mayer reviews at length the matrix algebra technique used by Luce and Perry and by Harary and Ross to identify key persons strategically located within a group, such as liaison persons, and strengthening members. Mayer argues that either model can contribute to an understanding of group structure. At the same time, he criticizes traditional clique definitions employed are mechanical and opportunistic. Boyle's work "has the flavor of a programmatic manual that future researchers study nonbinary relationships."

The second set of models, those of structural equilibrium, stability or instability of group structures at a given time section, Mayer includes the balance and generalization models of Cartwright and Harary, Davis's clustering model, and Leinhardt's ranked-clusters model. He omits a number of models including Holland and Leinhardt's transitivity model mentioned are special cases. The balance model divides a group into disjoint subsets or cliques, the clustering model allows overlapping cliques, and the ranked-clusters model is a structurally ordered cliques. Mayer points out that the generalization model while intuitively more appealing than the balance model does not possess the attractive mathematical properties of deterministic models. He also discusses briefly the utility of line models as employed by Davis and Leinhardt. In the final section by questioning the empirical adequacy of the

well as their theoretical importance and compares efforts in this area to algebraic curve fitting.

The last section of the book contains three models of structural hierarchy: Harary's formalization of French's theory of social power, Friedell's algebraic models of hierarchical organization, and Landau's dominance models. Harary's model employs a relative influence vector to determine the power which structural position accords a group member. Since Mayer finds French's theory mechanistic and unrealistic, he questions the usefulness of the Harary formalization. Friedell formulates a semilattice model and presents it as an ideal type paradigm for the hierarchical structure of organizations. Mayer finds Friedell's model intuitively unappealing but sees it as revealing the complexity of structural relationships. The work of Landau, a mathematical biologist, includes a model of hierarchy based on the inherent characteristics of group members and a social process model based on group interaction. After devoting considerable time to Landau's models, Mayer points out that both are built on rigid assumptions, reasonable perhaps in biology but unrealistic in human groups. He also notes that very few studies emerged from Landau's work. He neglects to mention here the empirical work of Mayhew and Gray.

The task Mayer set himself was an exposition of mathematical models, not an evaluation of them. In this regard, his selection is good but limited. He stresses static models and, with the exception of Landau's, ignores the few existing dynamic models of change in group structure. The static models chosen, if the transitivity model were included, would present a fairly accurate view of what has been done in this area.

In his concluding comments, Mayer argues that "the most obvious weakness [of mathematical modeling] is the continuing disjuncture between empirical research and the model building enterprise." A growing number of empirical tests of the models selected by the author do exist, however, in particular the models of structural equilibrium. Mayer's work would have been enhanced by mention of these tests, or at least by a comprehensive bibliography of empirical studies related to the models and of additional theoretical models.

Unfortunately, Mayer chose not to evaluate the models discussed. However, he does offer general impressions of each model, most of them negative. His main objection is that the models are divorced from reality. He would have provided a more valuable service to the reader if he had scrutinized each model more closely, revealed which assumptions were questionable, pointed out the role those assumptions played in the model, examined the possibility of altering them, and discussed the value of the model given a less-than-perfect fit to empirical data. This analysis would have been especially helpful if the book is to be used for teaching purposes or to stimulate further research in the area.

Finally, Mayer claims that social structure is an essential part of sociological theory and research but that the concept is vague and elusive. By intimating, on the one hand, that social structure has a kind of mystical importance and, on the other, that it can easily be used as an independent



or dependent variable in sociological analysis, Mayer does little to elucidate the concept. Nor is his justification of its importance very convincing in the absence of empirical studies to support his arguments. However, to grapple in a systematic manner with the notion of social structure is admittedly beyond the scope of the book.

Mayer's monograph is a helpful teaching guide and a convenient reference for persons interested in small groups or mathematical modeling.

*The Illusion of Attitude Change: Towards a Response Contagion Theory of Persuasion.* By Jozef M. Nuttin, Jr. Louvain: Leuven University Press; New York: Academic Press, 1975. Pp. xii+236. \$14.50.

Martha S. C. Arnold

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This book is an important and controversial contribution to social-psychological research and theory. Jozef M. Nuttin, Jr., reports on a brilliantly designed and executed series of "forced-compliance" experiments that led him to formulate a "response-contagion" theory of (apparent) attitude change. The theory postulates a basic psychological process by which evaluative responses emitted when a person is emotionally aroused become temporarily dominant in that person's response repertoire, even when a different evaluative response would normally dominate. In the forced-compliance experiment, the experimenter asks for an evaluative response immediately after the subject has given counterattitudinal responses. These responses are dominant because the subject is aroused; as a result, the subject's attitude appears to have changed. The arousal is caused by unusual stimuli that occur when the counterattitudinal speech is elicited. In the traditional forced-compliance experiment, the unusual stimulus is the low reward, which, Nuttin argues, is inappropriately low and therefore disturbs the subject. Nuttin succeeded in obtaining "attitude" change comparable to that obtained with a low monetary reward by using other unusual stimuli: an illegitimate *high* reward of extra examination points (offered to students with poor exam scores), an experimenter who unjustly accused a subject of disrupting her classes, and an inappropriately dressed experimenter. In an excellent discussion, Nuttin shows that neither dissonance theory nor incentive theory can account for these results. Although he does not discuss the self-attribution interpretation of forced compliance, I believe that it, too, would fail to account for these data.

Although this work is a brilliant and creative integration of theory and data, it has several weaknesses. First, in every case Nuttin's unusual stimuli were experimenter behaviors that violated normative expectations. He did not create emotional arousal in other ways, nor did he measure arousal directly. Also, he did not measure the feelings toward and perceptions of the experimenter that were evoked by the unusual behaviors. Such attitudes might interact in complex ways with subjects' beliefs about experi-

menter expectations, willingness to meet those expectations, and conceptions of socially appropriate ways to feel in a particular situation. Admittedly, it would take much ingenuity to explain all of Nuttin's findings as having resulted directly from the violation of normative expectations; however, without direct evidence of the role of emotional arousal, such an explanation cannot be ruled out.

Second, as Nuttin himself notes, the existence of response contagion as a basic psychological process has not been established. Until such an effect has been well documented, response-contagion theory must be considered a very tentative explanation of attitude change in forced-compliance experiments.

Third, the theory is narrower than the cognitive theories it attempts to replace; it cannot explain the results of the many forced-compliance experiments that do not deal with attitude change (e.g., experiments on changes in perceived motivational states). Moreover, response contagion cannot explain the many successful interpersonal simulations of forced-compliance experiments.

Nuttin points out that the implication of his analysis is that what is studied in so-called attitude-change experiments is not attitude change at all. The verbal evaluative response changes, but there is no reason to believe that the postulated underlying attitude (defined as a general predisposition underlying a range of verbal and other behaviors) has changed. Therefore, attitude scales are poor measures of attitudes, because the verbal evaluative responses they elicit are very sensitive to situational influences.

But Nuttin's argument goes beyond measurement problems; in a discussion that unfortunately lacks the clarity of the rest of the book, he questions the "theoretical and practical usefulness of the 'attitude' construct" (p. 190). Since his experiments illustrate that it is relatively easy to create situations that will elicit "counterattitudinal" behavior (the evaluative implications of which contradict verbal evaluative responses) even when the "attitude" issue involved is very important to subjects, he sees no reason to assume that the behavioral responses are determined by underlying predispositions any more than are the verbal evaluative responses. Instead, he sees both verbal evaluative responses and other behaviors as caused by situational determinants. Since they can be caused by different situational determinants, they may appear to contradict each other if taken as evidence of an underlying attitude; however, the apparent inconsistency vanishes if we eliminate our assumption that the attitude exists. The "attitude" response and the "counterattitudinal" behavior are merely two different behaviors.

However, Nuttin does not say how we are to explain individual differences in behavior in the same situation without some concept of underlying behavioral predisposition. We might postulate that different behaviors result from different *cognitions* of the situation, so that what appears to be the same situation is not the same for all, but Nuttin does not make

such an argument. And without some way to explain individual differences, his attempt to eliminate the concept of attitude on heuristic grounds cannot succeed.

*The Volunteer Subject.* By Robert Rosenthal and Ralph L. Rosnow. New York: John Wiley & Sons, 1975. Pp. xiv+266. \$14.95.

Shalom Schwartz

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Experimental social psychologists worry much about internal validity in their research but speak little of the limits on generalization imposed by unrepresentative sampling. Rosenthal and Rosnow direct our attention to the hazards of estimating population parameters from experimental effects. More strikingly, they explicate and illustrate how lawful relations among variables detected in experiments may disappear or even reverse with different samples. They provide a wealth of information about the correlates and causes of volunteering to participate in behavioral research and argue convincingly that researchers should consider the implications of their subjects' volunteer status.

But beware of overreading! For all its emphases on artifacts, this volume discredits few, if any, conclusions from past experiments. Why? Because researchers seldom err by confounding their manipulations with their subjects' volunteer status.

The first half of the book systematically, if sometimes tediously, reviews hundreds of studies relevant to characteristics of volunteers and situational determinants of volunteering. Both the directions and magnitudes of observed effects are presented for each of 32 variables, and levels of confidence are assigned to conclusions which are drawn. Not surprisingly, volunteering is related to education, need for approval, interest in the subject of a research project, and expectations of favorable evaluation, among other variables. More thought provoking are associations presented less confidently, such as those involving unconventionality, nonauthoritarianism, and maladjustment. Moderator variables, such as type of task, are proposed to resolve some of the inconsistencies in effects between studies. Females, for example, are more likely than males to volunteer for research in general but less likely to volunteer for physically or emotionally stressful studies.

Overall, the pattern of findings for volunteer characteristics manifests a peculiar feature: 57% of the studies have significant results, but 20% of these oppose the prevailing trend. Some of this inconsistency is attributable to the use of different measures for single characteristics and some to moderator variables. But some is undoubtedly also due to the nonunitary quality of volunteering itself. As contexts change, the meaning and correlates of volunteering must change, too, as the findings for situational determinants imply. The authors probably overrate the reliability of vol-

unteering. The data they cite—a median reliability of .42 for volunteering for different activities—is based on only four research reports.

The high proportion of significant yet inconsistent effects found in the literature raises another question: Might the predilection of researchers and editors for publishing significant findings account for this pattern? If approximately three times as many inconclusive studies were performed but not published, the number of significant results opposing prevailing trends would be in line with chance expectations. The authors do not discuss this possibility, nor, unfortunately, do they describe how they located the studies to review.

Regardless of these reservations, Rosenthal and Rosnow have established that volunteer samples are likely to be unrepresentative. The practical methods they suggest for reducing volunteer bias are therefore welcome, and I hope they will facilitate and encourage recruitment of more diversified samples. The authors estimate that 80% of the human subject samples in psychology research are college students; I counted 66% in the 1974 volume of *Sociometry*.

In an important digression, the authors note that growing ethical pressures advocating informed consent from subjects could pose a dilemma for researchers by introducing another threat to validity. In a verbal conditioning experiment, for example, fully informed subjects showed a reversal in conditioning from the well-established pattern which was replicated with uninformed subjects. Ironically, the former expressed *greater* suspiciousness of the whole procedure!

A short and perceptive review of the literature on demand characteristics serves as background for a series of original studies on the responsiveness of volunteers to implied demands and on conceptions of the subject role. A study of the effect of pretesting on attitude change is especially interesting. It "suggests that using a before-after design can lead to overestimates of the attitudinal effects of persuasive communications when the subjects are [volunteers] . . . and to underestimates when they are captive nonvolunteers" (p. 155). Thus, volunteer status may interact with experimental conditions to produce errors of either type. Volunteers and nonvolunteers seem to agree in associating the subject role with altruism and evaluation, but nonvolunteers associate it more closely with work.

The authors present a general model of artifact influence processes into which they integrate volunteer effects. These are postulated as operating on a subject's motivation to accommodate to role demands he notices and is capable of enacting, with volunteers generally more accommodating. They conclude by discussing strategies for circumventing and/or identifying artifacts (field research, deception, quasi control, role play). They particularly recommend a strategy called "dual observation," which entails measuring the dependent behavior a second time outside of the original context with its demand contingencies. Each of these strategies raises problems of its own, only a few of which are mentioned. But concern with demand characteristics should not be exaggerated. Subjects *may* be influenced by them, especially if cues revealing the researcher's goals are deliberately

highlighted. There is little evidence, however, that subjects in typical experiments actually do form systematic hypotheses consistent with experimental manipulations and exhibit motivation to behave in response to these hypotheses.

This volume has much to say to those interested in experimental methodology, volunteering, and the social psychology of research. Some of its insights are relevant to surveys as well. Its concise summary chapter and detailed index should facilitate its use as a valuable reference.

*Caribbean Transformations.* By Sidney W. Mintz. Chicago: Aldine Publishing Co., 1974. Pp. xxii+355. \$12.95.

*Marriage, Class and Colour in Nineteenth-Century Cuba: A Study of Racial Attitudes and Sexual Values in a Slave Society.* By Verena Martinez-Alier. London: Cambridge University Press, 1974. Pp. x+202. \$13.95.

H. Hoetink

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Sidney W. Mintz is one of the very few North American social scientists who have dedicated long careers exclusively to the Caribbean area, and he is probably the most gifted among them. An anthropologist who for many years worked at Yale University and only recently moved to Johns Hopkins, Mintz, the author of *Worker in the Cane* (New Haven, Conn.: Yale University Press, 1960), has in the volume under review collected 11 of the numerous articles, reviews, and forewords that he wrote during the past 25 years. For the most part, the collection deals with three societies—Puerto Rico, Jamaica, and Haiti—and with three general themes—the relations among slavery, forced labor, and the plantation system; the (historical) anthropology of Caribbean peasantries and market systems; and the theme of national and Caribbean identities.

The book is divided into three parts, each devoted to one theme. Each section is preceded by an introduction, the first of which, on "Afro-Caribbeana," is of a more general character. It exemplifies the many virtues of Mintz's scholarship: an accomplished sense for careful, detailed exposition; a precise definition and use of theoretical concepts; a cautious balancing of viewpoints without relapsing into a sterile "neutrality"; a taste for the illustrative anecdote. His oft-quoted review of Stanley M. Elkins's *Slavery: A Problem in American Institutional and Intellectual Life* (Chicago: University of Chicago Press, 1959) and his work on the Jamaican market system and on Caribbean houses and yards are delightful further examples of his control over theme and style.

Where Mintz deals with "race" in the Caribbean, as in the last section of this volume, he is careful to show the reader the particular social dimensions and significance the issue has in each specific society. Time and again he stresses the importance of social perception in the "racial" labeling of individuals and groups and of the social context by which such perception

is influenced. Without denying here in the least the usefulness of such a "perceptionist" approach—especially in an area where such a variety of "racial" systems obtains—it may be pointed out that in its extreme consequences such an approach denies the reality of social consensus and thus of social comparability: each individual perception is influenced by a social context which, to some extent, is unique and perceived as such. Once reality is postulated to be in the eyes of the beholder, no part of it can escape that fate. Just as someone may be perceived as a black in one context and as a mulatto in another, so he may be perceived as wealthy in one and as below the poverty line in another. Terms such as "large landholdings" and "small peasant farms" should be allotted, then, a degree of "reality" similar to that allotted other categories, in order to produce a consistent perceptionist view; such consistency would not necessarily prevent one from postulating that the economic part of "reality," however perceived, has more explanatory force than other parts.

The elegance of Mintz's efforts to sail between the Scylla of perceptionism as far as "race" is concerned, and the Charybdis of realism in the economic sphere, becomes the more admirable once the reader observes the risky philosophical seas, or perceives them as such.

I now turn to a consideration of *Marriage, Class and Colour in Nineteenth-Century Cuba*. In what Max Weber considered to be a universal tendency, each group tries to monopolize access to its resources and positions, limiting or denying it to others on the basis of what he called social pretexts, such as religion, "race," social origin, and so forth. Verena Martinez-Alier, in her interesting study, prefers not to consider "race" as such a pretext on an equal footing with, say, religion. Instead of attributing to "race" such a "false permanence," she views it as a symbol behind which more "real" pretexts, such as slavery in 19th-century Cuba or religion in 16th-century Spain, take refuge. One feels tempted to conclude that as long as "real" pretexts keep hiding behind "race," the permanence—however false—of the latter, whether or not perceived as a symbol of a "real" criterion of stratification, seems assured.

If "race" is considered as a symbolic substitute for another, bipolar distinction (Catholicism vs. heresy, slavery vs. freedom), the ensuing socioracial stratification must by definition be depicted as essentially bipolar, too: whites versus blacks. Whatever social gradations may exist between these extremes must perforce derive their ranking from their genealogical distance to either pole. Based on such premises and moving on such a level of abstraction, differences in "racial" stratification and mobility between societies are difficult to grasp and without great significance, to boot: the efforts at monopolization are, indeed, universal, as is the allocation of positions to the different "racial" groupings by the dominant one, the resulting division of labor being as much a "cause" of selective racism (as Martinez-Alier suggests) as, from this perspective, a result.

The author has carefully analyzed some 250 cases in which persons planning to marry "interracially" met with resistance from relatives or

authorities who appealed to the state to impede it, or in which the prospective partners themselves sought direct permission to marry. The latter requests became more abundant after the 1830s; the dissenting appeals mostly date from the first three decades of the century. All cases were based on a decree of 1805 which, in its strictest interpretation (which slowly became the more generally accepted one), made it compulsory to seek dispensation for each marriage considered "interracial" by any interested party, including marriages between "coloreds" of different class or physique. The arguments used in the legal proceedings, admirably dissected by Martinez-Alier, give us a revealing insight into the rhetoric of metropolitans and Cubans, of defenders and critics of the decree, of those who favored increasing "interracial" intermingling and those who opposed it. This last group appears to have grown more vociferous during much of the 19th century, when Cuba's slave population increased and slavery on the sugar plantations became more oppressive and cruel.

Martinez-Alier's cases deal mostly with urban persons rarely from the lowest strata. In Cuba as a whole, the overwhelming majority of marriages in the sociological sense were not legally or religiously sanctioned, due in part to the dispersion of the free population away from the urban centers and the scarcity of priests. The study cannot (and does not pretend to) shed light on the actual frequency of "racially" mixed sexual unions, on either an equal or an unequal footing of the partners, even though some data (e.g., on p. 50) suggest that such a mixing was far from uncommon. Martinez-Alier does devote a separate, lively chapter to elopement and seduction, and in her final section makes some analytical comparisons on the concepts of honor, matrifocality, caste and race, and marriage.

While her theoretical premises lead her to suggest an essential structural likeness in all "racial" systems, she virtually equates North American "passing" with Cuban "improvement of the race" (p. 137). This would seem to show that comparisons on a lower level of abstraction sometimes lead her to neglect differences which others might consider crucial (in actual, as opposed to rhetorical, social reaction—in frequency, in degree of secretiveness, etc.).

Whereas Mintz's work, in my opinion, belongs to the special class of excellent, mature, and finely balanced scholarship, Martinez-Alier's work deserves praise for its meticulous research and its sometimes original theoretical suggestions, the appeal of which to some readers may be enhanced by their sharply cut, even slightly aggressive, formulation, even though their comparative plausibility, on second sight, may not always be as overwhelming.

*Health Insurance Plans: Promise and Performance.* By Robert W. Hetherington, Carl E. Hopkins, and Milton I. Roemer. New York: John Wiley & Sons, 1975. Pp. xii+341. \$13.50.

Charles N. Lebeaux

*Wayne State University*

How to provide access to medical care is a problem not in biology but in social organization. Thus, social scientists and the general citizenry are the experts. In the late 1940s, when national attention was first focused on the problem, only one serious obstacle was perceived—the financial block to service—and we were sure that a national health insurance scheme on the model of Social Security would solve that. But this view has been chastened by 10 years of experience with Medicare, whose most salient result (despite much good work) has been enrichment of health care providers. One computer simulation study predicts that extension of Medicare to the whole population would result in reduction in total medical care, because the providers would be so enriched they would work less. More than a financing system seems to be required. In the absence of government action, several forms of health insurance under voluntary auspices have evolved and have been eyed with much interest as possible vehicles for a national system that would avoid Medicare-type abuses.

This book reports a major study done in 1965–71 by an experienced group of health care researchers at the UCLA School of Public Health, subsidized by USPHS grants. The study compares the characteristics and performance of six unidentified health insurance programs in the Los Angeles area, one large and one small of each of three types: commercial, provider sponsored (e.g., the Blues), and prepaid group (e.g., Kaiser Permanente). Data were obtained from the sponsors, consumers, medical records, and physicians involved in the plans, the uses of services by consumers, quality of care and range of services, family expenses, and consumer opinions. Findings in each of these areas are presented in a complex set of analytic categories.

This study should be credited for tackling an important question on a large scale, for thoughtful conceptual analysis and research design, and for enormous if not always successful industry in getting cooperation from data givers and in accumulating data. And from among the literally thousands of findings there emerge many important points: as previously found, the prepaid group plans generally do give much more service for the money, but—and this is new—not necessarily for their poorer subscribers; the quality of medical care given by the group plans is least liked, apparently because of “bureaucracy”; the “nonprofit” physician- and hospital-sponsored plans come off less well on most counts than the commercial plans—but maybe because the parent insurance companies used the health programs as loss leaders.

There are numerous defects in this study report. The amount of careless or mechanical error is astounding. On page 240 alone, in a discussion of



table 6-1, there are three gross factual misstatements, and there are similar omissions elsewhere. Although mainly written in acceptable English, the book contains eruptions of vacuity that suggest a derailed computer hunting for its program. Could these troubles be due to the fact that "this book has been set in type using a photocomposition computer program. . . . This is the first book of this type. . . . The book was composed for the most part at a computer console" (p. viii)?

Further, how much do we trust data based on a 50% return of a mailed questionnaire, with no follow-up (as far as I could determine)? And what is to be made of an assertion that the overload on the care system in one plan comes from the more affluent and not from the "welfare types" (p. 291) when there is no evidence given that any of the consumers were welfare recipients or otherwise financially dependent? Perhaps the researchers invested too much of their energies in the research procedures and had too little left to interpret their findings properly.

Current sophisticated opinion on health care policy (e.g., Eli Ginsberg, "What Next in Health Policy?" *Science*, vol. 188, no. 20 [January 1975]) has it that sociocultural factors in American life—nutrition, tobacco, drugs, environmental stress—are now the big determinants of national health level, not the medical care delivery system. This is doubtless true, but it would be a big mistake to lose sight of the fact that most people are worried not about their overconsumption of Cokes and cigarettes but about how to get hold of and pay the doctor or how long the waiting line at the HMO (health maintenance organization) clinic is. Studies designed to reduce these problems are still worthwhile. The one under review, despite its defects, should interest students of the subject.

*Health Care Politics: Ideological and Interest Group Barriers to Reform.* By Robert R. Alford. Chicago: University of Chicago Press, 1975. Pp. xiv+294. \$12.50.

Gail Lee Cafferata

*University of Rochester*

*Health Care Politics* joins a long shelf of books written in response to the pluralist contention that the free interaction of private groups is sufficient to produce the good society. Robert Alford shares with pluralists the assumption that private interest groups *do* make the decisions and that they, not the state, are sovereign: "Government is not an independent power standing above and beyond the competing interest groups" (p. 251). Alford diverges from pluralists in his contention that not every group is represented in the political process. By casting his vote for the yardstick of equality rather than freedom, Alford joins that group of political theorists who might well be called the muckrakers of the sixties despite their sophisticated academic attire.

Alford contends that only "dominant structural interests" (more commonly called vested interests) make vital decisions affecting health policy.

These decisions are essentially "conservative" (p. 264) because they are made by two competing groups which both share a preference for the "market economy." These two groups are "professional monopolists," or the medical profession, and "corporate rationalizers," or bureaucrats in the health service industry. Against these stand "repressed interests," including "the white rural and urban poor, ghetto blacks," and others. These are "repressed" because no social institutions or political mechanisms insure that these interests are served" (p. 15). In the concluding chapter we learn why "repressed" interests exist. Alford proposes a "class perspective," by which he means the "dominance of the private sector and the upper middle classes" (p. 254): "The defects in current health care are regarded in this perspective as a disease of the entire social organism which must be diagnosed and treated as a whole" (p. 265). The essential reason for the health care "crisis," then, lies in "fundamental features of the political economy" (p. 167) which will not be remedied without "the emergence of a social movement and political leadership which is not yet visible" (p. 266).

It is unfortunate that Alford tends to play down this theoretical perspective. One must read the entire book to grasp his "class perspective." The author does not choose to relate his study to the "other books on the shelf," and in this sense does not address serious students of political theory.

*Health Care Politics* is not a book about the United States but about New York City. The two data chapters (2 and 3) contain evidence to support two themes: "First, responses to periodic 'crises' take the form of symbolically reassuring investigations, whose reports call for new administrative devices to 'coordinate' and 'integrate' the health care delivery system," and second, "the call for coordination and integration, and the reactive defenses of pluralism and diversity in the market, are part of a battle—sometimes manifest, sometimes latent—of deeply embedded structural interests for control of key health care resources and institutions" (p. xiv). This, then, is a book about ideologies, their uses, and the groups who devise them. Its empirical focus is primarily on the "structural interests" rather than the "equal health advocates" who challenge their power.

Chapter 2, "Commissions of Investigation, 1950–1971," is largely a chronologically organized, chapter by chapter, section by section, summary of eight reports published by selected commissions appointed to improve health services in New York City. (Why the author chose these and not the other 15 reports that appeared during the same time is never made explicit.) In an exegesis woven throughout the text, Alford points to the rhetorical earmarks of an ideology of "corporate rationalization"—"coordinate," "plan," "integrate," "link up," "clarify," "establish," and "designate," simply put, do anything but tamper with the existing system. The author's strategy seems to be to fully discredit the scientific validity of the reports so that the reader will be more easily convinced of their use as an ideological smokescreen to protect the vested interests of the medical

profession and health administrators. He fails to take this constructive analysis of ideology much further than a nonsystematic restatement of "thou shalt." The chapter's strength lies in the author's consistent perception of a set of concepts concerning the assumed rationality and benevolence of free enterprise. A table summarizing his findings would have proved a useful addition. Here, it would seem, is also an ideal situation for a formal computer-assisted content analysis, the author focusing as he does on the persistent appearance of certain key words over a 20-year time span.

It is important to note that Alford never goes beyond the reports themselves in asserting their "symbolically reassuring" use. We never know, for example, who read the final reports, what their reactions were, or whether the reports got "lost" in the city bureaucracy. In fact, there is little indication here about the actual division of authority in the city regarding health activities; yet Alford asserts that "division of authority is *not* the cause of the multifarious problems faced by the New York health system" (p. 101), no doubt because he wants to argue that all the recommendations made in these reports are misdirected and therefore of only "symbolic" significance. Ironically, chapter 3 bears sufficient evidence that the division of authority, if not *the* major problem, is certainly a serious one.

The rich documentary data of chapter 3, "The Planning Council and Coordination of Neighborhood Health Care," offer a case-by-case discussion of the lack of coordination in the planning of New York City neighborhood health centers. The data are based on what must have been a long period of field observation, informal content analysis of documents and files, and interviews. The case of Knickerbocker Hospital, for example, illustrates the point that there are few incentives for private hospitals to extend ambulatory care services through nonadjacent clinic facilities. According to Alford, most private hospitals moved into the federally subsidized neighborhood health center movement solely to support or expand their established domain, often without clear regard for the real needs of the community. While I would tend to agree with the author's assessment of unmet "needs" in the Manhattanville area (or others he discusses), a few tables or charts containing available mortality or morbidity data would have strengthened his argument considerably. Alford tends to assume there is a "crisis," but its specific dimensions are never clearly specified. A map or two would have helped the presentation as well.

The concluding five chapters of the book are an attempt to devise a "theoretical synthesis" of literature concerning "the underlying causes of the diverse phenomenon previously described" (p. 190). One chapter argues that New York City is not a unique research site, and another that the medical profession and health administrators are battling "for control of sources of profits." Chapter 6 provides a sketchy discussion of the ideology of "equal health advocates," and the remaining chapters criticize the "lack of adequate data" and conclude with presentation of the "class perspective." I feel obligated to comment on the appearance in these later chapters

of gratuitous and personally disparaging remarks about the state of health care research and its practitioners. We still do not know all we might about the American health care system. Nonetheless, certain of the author's assessments seem to be based on limited sources. Persons with whom the author disagrees are usually unnamed pronouncers of "smooth rhetoric," while those with whom he agrees are honored with occupational titles (e.g., "law professor Clark Havighurst," "medical sociologist David Mechanic"). The casual way in which these colorful adjectives are used does much to discredit the image of sociology as a scientific endeavor.

In sum, Alford's fine but brief excursion into the actual jurisdictional interactions of private health care sectors with local, state, and federal agencies whets one's appetite for more satisfying fare. It is a useful case study of fragmentation in New York's health services, despite repeated calls for reform. It points to an important problem whose underlying causes are a complex mixture of cultural, political, and economic factors which are difficult to understand, especially because many are unique to our nation. Alford's substantive efforts offer a promising approach to the collection of primary data with which to assess the full dimensions of the urban American health crisis.

*Development of Indices of Access to Medical Care.* By Lu Ann Aday and Ronald Andersen. Ann Arbor, Mich.: Health Administration Press, 1975. Pp. xiii+306. \$7.50.

Richard B. Warnecke

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As the authors state in the "Introductory Notes" (pp. xii-xiii), this volume is based on their final report to the Robert Wood Johnson Foundation of the first year of work on a project titled "Development of an Index of Access to Health Care." Although it contains a conceptual analysis of the term "access" and an extensive review of both the theoretical and empirical work which has employed the concept, the authors do not finally develop an index. Presumably this task will be the subject of a second volume.

The volume is divided into two sections. The first, containing four chapters, is the body of the report. In these chapters the authors deal with the theoretical framework for the concept, provide an analysis of selected indices, review the evaluative methods they employed, and provide an extensive bibliography. The second section contains two appendices (chaps. 5 and 6). The first provides extensive annotations for many of the sources summarized in chapter 4. The second appendix is a detailed description of the data sources available to those wishing to study the empirical bases of the concept "access."

As a reference for those dealing with the delivery of health services, this volume has many virtues. It is well organized and concisely written in a readable and understandable style. Chapters 1 and 2 provide an excellent analysis of the theoretical and empirical bases of the concept

in question. The sources on which this analysis is based are extensively annotated in chapters 4-6, which provide a scholarly history of the use of the concept and should be carefully studied and used by others doing research in this field. In particular, chapter 6 is of great potential value, since it documents the major data sources relevant to empirical research on the concept of "access." Information is provided on the period and frequency of data collection, the population and/or sample studied, the methods of data collection, the types of indices of access employed and a description of each, selected references wherein the data can be examined, and the name and address of the principal investigator or archivist from whom the data can be obtained. To my knowledge there are few compilations as complete or as useful as this one.

The major weakness of this volume lies in the almost complete absence of critical evaluation of the materials presented. The analysis in the first two chapters is primarily descriptive and contains little evaluation of the reliability or validity of the measures examined. Although an attempt at evaluation is made in chapter 2 (pp. 28 ff.), the criteria used are neither well defined nor complete, and the subsequent review of indices based on these criteria is weak. It is unfortunate that the authors do not seem to be aware of the current methodological work which is being done in the field of measurement development and evaluation.

Similarly, chapter 6 would have greatly benefited from an appraisal of the various data sources reviewed there. Potential users of these sources are left to make their own decisions about their possible utility. It would seem that the authors might have been in an excellent position to comment on the strengths and weaknesses of the sources for various types of analyses. In addition, this valuable section would have been greatly enhanced by the inclusion of some information about each source's completeness of data, possible biases, relevance to particular issues, and format.

The book should really be a "volume 1," with the promise of completion of the work in a second volume to follow. However, as a starting point for an investigator interested in the field of access to health services, it is an invaluable resource which can save many tedious hours of bibliographic research. I am sure that it will provide useful materials for lectures and might be useful as a supplemental resource in advanced graduate seminars. Graduate students and faculty alike should find it an important addition to their reference libraries.

*Measurement in the Social Sciences: Theories and Strategies.* Edited by H. M. Blalock, Jr. Chicago: Aldine Publishing Co., 1974. Pp. viii+464. \$16.00 (cloth); \$8.95 (paper).

Kenneth C. Land

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In a general sense, all of social science is concerned with measurement, either of properties of attributes of a class of objects (or events), or

of relations among properties. In the former case, measurement usually is treated as a mapping which associates numbers with the properties of the attributes of objects in such a way that the properties are faithfully represented as numerical properties. The result of this mapping process is a set of mathematical or stochastic variables which define a property-space within which the objects may be represented. The second type of measurement is concerned with the assessment of transformations in this property-space, and is usually associated with a statistical procedure such as regression analysis. Both types of measurements are represented in this collection of 14 essays edited, and with an introductory statement (chap. 1), by H. M. Blalock, Jr. Each of the essays merits some description and comment.

The first part of the book contains five chapters devoted to alternative data collection approaches to the measurement of properties of individuals and groups. Leading off in chapter 2 is a discussion by John Frederick Long and Paul H. Wilken of Clyde Coomb's nonmetric unfolding technique for deriving ordered metric scales from ordinal preferential choice data. By use of simulated data, they provide some evidence that the unfolding methods are robust under response errors of up to 10%.

The next two chapters are closely related and raise a number of issues. In chapter 3, Robert L. Hamblin surveys the literature on applications to the measurement of social attitudes of the magnitude estimation procedures originally developed by S. S. Stevens and his associates in the context of psychophysical measurement. On the basis of a variety of experiments, Stevens stated in 1957 a power law for stimulus-response relationships in man's sensory system,  $\psi = c\phi^n$ , where  $\psi$  is the magnitude of the sensory response,  $\phi$  is the magnitude of the related physical stimulus, and  $c$  and  $n$  are empirical parameters. During the past 10 years, Hamblin and a number of other social scientists have applied magnitude estimation procedures to the scaling of individual responses toward such objects as aggression, status, power, seriousness, dislike of self, wages, and poverty. On the basis of these investigations, Hamblin (p. 114) generalizes Stevens's law to describe the relationship between the magnitude of an attitude ( $A$ ) and the magnitude of its related social stimulus ( $\Sigma$ ), as in the equation  $A = c\Sigma^n$ . In chapter 4, Allen M. Shinn, Jr., presents a related discussion of some of the psychometric literature dealing with the functional relationship between magnitude estimation and category scales. Among other things, he provides a derivation of the logarithmic function for transforming magnitude scale estimates into category scale values, which means that the simple category scale is nonlinear relative to perceived magnitude.

On the whole, I believe the Hamblin and Shinn papers provide a useful summary of what will undoubtedly become an important method for scaling various social stimuli. However, lest the fervor of the Hamblin and Shinn discussions lead the reader into thinking that magnitude estimation provides a definitive solution to the problem of attitude scaling, it is worth emphasizing a few caveats. First, as Hamblin (p. 114) notes, his attitude law should be expected to apply only when both the attitudinal response

and the social-stimulus variable are measured quantitatively (e.g., when "disliking for self" is scaled relative to various numbers of enemy killed during wartime). Suffice it to say that such variables are quite different from the usual attitudinal context (e.g., affective reactions to the United Nations as an organization).

Second, there seems to be some confusion in the measurement literature about the type of scale (interval or ratio) which results from the application of magnitude estimation procedures. On the one hand, those who work with magnitude estimation methods (e.g., Shinn, p. 124) claim that they produce ratio scales. On the other hand, David H. Krantz, R. Duncan Luce, Patrick Suppes, and Amos Tversky have argued in their study of measurement theory that Stevens's technique leads to an interval scale (*Foundations of Measurement* [New York: Academic Press, 1971], p. 519, n. 9). In the discussion of his experiments, Hamblin (p. 83) points out that the origins of the stimulus and response scales must be "adjusted," or, alternatively, the origin can be fixed arbitrarily in the instructions to the respondent (p. 71). This would seem to provide evidence for the Krantz et al. position.

Third, some comment is in order regarding the very high correlations for Hamblin's attitude law, which, when averaged over a sample of respondents, are on the order of .98-.99 between the attitudinal response and the social stimulus. Given the first comment above, a magnitude estimation scale can be regarded as somewhat analogous to a standard multi-item scale or test in which the underlying "trait" or "continuum" is indexed by a quantitatively measured stimulus. Thus, Hamblin's correlations are somewhat analogous to reliability coefficients in psychometric theory and, as such, should be very high. In brief, with stimuli of this kind, one finds substantial average "interitem" correlations of, say, .5-.6. With 20 "items," one then gets a reliability coefficient estimate of about .98 by applying the standard formula (see, e.g., J. C. Nunnally, *Psychometric Theory* [New York: McGraw-Hill, 1967], p. 193). In brief, it is reasonable to conclude that Hamblin's correlation coefficients show that magnitude estimation procedures produce highly reliable scales of social attitudes.

Finally, something should be said about the power function form of the Stevens and Hamblin laws. It is certainly true that power functions are versatile and possess many nice mathematical properties. They ought to be considered prime candidates for psychophysical laws for these reasons alone. However, it should also be noted that other standard "item characteristic curves" of scaling theory, such as the normal ogive and the logistic, would provide approximately the same graduations to Hamblin's data as the power function. Moreover, these alternative functions could account for the zero levels of the median estimated reactions for some values in the extreme ranges of some of Hamblin's data (p. 80) which cannot be readily fit by a power function. Perhaps it would be appropriate to evaluate the empirical graduations produced by power functions relative to those produced by alternative models. In summary, I should reiterate that these comments are merely meant to provide a context within which to

interpret what I consider to be important contributions by Hamblin and Shinn.

Two additional chapters complete the first section of the volume. In chapter 5, Roy D'Andrade compares results from the direct observation of small groups with memory records from participants to show how memory may distort responses in a systematic way. Chapter 6 is a summary of work by Paul W. Holland and Samuel Leinhardt on how an inferred interpersonal sentiment structure depends on the way in which questions are asked, in particular, on the nature of fixed-choice questions.

The topic of the second part of the book is multiple indicator approaches. It contains four chapters on the incorporation of multiple indicators (of usually latent, unobserved variables that inherently contain significant random measurement error) into structural equation models. In chapter 7, Alvin L. Jacobson and N. M. Lulu give a simulated data analysis of estimates of a structural parameter by single indicators, indexes, and multiple indicators under varying structural and measurement conditions. Chapter 8 is a discussion by John L. Sullivan of criteria for selection of multiple indicators. In chapter 9, C. E. Werts, R. L. Linn, and K. G. Jöreskog summarize much of the recent literature (to which they have made substantial contributions) on models which incorporate causes and effects of latent variables. Michael T. Hannan, Richard Rubinson, and Jean Tuttle Warren discuss, in chapter 10, the complications of inferences from multi-wave, multivariable panel studies in the presence of multiple indicators of latent variables.

The third and final section of the book contains five chapters on ordinal measurement. The general concern of these chapters is the statistical analysis of data in the form of ordinal scales. This has been a topic of heightened interest in the past few years, and these chapters make some useful contributions to the literature on ordinal statistics. First, in chapter 11 is Thomas P. Wilson's discussion of the problem of choosing a statistic for a bivariate ordinal hypothesis from among the large set of available measures. In this discussion he introduces a new variation on the existing statistics. Chapter 12, by Donald R. Ploch, and chapter 13, by Dana Quade, attempt to elucidate the foundation of ordinal measures in theoretical statistics. Ploch gives a treatment of ordinal statistics in the context of the general linear model, and Quade treats notions of partial correlation among ordinal variables. In chapter 14, H. T. Reynolds argues that causal models can be constructed and analyzed with ordinal data and compares ordinal correlation with interval procedures. The final chapter of the book, chapter 15, is an analysis by Blalock of how ordinal statistics can be used to provide weak tests of the hypothesis that the data satisfy the interval measurement and distributional assumption of regression and related statistical models.

In summary, the essays in this volume address a variety of measurement problems in the social sciences. There is a good representation of authors and examples from the disciplines of anthropology, political science, and sociology. The volume would be useful reading for the individual social



scientist interested in some current approaches to measurement in these disciplines. It could also be fruitfully used as supplementary reading in a graduate course either on measurement or on structural equation models in the social sciences.

*Ferdinand Tönnies: A New Evaluation: Essays and Documents.* Edited and with an introduction by Werner J. Cahnman. Leiden: E. J. Brill, 1973. Pp. viii+302. \$23.40.

G. H. Müller

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This book most fortunately complements W. Cahnman and R. Heberle's edition of selected writings, *Ferdinand Tönnies on Sociology: Pure, Applied, and Empirical* (Chicago: University of Chicago Press, 1971). Together with E. G. Jacoby's recent translation of additional selections from Tönnies, *On Social Ideas and Ideologies* (New York: Harper & Row, 1974), the present volume has been instrumental in generating a new interest in Tönnies which may not yet have peaked.

As Heberle points out in the foreword, Cahnman has been indefatigable in his editorial work. His are also some of the major contributions in the volume. Not the least of these is the translation of and commentary on a number of sources which shed light on Tönnies's relation to Spencer, Marx, Durkheim, and Weber. Notable also is "A Research Note on Phenomenology and Symbolic Interactionism." These "Documents and Comments" form the second part of this book.

"Evaluation" constitutes the first and main part, combining three groups of writings. First, there is a series of evaluations extending from Albert Salomon's obituary on Tönnies (1936) to Parsons's "Note on *Gemeinschaft und Gesellschaft*" (1937) and "Some Afterthoughts on *Gemeinschaft und Gesellschaft*" (1973). Second, we are given Cahnman's and Jacoby's attempts at a new evaluation of Tönnies's work. Third, there is a reconsideration of Tönnies in the light of contemporary developments, with A. Oberschall scrutinizing Tönnies's empirical work, P. Etzkorn contrasting Tönnies with Simmel as a formal sociologist, and G. Lindt Gollin and A. Gollin elaborating on Tönnies's later (1922) work on public opinion.

As Cahnman rightly states, Salomon's obituary has lost nothing of its validity. Salomon hailed *Gemeinschaft und Gesellschaft* (1887) as "the first attempt to separate the trans-historical and historical elements within the social structure" (p. 34). While he paid tribute to the idea of natural law as "the greatest attempt to break through the relativism of the historical world and to establish a system of general concepts of social relations" (p. 41), he was no less aware of the "genuine romantic" strain that informs and pervades Tönnies's conception of *Gemeinschaft*. In fact, as Heberle points out (p. 50), "the significance of *Gemeinschaft und Gesellschaft* for the history of ideas lies in the combination of the ratio-

nalistic and the organicistic view of human society and in the elaboration of two opposite styles of life as possible forms of social reality."

Cahnman makes a virtue out of the dualism and extols the work of Tönnies as preeminently dialectical (p. 122). *Gemeinschaft* and *Gesellschaft*, while to be kept distinct in thought, "are to be considered as co-existing and co-effective as well as contradictory and opposing tendencies" (p. 109). Far from considering it contradictory and eclectic (as did R. Koenig in his scathing criticism of 1955), Cahnman celebrates Tönnies's work as *coincidentia oppositorum*, a brilliant feat of dialectics superior to any positivist critique. The underlying assumption in Cahnman's new evaluation is that *Gemeinschaft* and *Gesellschaft* are transhistoric concepts which are universally valid and, consequently, not historical sequences. Yet this interpretation is at odds with that of Jacoby: "The 'developmental' interpretation of *Gemeinschaft* and *Gesellschaft* . . . to which the study of Tönnies' sociology has reverted again and again is . . . borne out by his own comments" (p. 80). In other words, if Jacoby's "developmental" thesis is correct, Cahnman's "dialectical" thesis must collapse, and vice versa.

The developmental thesis is supported by P. Etzkorn's analysis of "Tönnies as a Formal Sociologist" (pp. 125-39): "Where Simmel deals with analytic forms, Tönnies uses type constructs" (p. 134); "to classify Tönnies along with Simmel and von Wiese as a formal sociologist is an interpretation which . . . is misleading . . ." (p. 138). Still, Cahnman's case is not lost. For while *Gemeinschaft* and *Gesellschaft* cannot be claimed to be pure concepts, to a large extent "essential will" and "arbitrary will" can be. The two types of will do indeed approximate analytical purity, whatever the ambiguity of *Gemeinschaft* and *Gesellschaft* as empirical-historical (i.e., "developmental," "real," or "construct") types. In the case of "essential will" (however awkward the term), the means are subordinated to an encompassing and absorbing goal (which accounts for the "unity" permeating *Gemeinschaft*), whereas "arbitrary will" subordinates the end to the means. The first denotes *Wertrationalität*, the second *Zweckrationalität*; the one encompasses Rousseau and Schopenhauer, the other Hobbes and Marx. At the same time, both forms of will are inseparable and mutually irreducible. Hence they constitute a dialectic—and Cahnman triumphs.

This thesis is felicitously corroborated by T. Parsons in his "Note on *Gemeinschaft* and *Gesellschaft*" (pp. 140-50). While *Gemeinschaft* is marked by diffuse solidarity, by permanent and deep-seated attitudes, sentiments, disinterested devotion, and value attitudes (p. 143), *Gesellschaft* is broadly equivalent to the rational pursuit of individual self-interest, specific limited-purpose exchange, domination, and institutional rules (pp. 141-42). Significantly, Parsons's interpretation is informed by a shift from historical to transhistorical categories. In fact, Parsons conceives of attitudes as another aspect of the social system besides the institutional sphere (p. 140). We have only to substitute *Gemeinschaft* for attitudes and *Gesellschaft* for the institutional sphere to see that Parsons

is almost in complete tune with Cahnman: the social system may indeed be construed as the dialectical product, albeit not of *Gemeinschaft* and *Gesellschaft* but of (diffuse) attitudes and (specific) institutions. The two are indispensable for the social system and mutually irreducible.

Still, it should be noted that this thesis is Parsons's reinterpretation, not Tönnies's original idea. Not even in his later work, notably the *Introduction to Sociology* (1931), which aimed more specifically at pure sociology, did he fully break away from his original dichotomy and its developmental overtones. The perceptive article by the Gollins, "Tönnies on Public Opinion," also substantiates this: Tönnies's provocative ideas "were not sufficiently interrelated or fully incorporated within a more general conceptual system. . . . [In fact,] Tönnies' work constitutes only a . . . first step towards the development of (pure) theory" (p. 201).

FOR A BETTER PERSPECTIVE  
ON THE PROBLEMS OF INDIVIDUALS  
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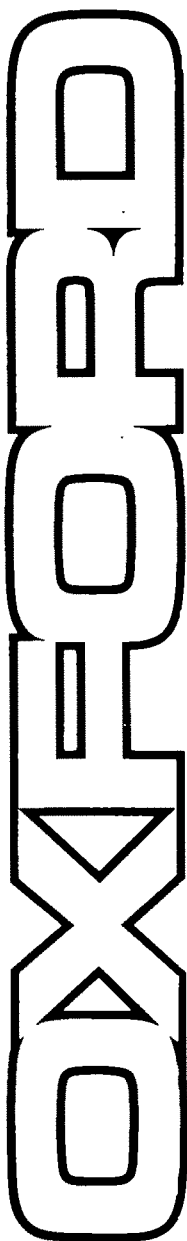
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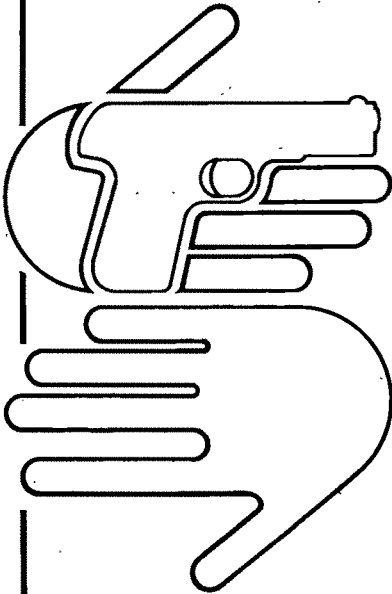
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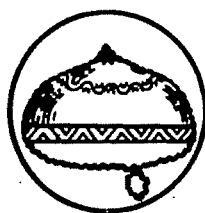


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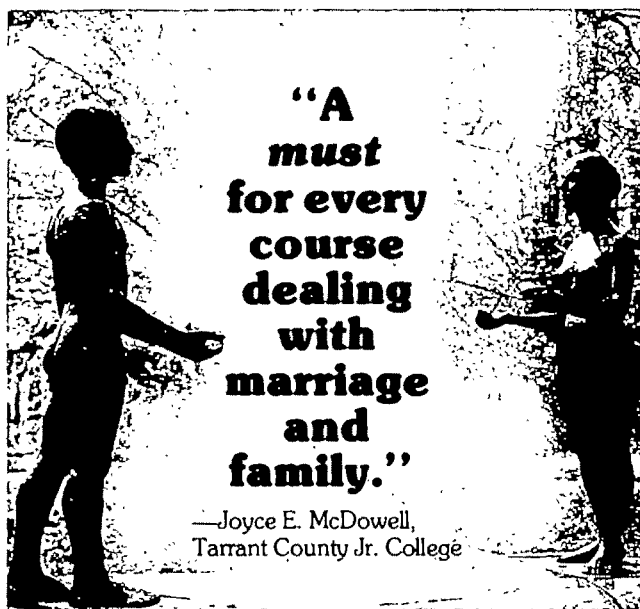
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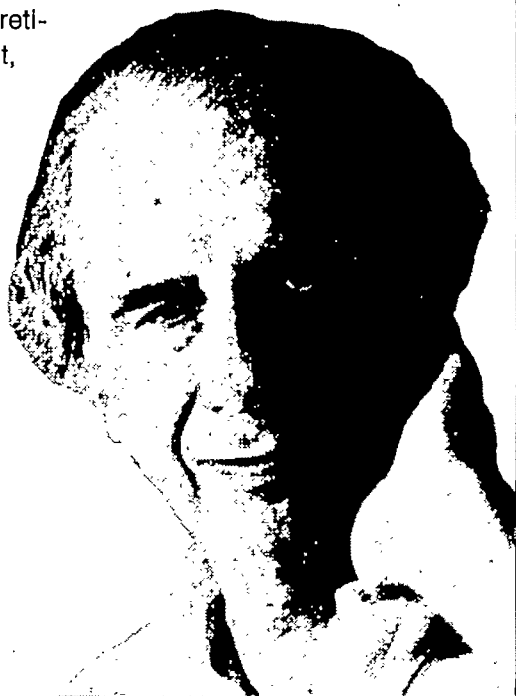
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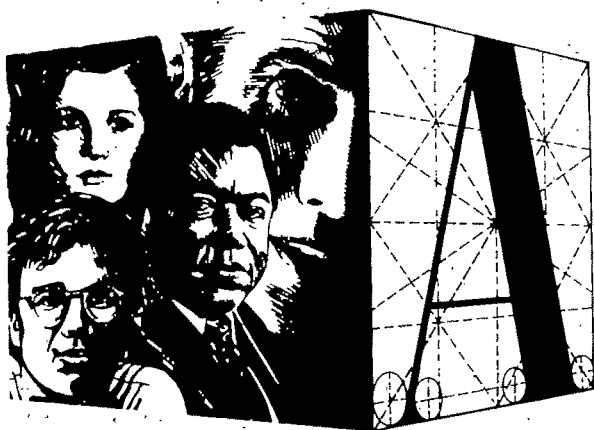
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We are looking especially for presentations of original theoretical arguments, descriptions and illustrations of important new methods, and critical syntheses and evaluations of the state of knowledge or method in major areas of the discipline. We also would like to publish papers that point up important connections between theory and method in sociology and other disciplines.

We will do our best to insure that these papers receive prompt, trenchant, and appropriate reviews.

## IN THIS ISSUE

CARMI SCHOOLER is a social psychologist in the Laboratory of Socio-environmental Studies of the National Institute of Mental Health. His areas of interest are the experimental investigation of the psychology and psychophysiology of schizophrenia, the examination of psychological consequences of occupational experience, and the study of the structural and cultural antecedents of both normal and abnormal adult functioning.

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RICHARD SENTER recently completed a Ph.D. at the University of Michigan; he is an assistant professor of sociology at Central Michigan University. His interests include the sociology of science and the sociology of organizations. His current work involves analyzing networks of scientists in a large atomic research laboratory.

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# Serfdom's Legacy: An Ethnic Continuum<sup>1</sup>

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The effects of ethnicity appear to occur along a historically determined continuum which reflects the social, legal, economic, and occupational conditions of the European countries from which American ethnic groups emigrated. Ethnic groups with a recent history of serfdom show the intellectual inflexibility, authoritarianism, and pragmatic legalistic morality previously found characteristic of American men working under occupational conditions limiting the individual's opportunity for self-direction. Although it is impossible to confirm each link in the causal chain, a model emphasizing the effects on ethnic groups' culture of historical conditions restricting the individual's autonomy seems a probable and parsimonious explanation of contemporary ethnic differences.

This paper evaluates the effects of ethnicity on the attitudes and values of a representative sample of American men as revealed by their responses to a survey conducted in 1964 (Kohn and Schooler 1969, 1973). It asks whether ethnic groups are essentially unorderable or can be conceptualized as existing along a meaningful continuum which reflects social-structural conditions known to affect individual functioning. In doing so it answers a series of more general questions: Does ethnicity have an effect? If so, which behaviors and attitudes are affected, and are its effects independent of those of other social-background variables?

The problem of the independence of ethnic effects is as central to cross-cultural research as it is to the study of ethnicity in America. Cross-cultural researchers have long debated whether cross-national variations in behavior are the results of differences in socioeconomically determined social structures or in historically determined cultural patterns. Viewing this problem, the anthropologist Caudill wrote:

Middle-class managerial personnel in England and France may have more in common than either group has with working-class machine operators in their own country. At the same time, however, I do not think that anyone would say that such Englishmen and Frenchmen are indistinguishable in their approach to work, politics, family life, or sexual activity. They are different in those historically derived and culturally patterned ways of thinking, feeling, and behaving that are passed on, often unknowingly, from one generation to the next. . . . I believe that each of these dimen-

<sup>1</sup>I would like to thank Mimi Silberman for her help in performing the statistical analyses, Pearl Slafkes for her editorial help, and Melvin Kohn and Nina Schooler for their critical readings.

sions—position in modern social structure, and continuity of historical culture—exerts a relatively independent influence on human behavior, and that both dimensions need to be considered simultaneously in the investigation of the psychological characteristics of a people. [1973, p. 241]

This paper postulates an interdependent relationship between these two dimensions, envisioning an interaction over time between culture and socioeconomic structure which affects both an individual's functioning and his values. Under such a model, historical circumstances affect a society's social structure which in turn changes its cultural values. The resultant values, however, may continue to be transmitted long after the original historical and social circumstances have ceased to exist.

The general hypothesis is that Americans belonging to ethnic groups which originated in countries whose mores have been influenced by a relatively recent history of feudal subjugation are different in their intellectual functioning, attitudes toward authority, and moral value systems from Americans belonging to ethnic groups coming from European countries where the institutions of feudal serfdom were never fully established or ceased to exist in the more distant past.

In line with this hypothesis, the major European ethnic groups can be thought of as falling along a continuum which reflects the time of the release of the peasantry from serfdom in the old country (see table 1). Such an ordering of ethnic groups can be seen as operating on two levels. On the level of index construction, it orders countries objectively in a manner which insures that findings are not merely the results of arbitrary weighting procedures which capitalize on chance. On a more substantive level, there is, as we shall see, considerable evidence that such an index ranks countries according to the degree of self-direction and the number of behavioral options which were available to the agricultural sector of the population, the very segment from which most immigrants to America came (Handlin 1951; Kennedy 1964).

Although the ethnic continuum is ordered according to the time serfdom ended, it actually reflects both the degree and recency of a whole series of parallel and interrelated legal, economic, and occupational constraints on the lives of the mass of preindustrial agricultural workers. Of course, since this complex web of interrelated constraints on the individual's independence occurred in the diverse contexts of different national histories, the present ranking—based as it is on the different times of the ending of legal serfdom—does not reflect perfectly the exact chronology and severity of the other components of this web of constraints. However, such a ranking does seem to provide us with a reasonable and nonarbitrary way of ordering European countries according to the general magnitude and recency of constraints placed upon the autonomy of the individual peasant.

It is, of course, impossible to confirm each link in the causal chain lead-

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TABLE 1  
ETHNIC CONTINUUM

AREA OF ORIGIN	END OF SERFDOM	
	Date	Circumstances
Scandinavia .....	...	Never established in Sweden and Norway ( <i>Encyclopedia Britannica</i> ; Gwatkin and Whitney 1957).
England .....	1603-25	In the reign of James I: "With the Tudors, serfdom disappeared in England" (Smith 1922, p. 7660).
Ireland .....	...	Because of the hypothesized importance of a tradition of autonomy and personal responsibility, Ireland, which had been a dependency under the tight control of England, is given a place in the continuum directly below England.
German states .....	1807-33	Changes that abolished serfdom were initiated before 1815 (Crawley 1965, p. 52), the Emancipation Edict of October 9, 1807, being of central importance. The last principality abolished serfdom in 1833 (Smith 1922, p. 7661).
South and central Italy .....	1848	As part of the abolition of serfdom in the Austro-Hungarian Empire (Bury 1964, pp. 413-14).
Eastern Europe (greater Russia, Poland, etc.) .....	1861	Alexander II abolished serfdom by imperial ukase (Bury 1964, p. 372).

ing from an ethnic group's past historical circumstances to the present functioning of its descendants. However, I hope to demonstrate that such a model, emphasizing as it does the effects on different American ethnic groups' cultural values of historical conditions which restricted individual autonomy, represents a parsimonious and compelling explanation of present ethnic differences.

Of the restrictions on personal autonomy reflected by the continuum, most straightforward are the legal constraints of serfdom. Despite all the local variations,

There was . . . one feature common to European serfdom wherever it existed. A peasant was recognized as unfree if he was bound to the will of his lord by ties that were degrading and socially incapacitating and that were recognized as a fundamental part of the legal and social structure of the land rather than the result of an agreement or contract between lord and peasant. In practice this meant that the lord had legal jurisdiction over his peasant to the complete or nearly complete exclusion of the state, so that to all intents and purposes the only rights the peasants had were those the lord was willing to allow him [*sic*]. [Blum 1960, p. 12]

Thus, for instance, according to a medieval English canon lawyer, "If they be serfs, I say that they are bound to pay the tallages [a form of tax the



lord could arbitrarily impose on his serfs] newly imposed on them, even those that tend to profit their lords alone, for serfs and their possessions are the property of their lords" (Bennett 1938, p. 138). In fact, "... This lack of protection from the arbitrary power of the lord was seen as part of the definition of serfdom. Hence Bracton's famous definition: 'For that is an absolute villainage, from which an uncertain and indeterminate service is rendered, where it cannot be known in the evening what service is to be rendered in the morning' " (ibid., p. 99). But even such a paucity of legal rights as described by these English examples of the legal defenselessness of serfs against the power and arbitrariness of their lords pales before the conditions of serfdom in Russia. "The only essential differences between the Russian serf and the American Negro slave that a contemporary apologist for serfdom could think of were that the serf had the privilege of taking the oath of allegiance to the Tsar, paying a personal tax, and serving in the army" (Blum 1961, pp. 468-69).<sup>2</sup>

The rankings of the continuum seem also to mirror the degree of economic independence of the peasantry. Those countries relatively free from a recent history of serfdom seem to have been characterized by a low proportion of large estates and a high proportion of moderate- and small-scale farmers who owned or controlled farms large enough to give them some measure of economic independence (Blum 1960).<sup>3</sup> There is some indication that the rankings also parallel the relative historical availability of commercial shipping and fishing industries as alternate sources of livelihood in the preindustrial period (Lewis 1958; Slicher Van Bath 1963). Obviously such ready availability of an alternative to serfdom would also loosen the peasants' bond to the land.

Historical evidence suggests further that, in those countries where serfdom was more remote, there may have been a tendency toward a greater degree of livestock farming, a form of farming in which the individual had a relatively high degree of independence and decision making (Wallerstein 1974). "Livestock farming is capital-intensive and labour-extensive; it forms, in this respect, a striking contrast to the business of arable farming. The stock-farmer has to think continually in terms of money and to have

<sup>2</sup> There is some evidence that the continuum reflects not only the legal but also the political rights of the peasantry. "No matter what the form of the government . . . , most peasants had no formal organized way of expressing their political, social, and economic demands or of influencing the decision of their rulers at the national or provincial level. . . . Exceptions to this general rule were Norway, Sweden . . . and to a much lesser extent . . . England . . ." (Blum 1960, p. 119).

<sup>3</sup> Similarly, Wallerstein (1974) comments, "The yeoman farmers were drawn . . . even more narrowly, principally from northwest Europe" (p. 87). A more extensive discussion of the relatively high prevalence of small independent farmers in England and the Dutch republic can be found on pp. 239-50, and the relatively greater power of the English yeoman than that of his German counterpart is noted on p. 177.

money at his disposal. Livestock breeding and trading require a good head for cost and sale prices, for which a strong business sense is essential" (Slicher Van Bath 1963, p. 130).

The continuum also reflects other differences in the nature of the work performed by agricultural workers in the various countries. Thus, historical evidence indicates that the ethnic ranking generally follows the chronology of the introduction of the "new agronomy." This system, customarily viewed as the start of modern agriculture, involved the introduction of the four-course crop rotation system, the emphasis on fodder crops, the disappearance of fallow years, and the consolidation of the individual farmer's fields. Once accepted, it affected both the motivation and the technical complexity of the farmer's work. This new system permitted him to take advantage of technologically more complex modern farming techniques, decreased his interdependence with his fellows, and, by increasing his control over his own land, served to raise his feelings of individualism (Slicher Van Bath 1963; Blum 1960; Heckscher 1954; Bloch 1967).

Little freedom of action was left to the individual in the traditional three-course strip-plot fallow rotation system which had predominated prior to the new agronomy, particularly where serfdom was prevalent. Under that system, each farmer's land consisted of a series of noncontiguous strips interspersed with those of his lord and his fellow villagers.

The strip system required them [the serfs] to do all of their field work communally. That is, they all had to perform the same agricultural operations at the same time. To mention just two difficulties that . . . individualistic practices would have occasioned—crops would have matured at different times, and in getting in and out to tend one strip damage would have been done to the still unripe crops in the neighboring strips. Secondly, after the crops were in, the field was often used as a pasture for villagers' livestock. This could not have been done if the crops in the field had different harvesting times. Communal farming was the only practicable method. This necessitated constant agreement and close co-operation among all the peasants and some form of central control. . . . [Blum 1960, p. 15]

"On the common fields, the whole community acted in common. The animals . . . were driven away on a certain day . . . determined by immemorial custom, then there followed the ploughing and sowing of the crop for the year . . . and in all this there was but little room for individual choice. The peasant was chained down to a routine that was, seemingly, unbreakable" (Bennett, p. 51).

Nor was the serf's economic relationship with his master one which would encourage him to exercise his innovativeness and ingenuity in an effort to increase his yield (Slicher Van Bath 1963; Bloch 1967; Blum 1960). When working on his lord's fields, the serf had even less freedom, being constantly under the supervision of the lord's overseers. The medi-

eval agricultural manual of Walter of Henley warns: "Let the bailiff . . . be all the time with the ploughmen, to see that they do their work well and thoroughly and at the end of the day see how much they have done . . . to guard against their fraud" (Bennett 1938, p. 113). And even non-field-related work required little or no skill. Here too the serf provided only physical labor, carrying out his assigned tasks under the close supervision of master craftsmen (Bennett 1938). Finally, even "when the peasant had performed all the ploughings . . . demanded of him he was still under many obligations—his lack of freedom showed itself in many ways—he could neither brew nor bake where he would . . . grind his own corn, sell his beasts, give his daughter in marriage, nor do many other things without the lord's permission . . . the lord's power was about him on all sides . . ." (Bennett, p. 129). In sum, there is ample reason to believe that, in terms of legal rights, economic freedom, and occupational conditions, the ethnic continuum reflects both the opportunity for autonomy and the complexity of decision making open to the peasantry in different countries.

This description of the interrelated legal, economic, and occupational correlates of the ethnic continuum is in almost complete accord with Wallerstein's account of the geographical division of agricultural labor which took place in Europe between 1450 and 1640, the period of the development of capitalistic agriculture. According to Wallerstein (1974), this division of labor is best understood in terms of three geographical areas: the core (centered in northwestern Europe and Germany west of the Elbe River), the semiperiphery (southern Europe), and the periphery (Europe east of the Elbe).

During this period the core areas were ". . . in the process of dividing the use of . . . land for pastoral and arable products. This was only possible as the widening market created an ever larger market for the pastoral products, and as the periphery of the world-economy provided cereal supplements for the core areas. The semiperiphery was turning away from industry . . . and toward relative self-sufficiency in agriculture" (p. 116). The "world-economy was based precisely on the assumption that there were in fact these three zones and that they did, in fact, have different modes of labor control." These modes can be seen as related to "product and productivity. . . . Slaves [used at that time for sugar cultivation] . . . are not useful in large-scale enterprises whenever skill is required. Slaves cannot be expected to do more than what they are forced to do. Once skill is involved, it is more economical to find alternative methods of labor control. . . . This resulted in a form of serfdom wherein the peasants are required by some legal processes enforced by the state to labor . . . on a large domain producing some product for sale on the world market" (pp. 87–91).

"... The semiperiphery represents a midway point on a continuum running from the core to the periphery... in labor control [it exhibits] an in-between form, share-cropping" (pp. 102-3). "... In the core, the situation was different in a number of respects. ... Agriculture was more intensive. ... More intensive agriculture requires better terms for the peasant. In addition, part of the land was shifted from arable to pastoral use. The result was less coercion. ... The coercion was to be more indirect via marketing mechanisms." Therefore, "The occupational range of tasks in the core areas was a very complex one; the trend in the core was towards variety and specialization" (pp. 100-102).

The application of Wallerstein's categorization to the countries in the ethnic continuum (table 1) results in essentially the same ranking as that of the continuum. Thus, according to Wallerstein, Sweden—though economically not highly developed—followed the model of the core countries, not only because the weather was not conducive to specialization in the growth of cereal grains, but also because of "a peasantry that was strong and well organized" (p. 312). Ireland, though in a semicolonial relationship to England, "became integrated into the British division of labor" (p. 281), a fair amount of her land being devoted to the raising of sheep for wool production. However poor the condition of the Irish peasantry, their subjugation to their masters rarely reached the level found in Europe east of the Elbe River. From this perspective, the homeland identified as Germany by our respondents has two quite different parts: a core area west of the Elbe and a peripheral one east of it, so that taken as a whole it ranks below Ireland in terms of the degree of autonomy and level of skill of its agricultural workers. Italy, being in the semiperiphery, ranks below Germany, while modern Poland and Russia, in the periphery, would lie at the bottom of the scale.

Thus, Wallerstein's historiographic analysis of the geographic distribution on the European continent of various forms of agricultural production and their concomitant methods of labor control strongly supports the historical accuracy of my hypothesis that the ethnic continuum does in fact rank countries according to the complexity of occupational tasks and the degree of self-direction characterizing the agricultural sectors of the population.

The social conditions reflected by the continuum can be hypothesized as having affected the peasant cultures of the various ethnic homelands in ways which would act upon intellectual functioning, attitudes toward authority, and moral values. In terms of intellectual functioning, earlier research indicates that intellectual performance is improved by exposure to relatively complex environments in which the individual is called upon to make decisions on the basis of a wide variety of factors (Schooler 1972) and that occupations characterized by closeness of supervision, routiniza-

tion, or substantively simple tasks decrease both the intellectual flexibility of those working in them and their intellectually demanding leisure activities (Kohn and Schooler 1969, 1973). Since, as we have seen, the work done by serfs was generally highly routinized, closely supervised, and substantively simple and the number of options available to them in the other areas of their existence severely limited, it would appear that a culture developed under such social and occupational conditions would not emphasize the development of intellectual capacities and interests.

Similarly, persons living and working under such restrictive conditions seem to develop essentially authoritarian, conservative, and conformist attitudes toward authority (Kohn and Schooler 1969, 1973; Schooler 1972). Accordingly, it might be expected that individuals belonging to ethnic groups from cultures long affected by the institutions of serfdom would hold such values for themselves and their children.

Finally, members of ethnic groups from countries where the patterns of relationships and responsibilities among individuals have a long history of being prescribed by the formal rules and regulations of feudal serfdom might be expected to value personally responsible moral autonomy less than members of groups from countries where the interrelationships among individuals were relatively free and unstructured. As used here, "moral autonomy" means that the individual holds himself responsible for maintaining and living up to an internalized set of moral standards, instead of limiting his moral obligations to stay out of trouble by conforming to rules laid down by an external authority. Taken together, the foregoing hypotheses suggest that the differences found among European ethnic groups in present-day America result from cultural values which are the residue of historical processes whose major determinants occurred on another continent and in other centuries.

#### METHOD

The data used in this paper were collected as part of a study conducted by Kohn and Schooler (1969) on the social-psychological consequences of occupational experience. Interviews with a sample of 3,101 men, representative of all men throughout the United States employed in civilian occupations, were conducted by the National Opinion Research Center in the spring and summer of 1964 (see Sudman and Feldman [1965] for a general description of the sampling methods). About half the interview questions referred to job, occupation, and career; the remainder to the background information, values, and orientations with which this paper deals. Of the men originally chosen for the study, 76% gave reasonably complete interviews, the median interview taking two and one-half hours.

Although the original sample is representative of all men employed in

civilian occupations in 1963-64, the current analysis excludes Jews and is also limited to whites who either were born in Europe or had a parent or grandparent born there. Such a nationality-based definition of ethnicity is somewhat more stringent than that frequently used, in that it limits the potential effects of race and religion. However, even though such limitations might tend to lessen the number of significant findings, the results that do occur can be ascribed with reasonable confidence to national origin, rather than to discrimination because of religion or skin color.

Of the various psychological variables measured in the Kohn and Schooler study, seven were chosen as dependent variables for the analyses reported on here.<sup>4</sup> Two of them are directly related to intellectual functioning:

1. Intellectual flexibility, as evidenced by performance in handling cognitive and perceptual problems.
2. Intellectually demanding use of leisure time.

Two others can be seen as measuring attitudes toward authority:

3. Authoritarian conservatism: rigid conformance to the dictates of authority and intolerance of nonconformity as opposed to open-mindedness.
4. Self-direction as opposed to conformity to external authority as a parental value.

Three can be seen as related to moral autonomy:

5. Personally responsible morality, the most central of the three, measuring a continuum of moral positions from defining, maintaining, and holding oneself responsible for one's own moral standards to believing that morality consists of strict adherence to the letter of the law and keeping out of trouble.
6. Attribution of responsibility to self for control over one's own fate instead of feeling controlled by outside forces.
7. Self-deprecation, the self-critical part of self-esteem: the degree to which men are willing to criticize themselves for not living up to their own standards.

Given the dispute over the extent to which ethnic differences merely reflect social-structural variables, the latter should be controlled when ethnic differences are examined. The results of earlier analyses of the present body of data suggest that in the United States the antecedent background characteristics of being young, having a well-educated father, being reared in a nonfundamentalist religion, and being raised in an urban

<sup>4</sup> See Appendix for information concerning these seven variables.

environment or in an area far from the South are indicative of growing up in a relatively complex and multifaceted environment. They suggest further that growing up in such an environment results in a relatively high level of intellectual functioning, a rejection of external constraints, and a belief that what takes place within oneself is of primary importance (Schooler 1972). Since these psychological characteristics are obviously related to the dependent variables under consideration, their antecedents must be controlled in order to insure that the findings are not spurious. The basic form of analysis employed is multiple regression, in which the linearized ethnic variable is used as an independent predictor variable in multiple-regression equations predicting the various dependent variables. The effects of the relevant background variables are controlled by considering only those ethnic effects for which the beta ( $\beta$ ) weight is significant when these background variables are also included as predictors in the multiple-regression equation.<sup>5</sup> Under such conditions, the  $\beta$  weight for a given independent variable indicates "how much change in the dependent variable is produced by a standardized change in one of the independent variables when others are controlled" (Blalock 1960, p. 345).

## RESULTS

The discussion of the findings begins with a description of the results of the analyses most pertinent to the specific hypotheses about the effects of the ethnic continuum on intellectual functioning, attitudes toward authority, and moral-value systems. Below, in examining alternative explanations of the findings, the paper addresses the more general questions raised in the beginning about the independence, power, scope, and linearity of the hypothesized ethnic variable.

When age, father's education, religion, rurality, and region of the country in which reared are controlled, all the hypotheses about the effects of ethnicity are generally confirmed, as shown in table 2. Individuals belonging to ethnic groups originating in countries with a longer history of freedom from serfdom show a higher level of intellectual functioning (table 2, cols. 1, 2). The  $\beta$  weight for ethnicity and intellectual flexibility is both significant and in the appropriate direction. The  $\beta$  weight on ethnicity of intellectually demanding use of leisure time is also in the predicted direction, although its significance is somewhat equivocal (see notes to table 2).

The predictions about attitudes toward authority also prove to be correct. Members of ethnic groups from countries at the end of the continuum

<sup>5</sup> The linear weightings for these antecedent background variables can be found in Schooler (1972), p. 302. Within each variable the categories are ranked according to the complexity of the child-rearing environment they represent.

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TABLE 2

EFFECTS OF ETHNIC CONTINUUM CONTROLLING FOR FIVE ANTECEDENT VARIABLES,  
SOCIAL CLASS, AND OCCUPATIONAL SELF-DIRECTION  
(*N* = 930)

DEPENDENT VARIABLES	CONTROLLING FOR ANTECEDENT VARIABLES		CONTROLLING FOR ANTECEDENT VARIABLES AND SOCIAL CLASS		CONTROLLING FOR ANTECEDENT VARIABLES AND OCCUPATIONAL SELF-DIRECTION	
	$\beta$ (1)	<i>P</i> (2)	$\beta$ (3)	<i>P</i> (4)	$\beta$ (5)	<i>P</i> (6)
Intellectual flexibility .....	0.146	.001	0.123	.001	0.116	.001
Intellectually demanding use of leisure time .....	0.053	N.S.*	0.030	N.S.*	0.023	N.S.*
Authoritarian conservatism ....	-0.172	.001	-0.152	.001	-0.149	.001
Self-directed parental values <sup>a</sup> ..	0.134	.005	0.113	.015	0.109	.02
Personally responsible morality ..	0.172	.001	0.162	.001	0.159	.001
Sense of control over one's fate ..	0.091	.01	0.085	.025	0.086	.02
Self-deprecation .....	0.098	.005	0.102	.005	0.105	.005

NOTE.—Antecedent variables are: age, father's education, rurality, religion, and region of the country in which reared. The weightings for these variables can be found in Schooler (1972).

\* This question was asked only of parents of children aged three to 15 years (*N* = 489).

<sup>a</sup> When linear analysis of covariance is used, the relationship between ethnicity and intellectually demanding use of leisure time is significant ( $F = 7.70$ ,  $P > .006$ ). The difference between the significant finding and the nonsignificant one seems to lie primarily in the exact population used, the significant finding occurring in that segment of the population for which data are available on all relevant background variables.

relatively remote from serfdom are less authoritarian and more likely to have self-directed, rather than conforming, parental values (cols. 1, 2).

Proof of the hypothesis that persons from ethnic groups long free from serfdom value moral autonomy more is found in their tendency to show more personally responsible morality, to hold themselves more responsible for their own fate, and to have a more self-deprecatory/self-critical view than do members of ethnic groups more recently emerged from feudalism (cols. 1, 2).

Evidence that the ethnic experience is important in causing these differences can be found in the increased strength of the relevant dependent variables' relationships to ethnicity among respondents who report that their ethnic membership has affected them noticeably. The  $\beta$  weights of six of the seven dependent variables on ethnicity are higher among respondents who see their ethnic membership as having a strong or moderate influence on their lives, compared with those who see it as having little or no influence (see table 3).

All these findings seem to verify the hypotheses presented above concerning the effects of serfdom. As predicted, compared with members of ethnic groups from countries where serfdom continued until relatively recently, Americans from ethnic groups originating in countries where the institutions of serfdom were abolished in the more remote past show



TABLE 3

EFFECTS OF ETHNIC EXPERIENCE CONTROLLING FOR AGE, FATHER'S EDUCATION,  
RURALITY, RELIGION, AND REGION OF THE COUNTRY IN WHICH REARED

Dependent Variables	Great or Moderate ( <i>N</i> = 325)	Not Much or Not at All ( <i>N</i> = 563)
	$\beta$	$\beta$
Intellectual flexibility .....	0.173	0.119
Intellectually demanding use of leisure time ..	0.053	0.028
Authoritarian conservatism .....	-0.239	-0.112
Self-directed parental values .....	0.177	0.110
Personally responsible morality .....	0.280	0.124
Sense of control over one's fate .....	0.027	0.103
Self-deprecation .....	0.163	0.065

NOTE.—The question on self-directed parental values was asked only of parents of children aged three to 15 years (great or moderate, *N* = 150; not much or not at all, *N* = 310).

more efficient intellectual functioning, an independent attitude toward authority, and a concern for moral autonomy. The differences are even more pronounced among those who see their ethnic experience as having noticeably affected them.<sup>8</sup>

#### ALTERNATIVE EXPLANATIONS

Although the predicted relationships have been found, many explanations other than these hypotheses are conceivable. The most plausible alternative explanation deals with the question, asked earlier, of whether ethnicity does in fact constitute a social variable distinct from social class. This question can be answered by adding the respondent's present social class,

<sup>8</sup> It is also worth noting that the ethnic continuum represents not only relative but also absolute differences among the ethnic groups. Thus, the groups in the middle of the continuum generally have average scores on the dependent variables, close to the mean scores of the total sample representative of all American employed males (Kohn and Schooler 1969, 1973), while those at either end generally have scores significantly different from the total sample mean. Suggestive evidence in support of the paper's hypothesis is obtained by examining the response patterns of segments of the population not included in the subsample of European ethnic groups. As might be predicted from a simple extension of the hypothesis, blacks, with the most recent history of legal servitude among all American groups, show most strongly the pattern of response typical of ethnic groups with a relatively recent history of serfdom. Interestingly, nonethnic Americans (white gentiles with no grandparents born abroad) are in the same general range as the Irish. This position might well have been predicted on the basis of my rules for ranking ethnic groups, since the United States was once also an English colony. Finally, although individuals belonging to the diverse ethnic groups making up the Austro-Hungarian Empire were not included in the overall analysis because of the limited number of respondents from each of the various cultural and linguistic groups, it is worthy of note that in accord with the general hypothesis, their aggregate scores on the various dependent variables tend to fall in the same area as do those of the Germans and the Italians.

as measured by the Hollingshead Index of Social Position (Hollingshead and Redlich 1958), to the five antecedent variables used, along with ethnicity, as predictors in the multiple-regression equations.<sup>7</sup> Doing so leaves virtually unchanged the effect of the ethnic variable on the dependent variables measuring intellectual functioning, attitudes toward authority, and moral autonomy (see table 2, cols. 3, 4). It is reasonable, therefore, to conclude that ethnicity does have an effect distinct from that of adult social class and the antecedent background variables under consideration.

Although these additional findings further substantiate the hypotheses, it could still be argued that the imposition of a linear continuum is artificial and arbitrary and distorts the true nature of ethnic differences. Fortunately, it is possible to assess directly the degree to which the linearization of an independent variable accounts for the total variance which that variable explains in a dependent variable. This can be done by using analysis of variance to compare the proportion of the variance accounted for by the linear trends (i.e., by the rankings and equality of intervals assumed by the dimensionalization in question) with that accounted for by nonlinear trends (i.e., the variance which violates the linear model). Only two significant nonlinear findings occurred—both in the presence of even more powerful linear ones (respondents of English and Italian origins have somewhat more opportunistic moral standards and a lower level of intellectual flexibility than would be predicted by their position on the ethnic continuum). Furthermore, not only does the hypothesized linear continuum explain most of the variance attributable to ethnicity, but the assumption of such a model seems to be a necessity for the study of ethnicity.<sup>8</sup> Without the continuum, the power of the ethnic variable is so modest that, even with a sample of almost 1,000, if we disregard the linear hypothesis and examine the effect of ethnicity using an analysis-of-variance model that does not test for linearity, only the differences in personally respon-

<sup>7</sup> As is well known, the Hollingshead index is based on a weighted combination of education and occupation. It should be noted that in the original study there was a correlation of 0.89 within a random subsample between the Hollingshead occupational classification and Duncan's, which is based on weighting the census occupational classification in accordance with judgments of occupational prestige held by the society at large (Duncan 1961). For a more complete discussion of the adequacy of the social-class measures used, see Kohn and Schooler (1969).

<sup>8</sup> A slightly altered ranking of the ethnic continuum might be made on the basis of the suggestions of several authors that in the mid-20th century peasants in southern Italy and in Sicily remained as powerless and under the control of large landlords as were their serf ancestors (Muraskin 1974; Galtung 1971). If this is so, Italy might well be ranked behind, rather than ahead of, eastern Europe. For some of the dependent variables, such a change in the ordering of the ethnic groups both decreases the nonlinear and increases the linear effects of ethnicity slightly. It is, however, probably just as well to avoid the possibility of capitalizing on chance by following the statistically somewhat more conservative course of adhering to the hypothesized rankings.

sible morality, intellectual flexibility, and authoritarianism reach statistical significance.

It could also be argued that the neatness of fit between the findings and the model of ethnic effects is a function of the limited choice of dependent variables. As a check, the 15 remaining psychological variables of the original study that measure values and orientations were subjected to both linear regression and nonlinear multivariate analysis of variance.<sup>9</sup> For only one of these psychological variables was there a significant finding, and it is in agreement with the general tenor of the hypothesis: individuals from ethnic groups with a long history of freedom from serfdom are less likely to value the extrinsic characteristics of their jobs than are those belonging to ethnic groups whose mores were more recently influenced by feudalism ( $\beta = 0.109$ ,  $P = <.005$ ).

Another possible objection to the continuum might be to the potential overlap of the ethnic variable with religion. Although the exclusion of Jews from the analyses limits some of the potential effects of religion and although the effects of the differences in liberality among the various types of Christianity are statistically controlled, religious differences are obviously closely intertwined with ethnic ones. In order to demonstrate that the effect of the ethnic dimension is independent of that of religion, two additional analyses were performed. In one a multivariate analysis of variance was carried out with religion and ethnicity as the categorical variables; examinations were made for interactions. There are no significant interactions for the dependent variables with which we are concerned.<sup>10</sup>

An even more stringent test of the independence of the ethnic from the religious factor can be made by examining the effects of the ethnic variable within a given religion. Such an analysis was carried out by analyzing the data for Catholic and Protestant subsamples separately (see table 4). In each instance, when the relevant antecedent variables are included, in six of the seven regression equations the  $\beta$  weights for ethnicity remain roughly the same as those based on the entire ethnic sample. For Catholics, the only  $\beta$  weight noticeably reduced is the one for attribution of responsibility;

<sup>9</sup> The other dependent variables analyzed are: self-confidence, anxiety, idea conformity, disenchantment, noncompulsiveness, distrust, stance toward change, self-evaluating values, self-oriented values, valuation of intrinsic qualities of the job, valuation of extrinsic characteristics of the job, life goal of seeking pleasure for self, life goal of success, social use of leisure time, and household use of leisure time. For the derivation of these indices and their factor loadings, see Kohn (1971) and Schooler (1972).

<sup>10</sup> This lack of significant interactions between religion and ethnicity held even though an extended series of analysis of variance interaction models was tested: (1) neither the ethnic groups nor the different religious denominations treated linearly; (2) both religion and ethnicity linearized; (3) ethnicity linearized and religion not linearized; (4) religion collapsed into Protestant and Catholic and ethnicity linearized; and (5) religion collapsed into Protestant and Catholic and ethnicity not linearized.

# Serfdom's Legacy: An Ethnic Continuum

TABLE 4

ETHNIC EFFECTS: PROTESTANTS AND CATHOLICS ANALYZED SEPARATELY\*

Dependent Variables	Catholics ( <i>N</i> = 416) $\beta$	Protestants ( <i>N</i> = 450) $\beta$	Total Population* ( <i>N</i> = 930) $\beta$
Intellectual flexibility .....	0.174	0.076	0.146
Intellectually demanding use of leisure time .....	0.038	0.044	0.053
Authoritarian conservatism .....	-0.196	-0.112	-0.172
Self-directed parental values† .....	0.099	0.141	0.134
Personally responsible morality .....	0.228	0.032	0.172
Sense of control over one's fate .....	0.014	0.102	0.091
Self-deprecation .....	0.076	0.089	0.098

NOTE.—For Protestants, all five antecedent variables were controlled. For Catholics, inclusion of religious liberality would have been meaningless, so only age, father's education, rurality, and region of the country in which reared were included.

\* Table 2, cols. 1, 2.

† This question was asked only of parents of children aged three to 15 years (Catholics, *N* = 213; Protestants, *N* = 230).

for Protestants, the only one substantially reduced is the conceptually central one of personally responsible morality. In evaluating the meaning of these reductions, two things should be borne in mind. First, although the size of the ethnic  $\beta$  weights for these two variables is very much reduced, they remain in the predicted direction, a trend consonant with the above-noted absence of significant interactions between religion and ethnicity. Second, in the case of the centrally important variable of moral responsibility, if one focuses on the predominantly Protestant ethnic groups and their major religious subdivisions, ethnicity does tend to have its predicted independent linear effect (see table 5).

All the previous alternative explanations are predicated on the possibility that the ethnic effects found are spurious or artifactual. Another class of

TABLE 5

ETHNICITY AND PERSONALLY RESPONSIBLE MORALITY AMONG MAJOR RELIGIOUS  
SUBDIVISIONS OF PREDOMINANTLY PROTESTANT ETHNIC GROUPS  
(*N* ≤ 5)

AREAS	OLD ESTABLISHED (EPISCOPAL, CONGREGATIONAL, PRESBYTERIAN)		LUTHERAN		METHODIST		BAPTIST, FUNDAMENTALIST, TRANSITIONAL SECTS	
	$\bar{X}$	<i>N</i>	$\bar{X}$	<i>N</i>	$\bar{X}$	<i>N</i>	$\bar{X}$	<i>N</i>
Scandinavia .....	46.4	11	46.5	12	47.1	12	43.9	5
England .....	48.3	21	...	...	49.4	21	47.1	23
Germany .....	50.4	23	48.1	74	49.0	23	46.3	26

NOTE.—Controlling for age, region, rurality, and father's education. Analysis of covariance, *F*(1.276): ethnic linear, *F* = 2.19, *P* = .14; ethnic nonlinear, *F* = 1.10, *P* < .30; linear (ethnic *X* religion), *F* = 0.42, *P* = .52.

alternative explanations exists, one which accepts the reality of the relationships between ethnicity and intellectual functioning, attitudes toward authority, and moral autonomy but sees them as caused by elements other than direct cultural transmission.

Particularly germane is a model based on the possibility that members of diverse ethnic groups may have different occupational histories because of either voluntary choice or discrimination. Ethnic differences in occupational preference may occur because members of ethnic groups whose cultures have been influenced by relatively recent histories of serfdom may be less prone to choose self-directed occupations than those of ethnic groups more remote from serfdom. Thus, instead of resulting from direct cultural transmission, the ethnic differences found in values and functioning may result merely from ethnic differences in occupational experiences affecting just those psychological variables with which we are concerned. Furthermore, even if they should want self-directed jobs, those from ethnic groups more recently emerged from serfdom may be at a disadvantage in obtaining them. An examination of the ethnic continuum shows that it is correlated with the level of esteem in which the various ethnic groups are held and hence quite possibly with the degree of discrimination individuals face in seeking generally high-status self-directed jobs (Bogardus 1928; Spoerl 1951).

Although the effects of voluntary choice cannot be separated from those of discrimination, the data provide evidence for just such occupational selection among ethnic groups. Even with the antecedent variables mentioned above controlled, the  $\beta$  weight between our ethnic continuum and the factor score measuring occupational self-direction is significant and in the predicted direction ( $\beta = 0.084$ ,  $P < .015$ ).<sup>11</sup> However, the data also provide evidence that ethnic differences in occupational self-direction do not cause the differences in values and functioning described earlier; controlling for the effects of occupational self-direction by including it as an independent variable together with ethnicity and the five antecedent variables in the regression equations leaves the effects of ethnicity virtually

<sup>11</sup> To create an overall index of occupational self-direction, we (Kohn and Schooler 1969) used the factor loadings for the first unrotated factor of the entire set of items related to the use of initiative, thought, and independent judgment in work. The loadings are: (1) five-item Guttman closeness-of-supervision scale ( $-0.549$ ); (2) hours of work with data ( $-0.576$ ); (3) hours of work with things ( $0.595$ ); (4) hours of work with people ( $-0.531$ ); (5) complexity of work with data ( $0.834$ ); (6) complexity of work with things ( $-0.178$ ); (7) complexity of work with people ( $0.80$ ); (8) overall complexity of work ( $-0.826$ ); (9) repetitiousness of things done ( $-0.36$ ); (10) repetitiousness of way of doing things ( $-0.491$ ); (11) quantity of work defining work unit ( $-0.30$ ); (12) quantity of time defining work unit ( $-0.166$ ); (13) intrinsic definition of work unit ( $0.368$ ).

unchanged (see table 2, cols. 5, 6). Thus, ethnic differences in values are transmitted in some way other than through the effects of occupational selection on job complexity.

## DISCUSSION

This paper began with a series of questions about the nature of ethnicity in America and its effects. Through its analyses it provides substantial evidence that ethnicity does have an effect and that this effect is generally independent of the individual's adult social class and of background variables affecting the complexity of his childhood environment. Furthermore, the effects of ethnicity appear stronger among respondents to whom the ethnic experience appears central.

This study's findings also appear to demonstrate that the effects of ethnicity are congruent with the assumption of a historically determined continuum reflecting the social, legal, economic, and occupational conditions of the European countries from which American ethnic groups emigrated. Furthermore, the apparent effects of those conditions on the culture of the various ethnic groups are remarkably similar to the ones social and occupational conditions have previously been demonstrated to have on the individual in present-day America. Such a similarity raises the possibility that social and occupational conditions prevailing within a society may affect its culture in a manner analogous to the way in which an individual's occupational experiences affect his values and functioning. Serfdom and its correlates tended to involve living and working under someone else's control in situations which left few options open. Ethnic groups with a long history of serfdom show the intellectual inflexibility, authoritarianism, and pragmatic legalistic morality typical of men working under such circumstances.

Similarly, the occupational and social conditions of their ancestors appear to have affected those from ethnic groups with a relatively long history of freedom from serfdom. Belonging to an ethnic group with a long history of freedom from serfdom has the same general empirical relationship with intellectual functioning, attitude toward authority, and moral autonomy as does working in a substantively complex or self-directed job. Both conditions seem to produce persons who are intellectually more effective, who believe that they have some control over their lives, and who feel that the ultimate locus of ethical responsibility is within themselves, rather than in authorities, the law, or other external enforcers of conformity. The internalization of ethical responsibility of those from such ethnic groups also seems to limit their ability to shift the burden of their ethical responsibility onto others, thus tending to make them more self-critical.

Underlying the tentative tone of all of these conclusions are two linger-

ing doubts. Is ethnicity the actual and nonspurious cause of the differences found? If so, are the interrelated legal, economic, and occupational correlates of a history of recent serfdom which have been enumerated really centrally involved in the causal processes leading to these differences? The answer to the first question—Have we found true ethnic differences?—is very probably yes. The various controls used seem to rule out the possibility that the results are spurious artifacts of differences in rurality, region, age, religion, father's education, present social class (as measured by education and occupational status), and substantive complexity of present occupation. The ethnic differences in question consistently remain when these variables are controlled. If they were spurious, they would have to be the results of something outside this wide range of variables.

The answer to the question of the centrality of the conditions of serfdom to the causal process making for differences among American ethnic groups must remain somewhat tentative. Here too, however, certain alternative hypotheses may be ruled out. Thus, although the ethnic continuum reflects the time of the end of serfdom, it does not directly reflect climate, time of industrialization or urbanization of the old country, time of major immigration to the United States, or social class and motivation of the immigrants.<sup>12</sup> Thus, since they are not reflected by the continuum, these variables too may be added to the list of background variables that are not primary causes of the ethnic differences found. Such an augmented list seems to suggest that the ethnic continuum is not the result of differential immigration patterns or of nonspecifically ethnic social patterns in the United States. And thus it seems likely that we are in fact dealing with the remnants of cultural patterns originating in the agricultural sectors of the old country, brought over by the immigrants and carried on to some extent by their descendants. It is, of course, still possible that the ethnic differences found among ethnic groups are due to variables unrelated to the processes described above. Although such alternative models cannot be excluded, the causal processes they postulate would have to operate relatively independently of any intervening mechanisms represented by the variables that have been statistically controlled (Blalock 1964). Since controlling these variables does not substantially reduce the effect of the

<sup>12</sup> Some of the more notable discrepancies among the ethnic groups' rankings on these variables and their rank order on the continuum are the following: England and Germany were industrialized and urbanized before the Scandinavian countries (Crawley 1965); Russia and Italy, at the same end of the continuum, have distinctly different climates; and the peak of Irish immigration was 40 years earlier than that from Scandinavia (Kennedy 1964). Finally, among the ethnic groups under consideration, the German group was the only one with a noticeably large middle-class and urban minority and with motives for migration that included political as well as economic elements (Kennedy 1964).

ethnic continuum, they cannot be a direct part of the causal processes leading to the ethnic differences in question.<sup>13</sup> Thus, it would seem that a model which emphasizes the effects on different ethnic groups' cultural values of historical conditions in Europe which restricted the individual's autonomy does in fact represent a parsimonious and compelling explanation of certain ethnic differences in present-day America.

The ethnic differences found are admittedly modest and circumscribed. Possibly, if this paper were not limited to dealing with structured survey data, differences in the characteristic flavor of the various ethnic groups might emerge more clearly. One would not readily try to linearize the difference between a smorgasbord and an antipasto. On the other hand, the hypotheses underlying the ethnic continuum are based on previous findings of how occupational conditions affect individual functioning. Thus, to the extent that the ethnic continuum's explanatory power is based on historical differences in occupational conditions, it not only gives meaning to the present findings, but it also extends the known range of occupational effects from the individual to the culture to which he belongs. But since, as we have seen, the differences in occupational self-direction reflected by the ethnic continuum seem to have been paralleled by legal, economic, and social constraints on the individual's freedom, the model suggests that a whole range of historical circumstances may have combined to determine at least partially the differences among American ethnic groups in intellectual functioning, attitudes toward authority, and moral autonomy. Thus, though no longer serfs, Americans from ethnic groups with a recent and pervasive history of serfdom appear heirs to cultural traditions whose values were molded by the oppressed lives of their ancestors.

<sup>13</sup> Further insight can be gained by comparing the effects of coming from an ethnic group having a relatively long history of freedom from serfdom with the effects of relatively high adult social-class status on the one hand and those of being raised in a complex environment on the other. Although all three characteristics result in relatively good intellectual functioning, feelings of efficacy, and the placing of a high value on self-direction, an earlier paper noted that adult social class and complex childhood environment have different effects on personally responsible moral standards (Schooler 1972). Higher social-class status increases the likelihood of having such a personally responsible moral system, while being raised in a complex environment generally decreases it. In this respect the effects of ethnicity seem to parallel those of social class. In both cases, those in relatively privileged positions may more readily identify their own well-being with that of society as a whole; some such internal identification of one's own goals with those of the social system the values of which one internalizes may well be necessary if one is to profess a personally responsible morality. On the other hand, individuals raised in complex environments, where many alternate goals are readily visible and relationships with larger social groupings are at best ambiguous, may find it difficult to identify their own goals with those of larger social groupings, thus decreasing the likelihood that they would espouse a personally responsible morality.



APPENDIX

Psychological Variables Used in This Study (Factor Loadings in Parentheses)

*Intellectual flexibility.*—Specifically, we asked: (1) "Suppose you wanted to open a hamburger stand and there were two locations available. What questions would you consider in deciding which of the two locations offers a better business opportunity?" (2) "What are all the arguments you can think of for and against allowing cigarette commercials on TV? First, can you think of arguments *for* allowing cigarette commercials on TV? And can you think of arguments *against* allowing cigarette commercials on TV?" The perceptual test consists of a portion of Witkin's Embedded Figures Test (Witkin et al. 1962), selected by Witkin. The Figure-drawing Test (pp. 117–29) consists simply of asking the respondent to draw a figure of a man on a standard-sized card with a standard pencil. Respondents are assured that artistic ability is not required. The meaningful coherence of the figure is what we appraise. Ideational flexibility is based on the interviewer's appraisal of the respondent's intelligence ( $-0.69$ ), the cigarette-commercials problem ( $-0.61$ ), the respondent's "agree" score ( $0.56$ ), the hamburger-stand problem ( $-0.54$ ), and the Embedded Figures Test ( $-0.52$ ). Perceptual flexibility is based primarily on the draw-a-man test (summary score,  $-0.91$ ; Goodenough estimate of intelligence,  $-0.91$ ; Goodenough 1926) and the Embedded Figures Test ( $-0.43$ ). For the derivation of these indices and their factor loadings, see Kohn (1971, pp. 464–65). The overall measure of intellectual flexibility used in this study is the summation of the ideational and the intellectual flexibility factor scores. Analyses done with each of these factor scores separately, though not so clearcut, are generally in accord with the findings presented here. As with the other dependent variables, the lower the score, the more of the named quality.

*Intellectually demanding use of leisure time.*—This variable includes frequency of visits to plays, concerts, and museums ( $-0.60$ ); number of books read in the past six months ( $-0.54$ ); time spent working on hobbies ( $-0.35$ ); amount of magazine reading ( $0.61$ ); and time spent watching television ( $0.35$ ).

*Authoritarian conservatism.*—(1) The most important thing to teach children is absolute obedience to their parents ( $-0.61$ ). (2) Young people should not be allowed to read books that are likely to confuse them ( $-0.58$ ). (3) There are two kinds of people in the world: the weak and the strong ( $-0.58$ ). (4) People who question the old and accepted ways of doing things usually just end up causing trouble ( $-0.55$ ). (5) In this complicated world, the only way to know what to do is to rely on leaders and experts ( $-0.52$ ). (6) No decent man can respect a woman who has

had sex relations before marriage ( $-0.51$ ). (7) Prison is too good for sex criminals. They should be publicly whipped or worse ( $-0.51$ ). (8) Any good leader should be strict with people under him in order to gain their respect ( $-0.45$ ). (9) It's wrong to do things differently from the way our forefathers did ( $-0.43$ ).

*Self-directed parental values.*—The factor loadings for this variable contrast neatness ( $-0.62$ ), manners ( $-0.56$ ), being a good student ( $-0.35$ ), and obedience ( $-0.34$ ) with interest in how and why things happen ( $-0.51$ ), consideration ( $0.43$ ), good sense ( $0.30$ ), self-control ( $0.29$ ), and responsibility ( $0.28$ ). The original study also included the indices of men's values for themselves; they were not, however, so well constructed or unambiguous in their meaning as these parental values. For a fuller discussion of the derivation and adequacy of these indices, see Kohn (1969, pp. 56–59).

*Personally responsible morality.*—(1) It's all right to do anything you want as long as you stay out of trouble ( $0.66$ ). (2) If something works, it doesn't matter whether it's right or wrong ( $0.57$ ). (3) It's all right to get around the law as long as you don't actually break it ( $0.54$ ). (4) Do you believe that it's all right to do whatever the law allows, or are there some things that are wrong even if they are legal? ( $0.51$ ).

*Sense of control over one's fate.*—(1) When things go wrong for you, how often would you say it is your own fault? ( $-0.72$ ). (2) To what extent would you say you are to blame for the problems you have—would you say that you are mostly to blame, partly to blame, or hardly at all to blame? ( $-0.68$ ). (3) Do you feel that most of the things that happen to you are the result of your own decisions or of things over which you have no control? ( $-0.60$ ).

*Self-deprecation.*—(1) I wish I could have more respect for myself ( $0.62$ ). (2) At times I think I am no good at all ( $-0.55$ ). (3) I feel useless at times ( $-0.54$ ). (4) I wish I could be as happy as others seem to be ( $-0.53$ ). (5) There are very few things about which I'm absolutely certain ( $-0.43$ ).

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# Network Sampling: Some First Steps<sup>1</sup>

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Social network research has been confined to small groups because large networks are intractable, and no systematic theory of network sampling exists. This paper describes a practical method for sampling average acquaintance volume (the average number of people known by each person) from large populations and derives confidence limits on the resulting estimates. It is shown that this average figure also yields an estimate of what has been called "network density." Applications of the procedure to community studies, hierarchical structures, and interorganizational networks are proposed. Problems in developing a general theory of network sampling are discussed.

Sociologists and anthropologists have discussed and studied communities since their disciplines began. As the communities studied have increased in size, the fact that not all community members have social relations with one another has become a matter of prominent theoretical focus. The metaphor most consistently chosen to represent this situation is that of the "social network"—a device for representing social structure which depicts persons as points and relations as connecting lines. (Good general discussions are found in Barnes 1969; Bott 1957; Mitchell 1969; White, Boorman, and Breiger 1976).

Most discussions of network ideas, however, have had practical application only to small groups. Inability to apply the ideas effectively to larger structures has stemmed in part from the lack of a theoretical framework in which to place the network metaphor and in part from the absence—and perceived difficulty—of methods applicable to and statistical understanding of large networks. In an earlier paper (1973) I suggested some theoretical leads for the application of network ideas to macrosociology; here I explore some statistical and methodological avenues which, when more fully developed, should help to bring the network perspective more squarely into the mainstream of sociological research.

It is clear why network methods have been confined to small groups: existing methods are extremely sensitive, in their practicality, to group

<sup>1</sup> An early version of this paper was delivered at the Mont Chateau conference on the anthropological study of social networks, sponsored by the Mathematical Social Science Board, Morgantown, West Virginia, May 16–19, 1974. Discussions begun at that conference led to crucial improvements. In particular, the progress reported here would not have been possible without the collaboration and stimulation of Paul Holland; remarks by Samuel Leinhardt triggered important parts of the work. I am also indebted to Harrison White, Stanley Wasserman, and Ove Frank for valuable criticism.

size because they are population rather than sampling methods. In a group of size  $N$ , the number of potential (symmetric) ties is  $[N \cdot (N - 1)/2]$  (i.e., proportional to  $N^2$ ), so that any method meant to deal with the total population faces insuperable obstacles for groups larger than a few hundred. A group of 5,000, for instance—which we might think of as a small town—contains over 12 million potential lines in its network. Yet most Americans live in much larger aggregates, which analysts nevertheless, in community studies, persist in thinking relevant as social units. Implicitly, these studies often make arguments about the community's total network, but they rarely do so explicitly, because no methods exist for investigating such an object. Implicitly, again, what all such studies do, and must do, is to sample from that network. But because the procedure is not explicit and no statistical theory guides it, we are left guessing about the representativeness of the patterns of social relations found. This uncertainty is particularly noticeable when the "sampling" procedure is one of participant observation, but representativeness is problematic even if the procedure consists of asking a random sample of the community some sociometric questions. Just as an enormous advance in sociological work ensued when the general theory of random sampling was developed and applied to sociological problems, so the full development of network ideas in macrosociological perspective must await a comparable theory of network sampling. At present, only a few analysts have attacked this problem (Goodman 1961; Bloemena 1964; Capobianco 1970; Frank 1971), and only Frank attempts a comprehensive statistical treatment.

In this paper, I show that for one simple but important property of social networks, "density," a straightforward and practical method can provide acceptable sampling estimates even for very large populations. I then suggest applications of the method and discuss the more general problems of sampling from networks.<sup>2</sup>

Network density is the ratio of the number of ties actually observed to the number theoretically possible. In small groups, density is usually treated as a measure of group "cohesion" (Festinger, Schacter, and Back 1950, chap. 5) and as a partial indication of the extent to which a group

<sup>2</sup> I should stress that the basic results here are made possible by the pathbreaking work of Ove Frank (1971), professor of statistics, University of Lund, Sweden. Even more important than his actual results is Frank's demonstration that network sampling problems can be attacked with relatively standard methods of statistical inference (e.g., the use of indexing variables), although their application requires a good deal of imagination. Another important breakthrough which deserves to be followed up is Goodman's paper on "snowball sampling" (1961). Snowball sampling is not appropriate in the present paper, however, because its practicality is limited to cases where respondents make a fairly small number of sociometric choices. I mean to develop methods relevant to respondents' *entire* friendship networks, including the many people they know whom it would not occur to them to choose in a limited-choice situation. On the general significance of "weak ties," see my 1973 paper in this *Journal*.

is "primary" or "closed" (see Homans 1974; Bott 1957). In communities or larger settings, density has been used to indicate levels of "modernization" (see Mayer 1961; Tilly 1969). A good general discussion of density can be found in Barnes (1969).

#### DENSITY AND THE "HOW-MANY-PEOPLE" PROBLEM

Before discussing the sampling method, I want to make a detour to show that finding the density in networks is actually, given certain limitations, equivalent to answering the question "How many people do people know?" That question, though one might suppose its answer to be a fundamental social fact, has actually been studied very little, and no systematic information exists for representative populations.

Initially, the problem may seem straightforward: If we want to know how many people someone knows, why not ask him? To be quite frank, no *direct* evidence shows that this procedure would give poor results. Indirect evidence and everyday experience, however, suggest that most individuals could give only a very rudimentary estimate. The obvious method would be to ask a respondent to write out a list of all the people he knows and count the names. But such a substantial proportion of one's contacts are seen infrequently that they would come to mind only with some difficulty, particularly during the limited time one could allow or expect for the schedule to be filled out.

Gurevitch (1961) used an ingenious extension of this method, which insured far greater accuracy. He asked each respondent to keep a daily diary over a period of 100 days, listing, each day, all the persons he came into contact with who also knew *him* by this criterion (p. 1, n). This time period was chosen because "the net increment during the tail end of this period was small enough to justify termination of the procedure" (p 43).

The method, a variant of time-budget techniques, gives excellent results but has serious drawbacks. The most serious and obvious is that such sustained commitment of respondents can probably only be secured on a paid basis. Gurevitch's sample consisted of 15 individuals who responded to a notice offering pay for this activity and three unpaid volunteers (who presumably knew him). The size and character of his sample make generalization from it impossible, and the cost of the method makes reasonable samples impractical to draw. Another difficulty is that the method misses contacts seen less frequently than every 100 days. In a place where someone has lived for some years, there may be a substantial number of these.

A very different method becomes available if we shift the focus away from the individuals to the communities in which they live. In this more macroscopic perspective, we may view acquaintance volume as a char-

acteristic not simply of individuals but of the entire community. In fact, if we are willing to sacrifice individual detail and investigate the *average* number of people known to people within a bounded community (in which case we also miss, of course, contacts outside the boundary we set), the question does reduce to that of network density, as follows: In a group of size  $N$ , where  $N_t$  ties are observed, the density measure,  $D$ , is  $N_t/[N(N-1)/2]$ , where all ties are assumed symmetric. But since each of the  $N_t$  observed ties represents *two* cases of someone knowing someone else, the total number of contacts in the group must be  $2N_t$ , and the average number per person  $2N_t/N$ . Call this quantity  $V$ , for average acquaintance volume. Simple algebra now shows that  $V = (N-1)D$ . Hence, any method which finds density also finds average acquaintance volume. In what follows, I will often describe networks in terms of their average acquaintance volume instead of their density, given the greater intuitive appeal of the former. When I discuss applications, it will be clear that the equivalence also has substantive importance.

#### THE SAMPLING METHOD

Given a population of size  $N$ , the method proposed is to take a number of random samples from that population, each of size  $n$  (with replacement), and *within* each such sample ask each respondent some sociometric question about *each other respondent*. Which sociometric question is asked depends on the purpose of the particular investigation. If the main focus, for example, were the "how-many-people" question, it would be sufficient to ask whether the respondent knew each of the other  $n - 1$  respondents by name. In this method, frequency of contact is irrelevant—people seen only every few years, or less often, have the same chance of being named as those seen every day. One could assure himself that results would be accurate for a given sample by providing as a stimulus not only the names of the  $n - 1$  others, but also other relevant information such as address or occupation; even photographs could be used to be sure that acquaintances whose faces were better remembered than their names would be recognized.

In the language of graph theory, each sample, once lines are drawn among respondents corresponding to their sociometric responses, is a "random subgraph" from the population.<sup>3</sup> By averaging the densities found in the various samples taken, one arrives at an estimate of the density in the population network. (In the Appendix I give a proof that this estimate is unbiased.)

Two sampling parameters need to be set: the number of samples taken

<sup>3</sup> The use of random subgraphs was first suggested to me by Samuel Leinhardt.

and the size of each sample. One can imagine taking a large number of small samples (e.g., thousands of random pairs) or a small number of large samples (e.g., a few samples of several hundred or more). Some previous work on network sampling has focused on the idea that the relevant sampling unit ought to be the "tie"—that one should sample not from the  $N$  individuals but from the  $N(N-1)/2$  possible lines in the network, to see in which cases lines are actually observed (Capobianco 1970; Niemeijer 1973; Tapiero, Capobianco, and Lewin 1975). This has a certain intuitive appeal but needs to be seen as a special case of sampling random subgraphs, with each subgraph containing two points. In effect, it is one example of the "large number of small samples" strategy referred to above. In this perspective, the calculations below make it clear that the "small number of large samples" is almost invariably a more efficient strategy.

The crux of the statistical problem then, is to determine what combinations of the two sampling parameters will insure a good estimate of acquaintance volume.

The first step must be a formula for the variance of our density estimate. This can be derived easily from a crucial result obtained by Frank (1971, p. 92), who shows that when exactly *one* subgraph of size  $n$  is sampled from a population of size  $N$ , and  $T$  denotes a random variable, the number of ties observed in this subgraph,

$$\text{Var}(T) = (N-n)n(n-1)(n-2)s^2(a)/(N-1)(N-2)(N-3) + (N-n)(N-n-1)n(n-1)s^2(C)/2(N-2)(N-3), \quad (1)$$

where  $s^2(a)$  is the variance of the true vector of "outdegrees," that is, individual acquaintance volumes, and  $s^2(C)$  is the variance of the true (population) sociomatrix.

By definition, our density estimate, to be called  $\hat{D}$ , is equal to  $T/\binom{n}{2}$ . Thus,  $\text{Var}(\hat{D}) = \text{Var}(T)/(4/[n^2(n-1)^2])$ . Now, suppose more than one subgraph of size  $n$  is sampled—namely,  $w$  such samples—and we average all their density estimates to get an overall estimate  $\hat{D}_{av}$ . Neglecting the covariance among the pooled estimates,<sup>4</sup> we then have

$$\text{Var}(\hat{D}_{av}) = (1/w) \left[ \frac{2(N-n)}{(N-2)(N-3)n(n-1)} \right] \cdot \left[ \frac{2(n-2)}{(N-1)} s^2(a) + (N-n-1)s^2(C) \right]. \quad (2)$$

Equation (2) shows, as one might suspect, that the variance of the

<sup>4</sup> It is safe to neglect the covariances so long as  $n \ll N$ , or where  $n$  is moderate, so long as  $w$  is small. As will be shown below, these conditions apply in nearly all conceivable cases.



density estimate depends on the detailed structure of the actual population network, as given by  $s^2(C)$  and  $s^2(a)$ . It is easily shown (see Frank 1971, pp. 70-72) that  $s^2(C) = D(1 - D)$  and that

$$s^2(a) = (N - 1)s^2(C) - \frac{N - 1}{N} \cdot \sum_{i \neq j} \{C_{ij} - [a_i/(N - 1)]\}^2,$$

where  $a_i$  is the number of ties involving person  $i$ , that is, the sum of row  $i$  in the sociomatrix.

Since the parameter  $s^2(C)$  is completely determined by the density, the first step in arriving at a density estimate is to guess at the *true* density for a given population; to fix  $s^2(a)$  requires more complex assumptions about how that density is distributed. First of all, since it is a variance, it must = 0 if  $a_1 = a_2 = \dots a_n$ —that is, if every individual knows the same number of other people. The number,  $a_i$ , would then be exactly the average acquaintance volume and would hence minimize  $\text{Var}(\hat{D}_{av})$  and the required sample size, for fixed density and confidence limits. (This is also clear from inspection of eq. [2].)

Correspondingly,  $s^2(a)$  is maximized, for a given density, when all the acquaintanceship is concentrated in the smallest possible number of people, and everyone else knows no one. In order to say what this "smallest possible number" is, we must first specify the maximum number of people anyone might know. The larger this number, the higher the variance of  $s^2(a)$ , since the number who know any people can then be quite small compared to population size. A conservative procedure would be to set this number high and imagine that, in our maximum baseline population, if someone knows *any* people, he knows the maximum number. I will set this figure at 2,000. (In the Gurevitch 100-day diary study [1961] the largest number of acquaintances reported by any respondent was 658.) In a population of 100,000, then, with average acquaintance volume of 100, the maximal case would exist if 95,000 people knew no one, and each of the 5,000 others knew 2,000 from among one another. If  $(N \cdot V)/2,000 < 2,000$ , some modification is needed in the definition of "maximal." For instance, where  $N = 10,000$  and  $V = 100$ , the present definition suggests a maximal graph as one with 9,500 people who know no one and 500 who each know 2,000 others. But this is internally inconsistent, since "knowing" is symmetric. In such cases, the logical procedure is to take  $(N \cdot V)^{1/2}$  as the number of people who know anyone at all, and assume that each of them knows each other person in the group, and no one else knows anyone. Utilization of this procedure yields, if  $N = 10,000$  and  $V = 100$ , a graph in which a set of 1,000 people know each other and 9,000 know no one.

Given fixed  $D$  (or  $V$ ) this condition maximizes the variance of  $\hat{D}$ . The

overall result is plausible: Variance (and hence required sample size) is minimized in the fully homogeneous network and maximized in the maximally cliqued one.

It seems clear to me that the latter situation is much further from reality than the former. Even people who moved into a community yesterday are not isolates, and the great majority are likely to know some moderate number of people. Although I have no direct evidence, I would be surprised if more than, say, 20% of a population knew *many* more than the average number of people. Let me, then, arbitrarily define a "typical" population as one in which this is so. Specifically, let  $f_k$  be the proportion of the population whose acquaintance volume is equal to  $k$  times the average. For volume of 100 or 500, suppose  $f_{0.5} = 0.4$ ,  $f_1 = 0.4$ ,  $f_{1.5} = 0.1$ ,  $f_2 = 0.075$ ,  $f_4 = 0.025$ . For volume of 1,000,  $f_4$  should be zero, in keeping with our stricture that no one knows more than 2,000 others; for that case, let  $f_{0.5} = 0.3$ ,  $f_1 = 0.5$ ,  $f_{1.5} = 0.1$ ,  $f_2 = 0.1$ . Using this somewhat arbitrary notion of a typical population distribution of acquaintance volume, and the previously defined minima and maxima, we can arrive at some idea of what sample size will be needed to get a decent density estimate.

Let our idea of "decent" correspond to permitting a 20% error. While statisticians will blanch at this definition of decent, I argue that such a range will serve most purposes well enough. When two communities are compared whose true acquaintance volumes fall within the range of one another's 20% error limits, it seems doubtful that the true difference would be of much substantive significance anyway. In any case, formula (3) below can be modified to permit more narrow error limits.

Suppose the density estimates from a subgraph of size  $n$  are approximately normally distributed about the true density. Then 95% of the estimates will fall within 1.96 SD of the true density. That is, our 95% confidence interval of 20% tolerable error requires that

$$0.2D \geq 1.96 \left\{ \frac{1}{w} \left[ \frac{2(N-n)}{(N-2)(N-3)n(n-1)} \right] \cdot \left[ \frac{2(n-2)}{(N-1)} s^2(a) + (N-n-1)D(1-D) \right] \right\}^{\frac{1}{2}}. \quad (3)$$

For specified  $N$ ,  $n$ ,  $D$ , and  $s^2(a)$ , we can solve the inequality for  $w$ , giving us the minimum number of samples of size  $n$  needed to come within the specified limits. (Changes in degree of error tolerated or in level of confidence required can be introduced by substituting the desired values for 0.2 or 1.96.)

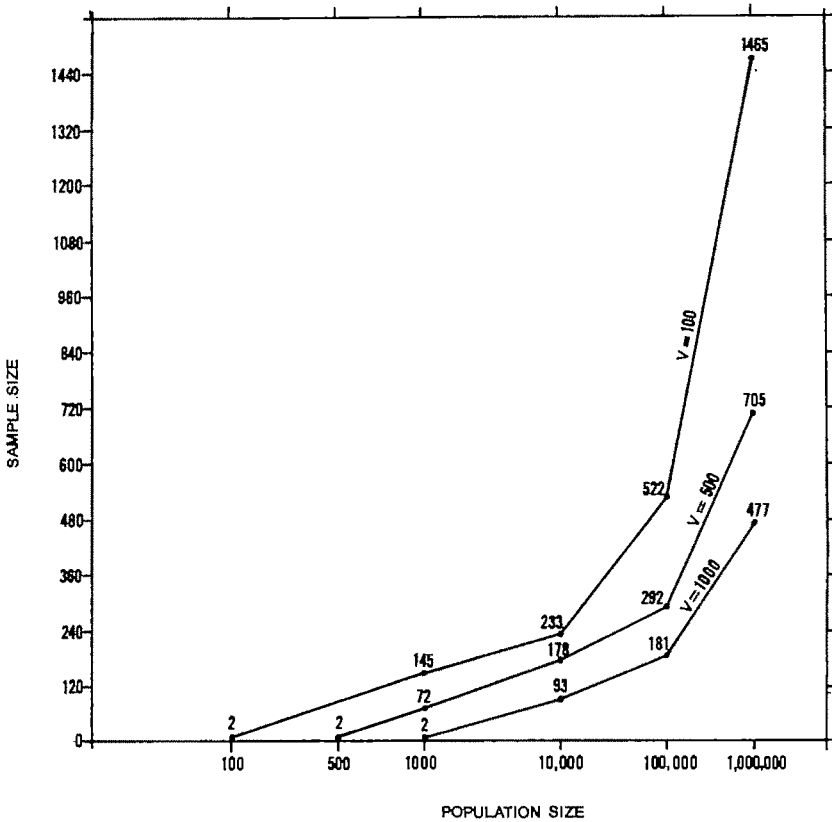


FIG. 1.—Sample size required for 95% confidence limits on network acquaintance volume, 20% error, for population sizes from 100 to 1,000,000, and true volume ( $V$ ) of 100, 500, and 1,000.

Figure 1 gives the values of  $n$  for which  $w = 1$  (i.e., only *one* sample of size  $n$  is needed) under various assumptions about acquaintance volume (density) and population size ( $N$ ). In this graph, the population is assumed to show the "typical" distributions of acquaintance volumes outlined above, except that where volume is 500 and  $N = 1,000$ , the 0.3, 0.5, 0.1, 0.1 pattern is used. With the exception of new towns in very early stages, it seems unlikely that any communities would fall outside the acquaintance volume range of 100–1,000 used in figure 1.

The numbers here suggest that even for rather large populations the sample size required for the estimate is modest. Some empirical experience will be needed before we can say what the largest sample is in which we can, as a practical matter, expect each individual to answer sociometric questions about each other. I would think that this could be done easily

for samples of 500.<sup>5</sup> Figure 1 shows that only for the community of 1 million would a larger sample be needed. In such cases, if indeed 500 were the practical cutoff for this method, the difficulty could be met by taking *multiple* samples of size 500. With a population of 1 million and true average acquaintance volume of 500, two such samples would suffice; where true volume drops to 100, eight would be needed.

A nice irony of the method outlined is that it is of only marginal value for *small* populations, unlike all other network techniques. For a population of size 50, for example, one would need a random sample of at least 25 for a decent estimate. While this might occasionally be of some value, the real power of the method appears only for larger networks, ones at least in the hundreds.

Table 1 shows these computations not only for the "typical" case, but

TABLE 1  
SAMPLE SIZES NEEDED TO MEET 20% ERROR, 95% CONFIDENCE LIMITS, FOR  $w = 1$

Population Size ( $N$ )	Average Acquaintance Volume	Value of $n$ for Which		
		Minimum	$w = 1$ "Typical"	Maximum
1,000,000 .....	100	1,382 (7.69)	1,465 (8.01)	7,460 (22.24)
1,000,000 .....	500	619 (1.54)	705 (1.86)	1,415 (3.83)
1,000,000 .....	1,000	438	477	668 (1.53)
100,000 .....	100	436	522 (1.08)	6,800 (15.25)
100,000 .....	500	195	292	1,165 (2.44)
100,000 .....	1,000	138	181	425
10,000 .....	100	136	233	2,570 (6.62)
10,000 .....	500	60	178	1,030 (2.20)
10,000 .....	1,000	41	93	370
1,000 .....	100	40	145	454
1,000 .....	500	14	72	137
1,000 .....	1,000	...	...	...

NOTE.—Where a single sample larger than 500 would be needed, the number of samples required of size 500 is given in parentheses.

also for the minimum and maximum variance conditions specified above. Where  $n > 500$ , the number of samples of size 500 needed to meet the criterion is indicated in parentheses. As one might expect, required sample size goes down as population size goes down and as acquaintance volume goes up. The figures for "typical" populations are much closer to the minimum than the maximum. An analyst with a population suspected to be highly cliqued, however, can take comfort from the fact that even the sample size required for the maximum degree of cliquing is within practical bounds, for most cases.

<sup>5</sup> In some situations this may be doubled (see the section below on one-way questioning).



One final issue, mentioned above, is the trade-off between sample size and number of samples. At the beginning of my work, I was intrigued by the idea that a large number of samples of pairs or triads would provide the key to network sampling. Table 2 shows why this is not so. For pair

TABLE 2  
TWO ILLUSTRATIONS OF TRADE-OFF BETWEEN SAMPLE SIZE ( $n$ ) AND NUMBER OF  
SAMPLES TAKEN ( $w$ )

$n$	$w$	Maximum Number in Samples	Maximum Number of Questions Asked
$N = 100,000$ ; $V = 500$ ; 20% Error, "Typical" Variance of $V$			
2 .....	19,112	38,224	38,224
3 .....	6,398	19,194	38,388
10 .....	439	4,390	39,510
50 .....	19	950	46,550
100 .....	5	500	49,500
200 .....	2	400	79,600
292 .....	1	292	84,972
$N = 1,000,000$ ; $V = 500$ ; 20% Error, "Typical" Variance of $V$			
2 .....	191,984	383,968	383,968
3 .....	64,022	192,066	384,132
10 .....	4,281	42,810	385,290
50 .....	160	8,000	392,000
100 .....	40	4,000	396,000
500 .....	2	1,000	499,000
705 .....	1	705	496,320

sampling, a prohibitive number of people would have to be contacted. The total number of people one would have in all samples goes down steadily as the size of the samples increases. (Since sampling is with replacement, 19,112 samples of two from a population of 100,000 would involve fewer than 38,224 persons, but the expected number is not far from the maximum, so that this offers little help.)<sup>6</sup> The maximum number of questions asked of all respondents is arrived at by multiplying the maximum number of respondents by ( $n - 1$ ). This number increases with  $n$ , but the decline in the total number of respondents sampled is far more rapid. Since so much of an interviewer's time consists of finding respondents, the strategy of taking many small samples would therefore be much less practical than that of taking a lesser number of larger ones.<sup>7</sup>

<sup>6</sup> This calculation results from formulas developed by Paul Holland and Stanley Wasserman.

<sup>7</sup> I am indebted to James Beniger for pointing out errors in an earlier draft of this paragraph and of table 2.

More important, at a theoretical level, the larger the sample, the more properties of the population network can be estimated. In pair sampling, for instance, nothing *but* density could be estimated, since the subgraphs sampled have no structural properties other than the 1-0, yes-no question about the one potential tie. In a single, large random subgraph, by contrast, nearly all relevant properties appear. On the question of estimating the *full distribution* of acquaintance volumes, for example, Frank shows that the problem is simplified (although it is still not solved) if the sample size is "so large that all the frequencies that it is intended to estimate will have a chance of being represented in the sample graph" (1971, p. 114).

For both theoretical and practical reasons, then, I believe that network sampling must go in the direction of a few large samples rather than many small ones.

#### ONE-WAY QUESTIONING<sup>8</sup>

The above discussion is conservative, in that it assumes a method in which each possible tie is asked about from both ends—that is, each of the  $n$  sampled individuals is asked about each *other* one, so that  $i$  is asked about  $j$  as well as  $j$  about  $i$ . This means that we are asking  $[n(n-1)]$  questions to find out about  $[n(n-1)]/2$  ties. In most cases we will want to do this, either because we are interested in the symmetry or asymmetry of the sociometric choices or because we have more confidence in the validity of the information when both ends of a tie affirm its existence. If we are not interested in symmetry, however, and are willing to assume that a tie exists whenever one participant says it does, the sampling work can be cut in half. It is easy to arrange for each of the  $n$  sampled individuals to be asked, not about *each* of the  $n-1$  others, but about roughly  $(n-1)/2$  others; information is still obtained about each possible tie. Where, for example, 500 people are sampled, table 3 shows one simple scheme for using this method.

Such a procedure might be especially reasonable in obtaining acquaintance volume estimates for large cities. Whereas in a sample of 1,000 it might not be practical to ask each member questions about 999 others, questions about 500 others could probably be managed.

#### SOME APPLICATIONS

The applications that follow are illustrative only; they are far from exhaustive.

<sup>8</sup> This section was suggested by comments of Harry Collins.

TABLE 3  
SOCIOMETRIC QUESTIONS FOR 500 RESPONDENTS UNDER ONE-WAY QUESTIONING

Respondent No.	Is Asked about Respondents No.
1 .....	2-251
2 .....	3-252
3 .....	4-253
· .....	·
· .....	·
· .....	·
250 .....	251-500
251 .....	252-500
252 .....	253-500, 1
253 .....	254-500, 1-2
· .....	·
· .....	·
· .....	·
498 .....	499-500, 1-247
499 .....	500, 1-248
500 .....	1-249

NOTE.—Since  $n - 1$  is an odd number, each respondent cannot be asked *exactly*  $(n - 1)/2$  questions: half are asked  $(n/2)$  and half  $(n - 2)/2$  since  $n/2[(n/2) + (n - 2)/2] = [n(n - 1)]/2$ .

1. *Sense of community*.—A central focus of community studies has long been the question of what determines whether residents feel a "sense of community" where they live (e.g., Nisbet 1953; Stein 1960). While this concept is ambiguous and involves many intangibles, a central part of all analysts' notion of "sense of community" is the existence of a relatively dense network of social ties over the specified area. Ethnographic studies make it clear that a crucial part of the sense of "belonging" in a place is the constant encounter with familiar, friendly faces in the course of everyday life. Young and Willmott (1962) found, after analyzing the close-knit Bethnal Green area of East London, that residents who had moved to "Greenleigh," a government-sponsored suburban new town, were extremely unhappy. Their number of social contacts fell precipitously, and they consequently experienced the general atmosphere as cold and "unfriendly." Young and Willmott explained the contrast by the differing ecology of the two areas—the city being more densely packed—and intermixing residential and commercial functions. This arrangement facilitates sociability, whereas the suburban one makes it difficult and artificial (cf. Jacobs 1961, chaps. 1-12).

But Willmott later studied Dagenham, a community very similar to Greenleigh except that it had been settled in the 1920s. He was surprised, after the study of Greenleigh, to see the extent to which Dagenham's residents had reestablished a sense of community more typical of urban

East London; most people felt the atmosphere to be friendly, and considerable visiting was reported (Willmott 1963, pp. 58-64). His conclusion is that earlier studies, such as that of Greenleigh, "have put altogether too little emphasis on sheer length of residence" (p. 111). The implication is that the six or seven years of Greenleigh's existence, although it seemed a substantial time to the authors, was actually brief in relation to the time required to build up dense social networks from scratch. But we are left guessing about how "sheer length of residence" has its effect. Other studies of new towns, or suburbs where a substantial influx arrives at one time, report far more cheerful social results than those found in Greenleigh (see Berger 1960; Gans 1967). What explains these differences? What is the process by which a sense of community develops, and what determines the requisite time?

The sampling method outlined in this paper offers a useful research tool for these issues. A time series of average acquaintance volume in a new town, beginning early and continuing for a number of years, would, in conjunction with ethnographic work, yield important insights. The statistical comparisons made possible by the sampling method would allow interesting questions to be posed which could not be answered by a single case study. For example: how important is *initial* acquaintance volume in determining the rate of community development? It may be that new towns in which there exist, at the outset, a substantial number of interpersonal ties are successful far more quickly than others. The initial ties may serve a pump-priming function—satisfying people's interim need for sociability and smoothing the way for new ties—since existing friends are a crucial source of new ones. Initial ties may come about in a variety of ways; for example, people may be more likely to move to a new town if they already know someone who lives there (see MacDonald and MacDonald [1964] on "chain migration"), or economic factors, such as plant relocations, may prompt a considerable migration from one place to another (see Berger 1960). Substantive questions such as that of the source of initial ties can easily be incorporated into a survey using the sampling method proposed here. When a respondent is given the stimulus of another name from the sample, and the response indicates some relationship, a variety of questions about the relationship—its origin, intensity, duration—can be asked, depending on the nature of the inquiry.

In this case, a comparative study could relate initial network density to the subsequent rate of increase. Different patterns of increase or decrease might be found to correlate with changes in a community's political or economic structure. In this connection, measures of average acquaintance volume for any communities, old or new, might be of great potential value as social indicators. Use of them would extend the recent wave of



interest in such indicators to interactional measures. Most indicators currently in use consist, by contrast, of individual characteristics (happiness, income, illness) aggregated over large numbers.

2. *Hierarchy*.—Recent theoretical work on acquaintance networks makes a good argument for the idea that unreciprocated sociometric choices indicate a status differential, the chooser occupying a lower status (Davis and Leinhardt 1972; Holland and Leinhardt 1971; Bernard 1974). Investigations thus far have been limited to groups of a couple of hundred, given data-processing difficulties. The sampling device discussed here offers an entrée into this question for larger populations. My statistical analysis is unchanged if the tie in question is unreciprocated instead of symmetric. Estimates for the density of both types are yielded by a single sample as follows: From the sample sociomatrix, construct two submatrices, one containing only the symmetric, the other only the asymmetric ties. Use each submatrix to arrive at a density estimate for its type of tie.<sup>9</sup> The ratio of the asymmetric density to the overall density might be an interesting measure of the degree of dyadic hierarchy in the group and a useful parameter for comparison of groups. For a single group, a time series of the measure would offer some insight into the evolution, stability, or disintegration of hierarchy. (A fuller treatment of hierarchy would require methods for sampling the average properties not only of dyads, as in this paper, but also of triads, which I do not deal with here.)

3. *Interorganizational networks*.—Networks whose nodes are organizations have recently generated increasing interest. Often, however, a network comprises too many organizations for any study to be feasible. In a given industry, say electronics, it might be of interest to know the extent to which firms interchange personnel. (In Granovetter 1974 I discuss the substantive significance of this question.) The degree of such interchange might be a nice parameter in comparison of different industries. All that would be needed to apply the findings of the present paper is adoption of some minimum level of personnel flow between a pair of companies as constituting a "tie" between them. A square sociometric matrix for the set of firms could then be filled in with the usual 1-0 entries. Insofar as flow were asymmetric, it might be possible to infer hierarchy, making this application a special case of the one suggested above.

## DISCUSSION

In this paper I have argued that sociologists interested in the idea of social networks must attend to the development of a related theory of sam-

<sup>9</sup> But notice that dividing ties found into *any* set of categories requires a larger sample size, the increase to be determined by the category of tie with lowest (assumed) true density. Otherwise, estimates for the various categories will carry errors larger than would be tolerable when all ties are considered identical in quality.

pling, if they are to incorporate macrolevel concerns into the framework. Building on results in Frank (1971), I have shown that a relatively simple sampling procedure will yield good estimates of network density, or average acquaintance volume.

This is only a small step in network-sampling theory, however, since density is a crude, global measure of interaction structures. It is, in fact, the global aspect of density which makes it comparatively simple to estimate by a method which is both intuitively appealing and practicable. More detailed measures of network structure, especially measures of local variations and inhomogeneities in the network, are necessarily more difficult to estimate from small samples, since such samples, tapping only average properties of the entire graph, give poor representation to rare events. Moreover, when an estimating procedure is found which yields an unbiased estimate of such parameters, great computational and conceptual difficulty ensues in finding the variance of such estimates, without which no confidence limits can be established. Frank, for example (1971, pp. 109-15), develops formulas for estimating from the sample graph the *exact* distribution of acquaintances in the population. The unbiased estimator, however, no longer has the intuitive flavor that our density estimate has but depends instead on complex sets of equations. Moreover, the variance of the estimate "is of no practical use, because it involves unknown population parameters that seem hard to estimate" (Frank 1971, p. 112).

Nevertheless, the realization that some good results can be achieved without recourse to wholly new methods should be an incentive to those with good mathematical and statistical skills to push ahead in an area which promises considerable rewards for the time invested.

## APPENDIX

In this Appendix I prove that density estimates from random subgraphs provide an unbiased estimate of true population network density.

To prove that the estimate is unbiased, consider  $w$  sets of  $n$  individuals, each set a random sample from a population of size  $N$ , with replacement.

Let  $T_i$  be a random variable, the number of ties actually observed in sample  $i$ , where  $i = (1, 2 \dots w)$ . Let  $P_k$  = the probability (based on empirical relative frequencies) of observing  $k$  ties in such a set of size  $n$ , where  $k = 0, 1, 2, \dots [n(n-1)]/2$ ,  $\sum P_k = 1$ .

Then:

$$E(T_i) = \sum_{k=1}^{n(n-1)/2} kP_k. \quad (A1)$$

In the population graph, there are  $P_k \binom{N}{n}$  sets of  $n$  people with  $k$  ties in

the set. Since any given tie appears in  $\binom{N-2}{n-2}$  different sets of size  $n$ , the total number of ties in the population is:

$$\sum_{k=1}^{n(n-1)/2} kP_k \binom{N}{n} / \binom{N-2}{n-2}. \quad (\text{A2})$$

This quantity divided by  $\binom{N}{n}$  gives an expression for network density ( $D$ ). Then, substituting from (A1), we have:

$$D = E(T_i) / \binom{n}{2} = E \left[ T_i / \binom{n}{2} \right]. \quad (\text{A3})$$

Thus,  $T_i / \binom{n}{2}$ , which is the density observed in sample  $i$ , gives an unbiased estimate of the population density. Call this estimate  $\hat{D}_i$ . Now if  $w$  separate samples are taken, we have  $E(\hat{D}_1) = E(\hat{D}_2) = \dots E(\hat{D}_w) = D$ . Since  $E(\hat{D}_1 + \hat{D}_2 + \dots \hat{D}_w) = E(\hat{D}_1) + E(\hat{D}_2) + \dots E(\hat{D}_w)$ , it follows that

$$E[(\hat{D}_1 + \hat{D}_2 + \dots \hat{D}_w)/w] = wD/w = D, \quad (\text{A4})$$

that is, the average density estimate from the  $w$  samples also unbiasedly estimates population density.

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# Subjective Social Distance, Occupational Stratification, and Forms of Status and Class Consciousness: A Cross-national Replication and Extension<sup>1</sup>

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After briefly reviewing some general theoretical issues in analyzing systems of social inequality and stratification, we propose a typology of forms of class and status consciousness. A specific procedure employing subjective social distance scales is proposed as an empirical strategy for assessing different forms of status consciousness and exploring their implications for class consciousness and other political and social attitudes. To evaluate the empirical and theoretical utility of this strategy, we report a West German replication of an American study in which substantial evidence is found for a remarkable degree of cross-national similarity in the subjective social distance responses accorded occupations varying in prestige and socioeconomic status, regardless of the class position of the respondent. Some working- and middle-class persons did, indeed, prefer to interact with members of their own class rather than with persons in higher- (or lower-) status occupations; and this manifestation of corporate status consciousness appeared to be specifically linked to other political and social views consonant with such consciousness. But these were relatively minor, albeit systematic, departures from the general picture of prestige- or upward-oriented preferences for intimate interaction at all class levels—what we have called a competitive status consciousness that appeared to be pervasive among lower-status persons in both the American and the German communities studied. While the results can hardly be regarded as definitive, they help to clarify a number

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of issues in studying subjective consciousness of the class and status order and suggest the promise of further work employing the approach.

This paper has two parts. The first sketches some general theoretical and analytic considerations that we feel should be taken into account in any systematic study of class and status consciousness in modern systems of social stratification. While generality of treatment is sought, we intend it to provide the broader theoretical context for the second part, which describes in some detail a comparative study of subjective social distance reactions to occupations in an American community and a German one.

To avoid creating unwarranted expectations in the reader's mind, however, we should clarify the intended linkage between the two sections. The empirical work reported in the second section is the product of the senior author's continuing interest over the past 10 years in the study of subjective aspects of stratification. In the course of designing and executing the study and subsequently analyzing the data gathered in the two communities, however, we found that we had to come to grips with some broader theoretical issues that had been only implicit, ambiguously treated, or simply ignored in our own thinking and in the relevant literature on status and class consciousness. The first section is thus an attempt to sketch, in what we hope is not too brief a compass, some of the insights and conclusions derived from our empirical work and a reading of this literature. They have been used to construct the elements of a more comprehensive framework for examining the forms of status and class consciousness. Such a framework inevitably raises more questions than can be answered in the empirical section of the paper, from which, in some sense, it represents an extension. But we hope this framework will clarify the place of our and other researchers' findings on status and class consciousness in the emerging theory of modern status systems.

#### PART I. A THEORETICAL FRAMEWORK FOR EXAMINING FORMS OF STATUS AND CLASS CONSCIOUSNESS

Social theorists have long recognized, with varying clarity and emphasis, two fundamental issues in the study of social inequality and stratification. The first issue concerns how differentiation of social positions in a society, especially with respect to the division of labor, is related to social inequality—that is, the differential distribution of rewards, privileges, and social honor or deference among various positions.<sup>2</sup> Explanation of the

<sup>2</sup> Social inequalities may usefully be distinguished into two major categories: those having to do with the unequal distribution of access to socially valued scarce resources, such as material goods, money, and physical comforts like shelter, sexual partners,

linkage between social differentiation and social inequality has been of special concern both to theorists stressing the system-maintenance functions of social inequality, such as Davis and Moore (1945), and to theorists stressing its disintegrative or conflict-producing functions, such as Dahrendorf (1959, 1968) and Marx. Of course, the explanations advanced differ radically, although there is some agreement that inequalities are linked to social differentiation within and across societies in remarkably uniform patterns (cf. Lenski 1966).

The second issue concerns how social positions regarded (for whatever reasons) as socially equal or unequal are linked to one another, both objectively and subjectively, in characteristic patterns of social relationships. Less developed theoretically than the first issue (cf. Nadel 1957), this perspective on interactional bases of social structure has had two important variants. The first, most often adopted by observers following certain Marxian assumptions, stresses the potential or actual emergence of competitive or even antagonistic relations between classes of persons located in opposing positions in the economic order (cf. Bendix and Lipset 1966; Ollman 1968). The second variant emphasizes consensually grounded face-to-face relations such as marriage and friendship and the formation and cohesiveness of social classes or status groups—without specifying whether relations among such groups will be relatively antagonistic, indifferent, or cooperative (cf. Weber 1946; Warner 1960).

In the study of inequality and stratification, these two general issues pose fundamental questions from both objective and subjective standpoints. From the objective point of view, investigators have examined such questions as how scarce resources like wealth, income, and power are distributed in a society's population (e.g., Miller 1966; Hamilton 1965) and the extent to which persons occupying similar social positions choose others like themselves for intimate relationships, while avoiding those in more or in less favored positions (e.g., Ellis 1957; Curtis 1963; Laumann 1966, 1973). From the subjective perspective, investigators have explored the ways in which people evaluate social positions differentially in terms of their general social standing or prestige (e.g., Reiss 1961; Hodge, Siegel, and Rossi 1966; Goldthorpe and Hope 1972) and the ways in which and degrees to which individuals are conscious of their class and status positions vis-à-vis others (e.g., Centers 1949; Ossowski 1963;

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and services; and those having to do with the unequal distribution of favorable and unfavorable social evaluations, especially social honor, respect, and deference. Very crudely put, one may see social inequalities relating to scarce resources as a major basis for differentiation of a population into economic classes, while social inequalities pertaining to differential social evaluations of worth are the bases for differentiation of a population into status groups. Lipset (1955), for example, implies such a distinction in his treatment of class vs. status politics (see also Barber [1957]; and the introductory chap. sections in Laumann et al. [1970]).

Landecker 1963; Leggett 1963, 1964; Bogardus 1959). Since Marx's brilliant exposition of the problems of transforming a *Klasse an sich* into a *Klasse für sich*, however, it has been widely recognized that the problems which must be treated in studying the objective differentiation of a society are analytically separable from those involved in studying the subjective awareness of class or status location and the implications of such awareness for social action. Admittedly, one must ultimately treat the interrelationships of the two sets of problems in order to construct a comprehensive explanation of a society's system of stratification.

Obviously we cannot discuss in detail here all the issues summarized above; they have been raised to place our argument in its broader context. Confining our attention to a preliminary theoretical and empirical analysis of consciousness of social position as it relates to interactional preferences in two culturally distinct urban settings, we shall be primarily concerned with the second set of problems.

Some 10 years ago, the senior author initiated a series of studies (see Laumann 1965, 1966, 1973) exploring the manifold ways in which a person's intimate associates are chosen with regard to his own and their differing amounts of social standing, prestige, or honor. Basic to the study design are two key assumptions derived from Weber's discussion (1946) of economic class and status groups (cf. Lipset 1968). First, a person's occupation is taken to be a crucial determinant of both subjective and objective aspects of his relative social standing or prestige in modern industrial society. (Admittedly, there are other important bases of social honor such as ethnic or racial group membership; the assumption does not deny their significance.) Underlying the assumption are the repeated findings that occupations are remarkably similarly evaluated in terms of relative social standing by different population subgroups within the same society and over time (cf. Reiss 1961; Hodge, Siegel, and Rossi 1966) as well as across societies (e.g., Hodge, Treiman, and Rossi 1966). (The particular explanation one adopts to account for this striking uniformity of occupational prestige evaluations across societies will have no bearing on the analysis to follow.) The second assumption is that the amount of social stratification in a society can be defined usefully as the variable degree to which persons of approximately equal social status choose one another for intimate relationships such as marriage and friendship, beyond what would be expected on the basis of chance factors alone (cf. Warner 1960; Laumann 1966, pp. 2-17; Zeitlin 1974, pp. 1108-12). In this view, a social class, defined as a grouping of people who interact selectively according to status characteristics they share or believe they share, is at one end of a hypothetical continuum. The other end of the continuum may be termed a social stratum or aggregate and is constituted of persons who share a status characteristic but typically disregard it in forming intimate



relations. (This assumption clearly differs from an important alternative approach to defining social stratification, which emphasizes the greater or lesser transmission of status differences from one generation to the next [cf. Blau and Duncan 1967; Treiman 1970]. This alternative perspective leads to a set of concerns quite different from, though not unrelated to, those considered here.)

These assumptions raise two critical questions. First, what are the actual patterns of intimate choice among various occupational strata, that is, what has been termed "differential association"? In other words, How socially exclusive or open are incumbents of given occupational categories to incumbents of others? The more self-selective an occupational category is in the choice of marriage partners or friends, the more it approximates the "social class" pole of the hypothetical continuum mentioned above. The second question is: What are the subjective preferences of individuals for intimate relations with "acceptable" or "ideal" partners, when the individuals and the potential partners are distinguished by some single status characteristic, such as occupation or ethnicity?

Answers to these questions need not be identical or even complementary, because the causal processes that determine the patterns of differential association are different from those underlying subjective preferences for intimate interaction. To elaborate, differential association arises as a result of physical and social opportunity, mutual attractiveness, and reciprocity of choice. By contrast, subjective preferences are essentially projective in nature; they can express aspirations that lack any socially imposed restraints, not the least of which is the social acceptability of the aspiring partner to the one chosen. It is precisely this projective feature of subjective preferences that first suggested to us the possibility of using such preferences as a means of assessing the character and level of a person's status consciousness. The senior author and others have previously examined the patterns of differential association by occupation in modern urban communities (see Ellis 1957; Curtis 1963; Laumann 1966, 1973; Reuband 1974). Here we confine attention to the comparative analysis of subjective preferences for intimate interaction. Specifically, we examine subjective social distance attitudes toward occupations varying in relative prestige. These attitudes are defined as the varying subjective willingness or reluctance to enter into intimate relationships with persons possessing specified status characteristics. We give special attention to discerning the patterns of consciousness people have about the occupational prestige hierarchy. Two research sites, one in Germany and one in the United States, allow a comparative analysis.

Before proceeding to a description of our procedures and results, it will be useful to consider a typology of forms of consciousness of social inequality and stratification. In reviewing the literature, we identified a

variety of approaches to the empirical study of class consciousness. They range from minimal assumptions about the levels of consciousness required of respondents for answering close-ended survey questions on subjective class identification (cf. Centers 1949; Hodge and Treiman 1968; Jackman and Jackman 1973) to highly sophisticated definitions, often based on putative Marxian assumptions, which recognize gradations of class consciousness from the nonexistent to "false consciousness" to well-developed "corporate class consciousness." Depending on the investigator, some or all of the following attitudes must be present for an individual to be fully class conscious. These attitudes include perceptions by the individual that: (1) his life chances depend on his group rather than on his personal resources; (2) his position is structurally, rather than individually, determined; (3) classes ideally are cohesive and self-conscious entities; (4) the criteria of placement in a class rest exclusively on economic considerations; (5) interclass relationships are necessarily mutually antagonistic or hostile; (6) the class structure is essentially dichotomous or at most trichotomous; and (7) the class structure is neither desirable nor inevitable (see Landecker 1963; Ossowski 1963; Manis and Meltzer 1963, p. 177; Leggett 1963, 1964; Ollman 1968; Portes 1971; Hurst 1972; Giddens 1973). MacIver and Page (1949, pp. 358-64; cf. Westie 1959; Nisbet 1959) propose a useful distinction between corporate and competitive class consciousness. The former corresponds roughly to the first three characteristic attitudes listed above, while Marxian usage would add some, if not all, of the other four. "Competitive class consciousness," on the other hand, refers to an individual's acute awareness of the inequalities in socioeconomic status in his society combined with his beliefs that his position in the status or class order is determined by his personal effort and resources in competition with other individuals in the society, and that it is possible and legitimate to rise or fall in the hierarchy as a result of such personal factors.

Other discussions of class consciousness make evident the need to distinguish between economically and honorifically defined categories of social positions (see esp. Morris and Murphy 1966; Giddens 1973). For example, in Warner and his students' treatment of social class consciousness in small-town America, class consciousness undergoes a not-so-subtle shift into status consciousness. This can be seen in the social exclusiveness displayed by members of the upper-upper class toward members of the parvenu lower-upper class. The latter cannot be distinguished from the former by economic class criteria but are quite distinct in terms of the number of generations their families have been in the upper bourgeoisie and in terms of other ascriptive characteristics such as ethnic origin or personal behavior. (Indeed, discussions of an individual's identification with an ethnic or racial group and of the relative cohesiveness of these

groups usually imply a form of status consciousness very similar to that implicit in Warner's treatment of social class consciousness [cf. Gordon 1964; Borhek 1970].)

Two crosscutting dimensions may thus be detected in these discussions of class and status consciousness. The first is the classic distinction Weber originally drew between economic class and status group; the second relates to the unit of reference in the individual's orientation to the class or status order. For example, the individual may take himself and perhaps his immediate circle of family and friends, that is, his primary group, as the point of reference in his subjective contemplation of the class or status order. Alternatively, he may regard a secondary group, that is, a more inclusive category of persons sharing a common attribute, many of whom he does not know or expect to know, as the primary referent for locating himself subjectively in the class or status order.

If one assumes that persons implicated in a given hierarchy of social inequality tend to make invidious, zero-sum comparisons among the ranked positions, the individual's unit of reference to that unidimensional order takes on special significance. On the one hand, if he adopts himself and possibly his primary circle as the principal point of reference, he is likely to be positively oriented up the hierarchy of status positions (or laterally if he is already at the top) and negatively oriented to lower-status positions, in order to maximize his personal honorific gain and minimize his loss of honor (exhibiting what we shall call competitive consciousness). On the other hand, if an individual takes his secondary group (which, it is important to stress, is his *own* stratum) as the principal point of reference to the class or status order, he is likely to accept the notion of collective status-group or class maintenance of honorific standing or economic reward and/or its improvement, if necessary at the expense of other groups (what we shall call corporate consciousness). Gains in honor and/or reward are to be realized by gains in the honor or reward awarded one's entire stratum (e.g., through union-linked parties, participatory management, or professionalization of an occupation).

We can summarize our discussion to this point in a fourfold table which cross-tabulates the two dimensions mentioned above. The empirical literature clearly suggests that the four types of consciousness indicated in table 1 have quite different implications for an individual's status- or class-linked behavior and attitudes. These include orientations to social mobility for the individual and his children or for his group, political preferences and behavior, activity in unions and other voluntary associations, and choices of persons with whom to spend leisure time (cf. Laumann 1966, pp. 105-32; 1973, pp. 73-110). Although the four forms of consciousness are analytically distinguishable, they are not mutually

## Forms of Status and Class Consciousness

TABLE 1

DIFFERENT TYPES OF CONSCIOUSNESS OF SOCIAL INEQUALITY AND STRATIFICATION

	UNIT OF SUBJECTIVE REFERENCE OR IDENTIFICATION	
	Individual and/or His Primary Circle of Intimate Associates	Secondary Group, Stratum Sharing Common Attributes
Economic class .....	Competitive class consciousness	Corporate class consciousness
Status group .....	Competitive status consciousness (e.g., in a celebrity system, meritocratic educational system, losing or gaining "face")	Corporate status consciousness (e.g., in a socially exclusive group, caste honor)

exclusive in actuality because individuals usually hold simultaneous memberships in different status and economic groups (cf. Wiley 1967).

In addition, individuals will, in fact, differ considerably in the degree to which they are even minimally aware of given class or status distinctions and the extent to which such distinctions play any role whatsoever in their subjective orientations to their social world. That is, there may be many people in a society of whom it makes little sense to say that they possess any of the four forms of consciousness. More formally put, we may say that there is implicitly a third crosscutting dimension to table 1, orthogonal to the others, that refers to the relative presence or absence of each of the four types of consciousness. The identification and explanation of why some persons in a society lack class or status consciousness in any or some of its forms, while others have it to an exceptional degree, is critical to providing a satisfactory empirical account of a given stratification system.

But what can one say about the interrelationships to be expected among these various forms of consciousness? This question opens up a whole range of important issues. We shall restrict ourselves to a few preliminary observations. The several forms of consciousness are especially likely to become mutually relevant and intertwined in the subjective orientations of individuals when the defining characteristics of membership in a particular status group approximate the defining characteristics of a given economic class grouping (cf. Giddens 1973). Consider, for example, manual workers: economic class position and shared status honor may converge in the minds of some workers in such a way that they not only prefer subjectively to interact on intimate matters with their "own kind" and to reject intimacy with status superiors but also have come to perceive their personal economic well-being as closely tied to the welfare of their class as a whole even if it must be at the expense of other classes. In such situations, corporate status consciousness may even be seen to be a neces-

sary, though not sufficient, condition for the emergence of corporate class consciousness (which, as we have noted, in the Marxian version implies the recognition of interests antagonistic to those of other groups). On the other hand, it is difficult to imagine a worker with competitive status consciousness of other occupational groups—in this case, a positive orientation to associating with status superiors—expressing at the same time a strong corporate class consciousness. If there is no apparent link between membership in a given status group and membership in a given class, there may be no constraint exerted on the forms of class and status consciousness adopted. One might hazard the hypothesis, however, that strong corporate status consciousness (for example, strong identification with an ethnic group whose members are distributed across economic classes) might impede corporate class consciousness in the individual (cf. Marx and Engels 1953, 1971).

Devising appropriate instruments to measure such complex and multifaceted attitudes as status and class consciousness is very difficult, as the literature richly testifies. Not only are there many possible aspects of the subjective orientations that might be relevant, but also explicit questions about consciousness are difficult to formulate and may even be theoretically inadvisable. Much criticism has been directed against the conventional questions on subjective class identification and other relatively direct questions on the grounds that they may simply be creating an attitude in the mind of the respondent where none existed before. Many have argued the need for more spontaneous, indirect assessments of the presence and character of the consciousness (cf. Leggett 1963, 1964; Manis and Meltzer 1963). While accepting the force of these objections and by no means claiming we have fashioned the perfect indicator, we have found that our experience with subjective social distance scales (to be described below) in three countries—the United States, Germany, and England (see Laumann 1966; Reuband 1974; Taylor and Taylor 1971, 1972)—suggests their utility as indicators of some of the hypothetical constructs we have in mind.

We turn in Part II to an empirical study that speaks to at least some of the theoretical issues sketched in this section. To be sure, the original motivation for undertaking the comparative study had nothing to do with the typology of forms of status and class consciousness. On the contrary, the typology was formulated in response to our need to provide an interpretive framework for some of our empirical results. While we hope to demonstrate the utility of this interpretive framework in clarifying some of our results, we also consider other important results of less direct relevance to it.

We direct particular attention to an instrument that we believe provides,

as well as a number of other important uses, a satisfactory means of tapping the presence of corporate versus competitive status consciousness with respect to the occupational prestige hierarchy. Although we also have something to say about corporate versus competitive class consciousness, we have been much less successful in identifying satisfactory indicators of these hypothetical constructs, probably because we did not fully appreciate their importance when designing the original study.

PART II. A COMPARATIVE INQUIRY INTO SUBJECTIVE SOCIAL DISTANCE REACTIONS TO OCCUPATIONS

In Laumann's examination (1965, 1966) of the subjective social distance attitudes of a sample of white men in Cambridge and Belmont, Massachusetts, he argued that there are two rival hypotheses to describe the patterning of an individual's and a social category's social distance reactions to occupations varying in social standing. The "prestige" hypothesis holds that, regardless of his status location, a person would prefer to associate intimately with those whose socioeconomic status is superior to his and to avoid associating with those of lower socioeconomic status. In contrast, the "like-me" hypothesis asserts that a person prefers to associate with those of approximately equal social status and to avoid intimate association with those of substantially superior and/or inferior status (cf. Riecken and Homans 1954; King 1961). Laumann found an overwhelming tendency of social distance reactions to follow negatively the prestige gradient of occupations. Of course, whether this is in accord with one or the other hypothesis depends on the person's own class or status location. Upper-class individuals, for example, will express a strong "prestige" effect in their reactions to various occupations by responding in accord with either hypothesis.

This earlier formulation of the prestige and like-me hypotheses can easily be seen to correspond rather closely to the two forms of status consciousness discussed in the preceding section. Except for members of top-prestige occupations where it is simply indeterminate, it is reasonable to interpret a prestige-seeking orientation to associating with persons in occupations of status higher than one's own as indicative of a competitive status consciousness. Similarly, a like-me orientation appears to correspond rather neatly to a corporate status consciousness. Both interpretations are predicated on the assumption that occupations do form at least protostatus groups, so that social distance responses pertain more to honorific, prestige, or style-of-life differences among occupations than to more narrowly conceived differences in economic interests or economic-life chances.

In this section, we describe the recent replication of the American study in a German community and a systematic comparison of several key results. We begin by describing briefly the two research sites and the instrument used to measure subjective social distance attitudes toward occupations. Then, following as closely as possible the original strategy for analyzing aggregate response patterns, we compare the United States and German results to show the general sensitivity of the technique to similarities and differences between American and German respondents and different class groupings within each community. In an individual-level analysis, we propose a way of identifying individual propensities to competitive or corporate status consciousness by examining each respondent's overall pattern of subjective social distance responses. The individual-level patterns are then correlated with other attitudes held by the respondents, partly in order to validate the measure and partly to suggest some of the presumed concomitants or effects of varying kinds of status consciousness. If all goes well in the empirical analysis, there is encouragement to press on in the present vein and attempt to formulate more adequate measures of class consciousness and explore their interrelations.

#### Data Base: The American and German Communities Compared

The initial study, completed in 1963, was based on a stratified, area-probability sample of 422 white male residents of Cambridge and Belmont. The subsequent study was designed to replicate this work and to extend the analysis. In 1971, Pappi and Laumann conducted a survey in Altneustadt (a pseudonym), a town of 20,000 people in West Germany. The town is more cosmopolitan than its size suggests: it is a government center; it has a varied, substantial industrial base; and about 15 years ago a large natural-science research facility employing over 3,600 people located there. It should be added that Altneustadt's population is disproportionately middle class and does not fully display the extremes in social status that would occur in a national sample of West Germany's population (see Pappi 1973; and Laumann and Pappi [1973, 1976] for a more complete description of Altneustadt and a comparison of it with other communities in West Germany). In comparison with the Cambridge-Belmont population sampled in 1963, the Altneustadt population includes a greater proportion of persons who could be characterized as having middle-class status and lower proportions of persons of either working- or upper-class status. A total of 820 interviews was garnered from a representative sample of the voting population, including men and women, aged 18 years or over. (See the methodological Appendix for a more detailed consideration of the sample and response rates as well as the scaling properties of the social distance scale.)

## Measurement Procedure

To measure subjective social distance, Laumann modified a version of a scale initially developed by Bogardus (1925, 1933) to study prejudice against ethnic groups and subsequently revised by others (Westie and Westie 1957; Westie and Howard 1954; Westie 1959; Martin 1963) to study social distance among occupational groups (see also Levine et al. 1976a, 1976b). Laumann's version for the U.S. study follows:

I believe I would like to have a *carpenter*:

a) as my son-in-law	SA	A	U	D	SD
b) as my father-in-law	SA	A	U	D	SD
c) as my closest personal friend	SA	A	U	D	SD
d) as a person I would have over for supper in my home	SA	A	U	D	SD
e) as a person I might often visit with	SA	A	U	D	SD
f) as a member of one of my social clubs, lodges, or informal social groups	SA	A	U	D	SD
g) as my next-door neighbor	SA	A	U	D	SD

This battery of items was repeated for each of 16 other occupations. Each of the seven Likert-type scales is intended to tap a relationship that affords the respondent intimate social access to someone in a particular occupation. Since there are well over 20,000 different occupations in the United States today, the set of 17 used in the booklet (see table 2) was primarily intended to include ones that are widely known and vary widely in socioeconomic status. They can in no sense be regarded as a "representative sample" of occupational titles.

For each occupation, each Likert scale was scored from 1 for "strongly agree" (SA) to 5 for "strongly disagree" (SD). Each respondent's set of seven Likert scores for a given occupation was then summed to a single score, ranging in value from 7 to 35, yielding 17 summary scores. Any differences in a respondent's reaction to an occupation that depended on which relationship he would hypothetically have with an incumbent of that occupation were ignored by this procedure. This procedure did, however, permit comparison of a person's social distance reactions among the 17 occupational stimuli; that is, we could see, by ranking the scores, which occupations the individual felt relatively close to and which he felt quite distant from.

Yet it was quite possible that the *range* of the 17 scores would differ considerably among respondents. One respondent's scores, for example, might range between 7 and 15, while another's might fall between 20 and 35. These distinctive response ranges might well reflect real differences among the respondents; that is, some might feel "close to" all 17 occupations, while others might feel more "distant from" all of them. But for our analytic purposes, we were more interested in being able to compare



TABLE 2

MEAN SOCIAL DISTANCE STANDARDIZED SCORES BY SELF-IDENTIFIED SOCIAL CLASS FOR CAMBRIDGE-BELMONT AND  
ALTEUSTADT SAMPLES, MEN ONLY

SUBJECTIVELY IDENTIFIED SOCIAL CLASS (SCHICHT)*												KRUSKAL- WALLIS TEST†	
TARGET OCCUPATION	Upper (1)	Ober- (2)	Upper Middle (3)	Ober- Tail (4)	Middle (5)	Mittel- (6)	Upper Working (7)	Arbeiter- (8)	Working (9)	TOTAL SAMPLE (10)	RANK (11)	RANGE (12)	
Physician (C-B) .....	378	...	378	...	398	...	491	...	420	401	1	113	.001
Doctor (A) .....	...	384	...	405	...	419	...	419	...	413	1	35	N.S.
Electrical engineer (C-B) .....	435	...	422	...	433	...	429	...	437	428	2	15	N.S.
Diplom physidist (A) .....	...	394	...	416	...	447	...	503	...	447	2	109	.001
College professor (C-B) .....	383	...	407	...	443	...	446	...	457	435	3	74	.001
University professor (A) .....	...	439	...	462	...	496	...	542	...	494	10	103	.001
Top executive (C-B) .....	412	...	419	...	431	...	438	...	454	435	4	42	.01
Top industrial executive (A) .....	...	394	...	440	...	466	...	490	...	459	5	96	.003
High school teacher (C-B) .....	416	...	433	...	490	...	462	...	451	446	5	46	N.S.
Primary teacher (A) .....	...	485	...	460	...	441	...	468	...	457	3	44	N.S.
Banker (C-B) .....	432	...	425	...	457	...	462	...	480	455	6	55	.001
Bank executive (A) .....	...	425	...	479	...	495	...	515	...	487	9	90	.008
Owner-manager, small store (C-B) .....	464	...	476	...	472	...	452	...	481	469	7	29	N.S.
Small grocery owner (A) .....	...	508	...	482	...	473	...	437	...	472	7	71	.004
Bank teller (C-B) .....	509	...	500	...	486	...	490	...	489	487	8	23	N.S.
Bank teller (A) .....	...	495	...	485	...	481	...	476	...	483	8	19	N.S.
Carpenter (C-B) .....	504	...	506	...	483	...	502	...	494	491	9	23	N.S.
Carpenter (A) .....	...	541	...	529	...	503	...	497	...	509	13	44	.02
Foreman (C-B) .....	500	...	492	...	503	...	515	...	509	502	10	23	N.S.
Industrial foreman (A) .....	...	534	...	532	...	500	...	484	...	503	11	50	.007

TABLE 2 (Continued)

SUBJECTIVELY IDENTIFIED SOCIAL CLASS (SCHICHT)*													KRUSKAL-WALLIS TEST† (13)
TARGET OCCUPATION	Upper (1)	Ober- (2)	Upper Middle (3)	Obere Teil (4)	Middle (5)	Mittel- (6)	Upper Working (7)	Arbeiter- (8)	Working (9)	TOTAL SAMPLE (10)	RANK (11)	RANGE (12)	
Garage mechanic (C-B) .....	543	...	543	...	518	...	487	...	503	510	11	56	.001
Auto mechanic (A) .....	...	497	...	464	...	452	...	447	...	459	5	50	.05
Machine operator (C-B) .....	556	...	541	...	530	...	529	...	511	522	12	45	.01
Skilled factory worker (A) .....	...	584	...	593	...	575	...	531	...	569	16	62	.002
Salesclerk (C-B) .....	550	...	540	...	540	...	529	...	525	530	13	25	.05
Salesclerk (A) .....	...	546	...	515	...	502	...	529	...	516	14	44	.02
Truck driver (C-B) .....	558	...	559	...	535	...	530	...	522	530	14	37	.01
Truck driver (A) .....	...	563	...	556	...	559	...	540	...	555	15	23	N.S.
Unskilled construction worker (C-B) .....	602	...	592	...	594	...	587	...	573	581	15	29	N.S.
Unskilled construction worker (A) .....	...	591	...	610	...	588	...	573	...	589	17	37	N.S.
Janitor (C-B) .....	611	...	596	...	591	...	578	...	579	584	16	33	N.S.
(No comparable occupation) (A) .....	...	...	...	...	...	...	...	...	...	...	...	...	...
Streetsweeper (C-B) .....	636	...	662	...	628	...	627	...	606	622	17	56	N.S.
Streetsweeper (A) .....	...	629	...	646	...	631	...	599	...	625	18	47	.05
(No comparable occupation) (C-B) .....	...	...	...	...	...	...	...	...	...	...	...	...	...
Draftsman (A) .....	...	482	...	454	...	456	...	448	...	459	5	34	N.S.
(No comparable occupation) (C-B) .....	...	...	...	...	...	...	...	...	...	...	...	...	...
Registrar (civil servant) (A) .....	...	510	...	473	...	512	...	502	...	504	12	39	N.S.
Total sample (C-B) .....	(25)	...	(50)	...	(105)	...	(35)	...	(112)	(327)	...	...	...
(A) .....	...	(27)	...	(30)	...	(90)	...	(45)	...	(192)	...	...	...

TABLE 2 (Continued)

SOURCE.—Cambridge-Belmont (C-B) means are from Laumann (1965), table 1, p. 31.  
 \* Subjective class identification for the American sample was determined according to responses to Centery's (1949) question asking the respondent to place himself in one of four social classes: upper class, middle class, working class, or lower class. A follow-up question asked the respondent to indicate whether he regarded himself as an average member of the working (middle) class or in the upper part of the working (middle) class. The German version of these questions has been extensively used in various surveys of the German population with satisfactory results. In order to obtain sufficient numbers for analysis, we categorized people according to their self-identification with the *Oberschicht* (both upper and upper middle class [*Obermittelschicht*]), *Obere Teil von Mittelschicht* (upper part of the middle class), *Mittelschicht* (average part of the middle class), and *Unterschicht* (working class, including both upper part and average members). The German sample's columns have been interspersed among the American sample's to indicate those American classes most comparable to them. Since the German sample had to be more coarsely divided because of insufficient numbers in certain classes, the categories employed are not strictly equivalent to the American categories.  
 † The Kruskal-Wallis one-way analysis of variance by ranks tests whether  $k$  independent samples are from different populations (see Siegel 1956, pp. 184-93).

respondents or groups of respondents in terms of their relative social distance evaluations of the 17 occupations, irrespective of interindividual differences in response ranges. In order to facilitate such relative comparisons, we standardized each respondent's set of 17 scores by calculating its mean and standard deviation and then, from each score, subtracting the mean and dividing the difference by the standard deviation to yield a standardized score. In this way each respondent received a new set of 17 scores with a mean of 0 and a standard deviation of 1. In order to eliminate decimal points and negative numbers, each standardized score was multiplied by 100 and then 500 was added to it, so that every respondent now had a set of scores whose mean was 500 and whose standard deviation was 100 (see Laumann 1966, pp. 30-38; the methodological Appendix contains a more detailed discussion of the scale.)

## Findings

A. *Cross-national comparisons of subjective social distance by social class.*—Table 2 presents the standardized social distance mean scores of both Cambridge-Belmont and Altneustadt respondents, classified by their self-identification with social classes.<sup>3</sup> To facilitate comparison, we have included only the responses of Altneustadt men and interspersed the German data among the most comparable class groupings in the American data. The class groupings in the two communities are not always strictly comparable, first, because of the necessity of combining certain groupings in Altneustadt to obtain sufficient cases for analysis (see table 2,\*) and, second, because of possible differences in the social meanings of social classes in the two societies. A detailed inspection of the table entries reveals a truly remarkable correspondence in the relative social distance re-

<sup>3</sup> We used the answers to the well-known Centers (1949) question on subjective class identification to assign people to various locations in the socioeconomic status hierarchy. Although some people, including Centers himself, have taken this question to be a measure of a person's class consciousness, it should be obvious that it corresponds only weakly to our notion of class consciousness. We agree with the many critics of the measure in arguing that this question is better regarded as a rough and ready way of sorting people into their relative locations in the socioeconomic status hierarchy. To be completely consistent with our general theoretical argument, it might be argued that we should have used an objective measure of a person's socioeconomic status, such as one based on educational attainment, occupation, and/or income. But many studies (e.g., Centers 1949; Hodge and Treiman 1968) have shown moderate correlations between subjective class placement and objective measures of socioeconomic status. We did in fact analyze the U.S. sample data, categorized by the actual occupational status of respondents, as did Reuband (1974; see Appendix also) for the German sample, and found the results to be highly consistent with those reported here. Therefore, in order to facilitate comparison with Laumann's earlier published work, we decided to use subjective class placement in the present study as a satisfactory estimator of objective class location.

actions of our two samples to different occupations. Despite the obvious predominance of similarities in the two samples' social distance reactions, there are also some fascinating divergences between them that may provide suggestive clues to possible differences between the two communities in their perception of status differences among occupations and their preferred patterns of intimate interaction.

More specifically, we should first note that the rank-order correlation (Spearman's rho) between the two samples' average social distances for the 16 occupations common to both surveys is 0.86. Although quite high, the correlation is less than perfect, in large part because of two occupations, professor and mechanic, exhibiting cross-national inconsistencies which we shall discuss shortly.

In both samples, the classes clearly differ significantly among themselves with regard to the average social distance reactions accorded given occupations. Eight of the 17 occupations in the Cambridge-Belmont sample manifested significant differences in social distance response by class; 11 of the 18 occupations in the Altneustadt sample did so (according to the Kruskal-Wallis analysis of variance by ranks: see table 2, col. 13). These results suggest that a man's location in the class hierarchy does indeed affect differentially his occupational preferences for intimate interaction, perhaps following the like-me rule mentioned above. We explore this possibility in greater detail below. Comparatively speaking, we note that, despite the somewhat more homogeneous status composition of the Altneustadt sample, the range of the mean scores among the various social classes (reported in col. 12) is greater in Altneustadt than in Cambridge-Belmont (the average range for Cambridge-Belmont is 43.2 vs. 57.8 for Altneustadt—a difference which just failed to pass a statistical test at the .05 level of significance), suggesting that corporate status consciousness may be more of a factor in German responses to occupational prestige differences.

With regard to systematic differences between the two samples, we note that at every class level German respondents expressed a considerably greater social distance attitude toward university professors than did American respondents at comparable class levels, reflecting the very special position such persons occupy in German society (cf. Dahrendorf 1967), in which even upper class respondents regard them as a group apart. The title "university professor" is much more restrictively used in Germany, where only very senior and intellectually prominent academics are accorded that honorific title. Historically such persons held very high-level state civil service appointments, often with major administrative responsibilities. More recently, liberalizing changes have been moving the German system of higher education closer to the American model, where the title "professor" is much more loosely used. We might, therefore, ex-

pect the systematic discrepancies in social distance responses between the two societies to moderate in the years ahead.

A complementary explanation may be advanced to account for the systematic differences observed between the two samples with regard to garage mechanics, toward whom the German respondents at every class level expressed less social distance than did American respondents. In Germany various state licensing procedures require extended formal training, apprenticeship experience, and the passing of a state-administered examination before a person may be certified as an auto mechanic. In the United States, on the other hand, entry into the occupation is almost a matter of self-report: nearly any filling station attendant, regardless of competence, may claim to be a mechanic. (Note, for example, the recent agitation here to bring the auto-repair business under greater governmental supervision.) Consequently, the designation "mechanic" indicates in Germany an occupation, access to which is carefully regulated, which is much more skilled than in the United States and thus, presumably, has greater social attractiveness.

Parenthetically, we should note a more modest systematic discrepancy between the two samples in their social distance evaluations of carpenters. Again the problem of finding strictly comparable occupational titles arises. Germans distinguish much more finely among different kinds of carpenters, whose designations reflect different levels of training, skills, and certification. While *Zimmermann* is the generic term for carpenter, it tends to be used in describing less skilled workmen involved in house construction; other terms, for example, *Tischler* ("cabinetmaker"), are reserved for much more highly regarded and skilled specialties. Such systematic differences in responses of the two samples, for which plausible explanations may be derived from cultural and societal differences among occupations in modes of access and social regard, provide evidence, in our opinion, for the view that the social distance scale is a sensitive device for identifying meaningful cultural and societal differences and similarities in comparative research.

Even a casual inspection of each column's average social distance scores in table 2 suggests that there is a regular monotonic increase in average social distance scores as occupational prestige declines, regardless of class. In order to see how well the prestige of an occupation predicted subjective social distance for each social class, Laumann used Duncan's Index of Socioeconomic Status as a measure of occupational prestige (Duncan 1961) for the Cambridge-Belmont sample.<sup>4</sup> Five regressions, one for each

<sup>4</sup> Goldthorpe and Hope (1972) argue rather convincingly that the customary interpretation of the Duncan Index as a measure of prestige is problematic, contending that it would be better treated as a measure of socioeconomic status reflecting differing amounts of rewards and privileges associated with various occupations, but not

of the self-identified social classes, were performed. The unit of analysis was occupation: the independent variable was "occupational prestige" and the dependent variable was the mean social distance score a particular class gave the occupation. Thus, each regression equation was based on 17 cases, the number of occupations about which respondents were queried.

Subsequently, we calculated the four regression equations for the Altneustadt sample. To preserve comparability with the Cambridge-Belmont data, we again used mean scores based only on the information provided by male respondents. Table 3 presents the results. For several reasons

TABLE 3  
REGRESSION EQUATIONS OF OCCUPATIONAL PRESTIGE ON SUBJECTIVE SOCIAL DISTANCE,  
BY CLASS, FOR CAMBRIDGE-BELMONT AND ALTNEUSTADT SAMPLES, MEN ONLY

Class	Regression Equations	$r^*$
Cambridge-Belmont:		
Upper class .....	606.00 — 2.29x	— .93
Upper middle class .....	603.35 — 2.23x	— .93
Middle class .....	579.63 — 1.72x	— .88
Upper working class .....	565.58 — 1.34x	— .80
Working class .....	560.65 — 1.31x	— .85
Altneustadt:		
Oberschicht .....	606.75 — 2.13x	— .92
Obere Teil, Mittelschicht .....	589.75 — 1.79x	— .83
Mittelschicht .....	565.96 — 1.32x	— .73
Arbeiterschicht .....	535.18 — 0.70x	— .46

SOURCE.—For Cambridge-Belmont equations, Laumann (1965), p. 34.

NOTE.—As Prof. Leo Goodman, of the University of Chicago, has kindly brought to our attention concerning the original article, the relationship between the  $y$ -intercept  $a$  and the slope  $b$  is the same in each of these equations. The interpretation of the results can be based on either the  $a$ s or the  $b$ s. For each of the equations, we have the relationship  $b = (500 - a)/X$ , where  $X$  is the average of the Duncan scores for the occupations. (Note that the value of  $X$  is the same for each set of equations.) Usually the relationship between  $a$  and  $b$  is not exactly the same in different regression equations; but for these data this relationship is fixed by the method of analysis. Thus, the reader is warned not to regard the consistently decreasing values of  $a$  as added independent evidence (in addition to the consistently increasing values of  $b$ ) concerning the consistent pattern of the data.

\* Significant at the .05 level or better.

the regression equations are, of course, not strictly comparable: the Cambridge-Belmont regressions include mean scores for 17 occupations, while the Altneustadt regressions include mean scores for 18, some of which do not directly correspond to any on the Cambridge-Belmont list.

necessarily their differing levels of social deference and honor (Shils 1968). That is, there is no strict one-to-one correspondence between the amount of social and economic reward an occupation enjoys and the social deference accorded to it. Consider, e.g., the cases of the college professor and the top business executive. Indeed, one might even argue that our subjective social distance scale is more sensitive to prestige differences among occupations in Shils's sense and might, therefore, be a better proxy measure of occupational prestige. Although one's interpretation of the Duncan Index has implications for his ultimate theoretical handling of the results, the choice of interpretation does not materially affect the relevance of the regression analysis reported below.

Both sets of equations demonstrate that prestige is, in general, correlated with subjective social distance: the more prestigious an occupation (or the higher in socioeconomic reward), the less social distance is reported regarding persons in that occupation. Note, however, that the data required to support the like-me and the prestige hypotheses are identical for persons of high-class standing. Such persons could, for example, be motivated by either hypothesis in reporting little social distance regarding physicians and great social distance regarding truck drivers. It is for this reason that the equations for the lower social classes are critical: high negative correlations mean that such persons are acting according to the prestige hypothesis, while low negative or any positive correlations would imply that they are acting according to the like-me hypothesis or some other principle of attractiveness. The fairly sizable negative correlations for the lower social classes in both communities imply that prestige is an important determinant of subjective social distance even for these classes.

More specifically, we have tested the null hypotheses that the slopes ( $b$ 's) for the five classes in Cambridge-Belmont and those for the four classes in Altnestadt (considered separately) do not deviate significantly from their respective common slopes. On the basis of the analyses of covariance (Brownlee 1965), we can reject both null hypotheses ( $P < .01$ ). This result implies that the classes do differ among themselves in the degree to which their average social distance reactions relate to occupational differences in prestige or socioeconomic status. More important, they appear to differ in a systematic fashion.<sup>5</sup>

We must, of course, also raise the comparative question: Are there sig-

<sup>5</sup> Unfortunately, as Donald Treiman has correctly observed in a personal communication, the regular decline of the  $b$ s as we go down the class hierarchy cannot be taken as sufficient evidence in and of itself of the differing impact of the prestige and like-me effects as a function of class location. The observed results could be produced by a process in which each class gave exactly the same weight to prestige and like-me considerations. The reason for this is that the subjective classes differ in their average prestige level in such a way that subjective distance (the converse of like-me) is greatest for low-prestige occupations in the upper-class equation, thus increasing the slope, and is greatest for high-prestige occupations in the working-class equation, thus decreasing the slope. (Of course, the other classes fall between.) Consider the equation  $\hat{D} = a + b(P) + c(S)$ , computed over occupations, where  $\hat{D}$  is the social distance score,  $P$  is the prestige score for each occupation, and  $S$  is a measure of the distance between the mean prestige for a given class and the prestige for each occupation. By plugging in the appropriate values for selected occupations evaluated by selected classes, one can observe the movement of  $\hat{D}$ , even under the assumption that  $b$  and  $c$  remain constant for all classes. We have examined these data in a more elaborate regression analysis (not reported here) in order to assess the differing impact of prestige and like-me effects across the classes and found that our initial supposition was correct: the prestige effect in each class is stronger than the like-me effect. But to improve the clarity of our theoretical and empirical exposition, we have adopted a different strategy for sorting out these effects in the next section.



nificant differences in the slopes between classes at approximately the same level in the two communities' respective social hierarchies? Table 3 reveals remarkable similarities in the numerical values of the *b*'s (or *a*'s) for comparable class groupings. Analyses of covariances for pairings of comparable classes yielded the results shown in table 4. A nonsignificant

TABLE 4  
ANALYSIS OF COVARIANCES FOR PAIRINGS OF COMPARABLE CLASSES

CLASS PAIRINGS	TEST FOR EQUALITY	
	Variances	Slopes
Upper class vs. Oberschicht .....	N.S.	N.S.
Upper middle class vs. Oberschicht .....	N.S.	N.S.
Upper middle class vs. Obere Teil, Mittelschicht ....	N.S.	N.S.
Middle class vs. Mittelschicht .....	N.S.	N.S.
Upper working class vs. Arbeiterschicht .....	N.S.	.10 (one-tail)
Working class vs. Arbeiterschicht .....	N.S.	.10 (one-tail)

test for equality of variance is a precondition for testing for equality of slopes and, as can readily be seen, is met for every cross-national comparison. Only in the working-class comparisons do we find even weak evidence for significant differences between the two communities' corresponding classes with respect to the relationship between social distance and occupational prestige (or socioeconomic status). For the working-class comparisons there may in fact be a stronger like-me effect among Altneustadt working-class respondents (the slope is flatter) than among Cambridge-Belmont respondents, perhaps reflecting a higher level of corporate status consciousness in Germany, at least in the working class. But this must be seen in the context of an almost incredible degree of cross-national similarity in the way occupations are responded to in terms of differential attractiveness for social interaction.<sup>6</sup>

*B. Competitive versus corporate status consciousness.*—Resting on a comparative analysis of aggregated data, the preceding section has inferred the differential operation of prestige and like-me effects on individual-level responses to the social distance scale (or the presence of different forms

<sup>6</sup> Since the original American study appeared in 1965, Treiman (in press) has developed an occupational prestige scale especially designed to be used for international comparisons of occupational prestige and has demonstrated its superiority for many comparative purposes. The analysis above employed the Duncan Index in both communities to insure comparability so far as possible with already published work. We did, however, replicate the entire regression analysis using Treiman's Standard International Occupational Prestige Scale, instead of the Duncan Index, in order to see whether our conclusions would be in any way affected. Happily, the results of the two analyses correspond in all important aspects, differing in only a few substantively trivial details.

of status consciousness in individuals) in order to explain the aggregate patterns observed among different classes of respondents. In this section we attempt to measure these variant forms of status consciousness at the level of individual respondents. If the measurement procedures described below are indeed meaningfully related to this objective, we should be in a good position to demonstrate the implications of an individual's status consciousness for his political and other status-linked behavior as well as at least to speculate on the likelihood and nature of confrontations among the social classes more generally. Clearly, to the extent that a competitive status consciousness pervades the class hierarchy, we might expect that, among other things, demands for changes in the actual distribution of valued resources and the means by which they are distributed will lack a class- or group-rooted base because individuals will see their own interests best served on an individualistic, rather than a class, basis. There will be a general acceptance of the legitimacy of the present levels of social inequality in the society and a lack of interest in changing fundamental social arrangements in any basic way. Corporate status consciousness, especially among the disadvantaged classes, on the other hand, should be associated with a willingness to change basic social arrangements in ways favorable to these classes. The following analysis of the German data is clearly speculative in nature and intended to suggest some of the possibilities of using the measurement device in question, rather than to demonstrate the validity of our interpretations. (See Laumann [1966], pp. 105-22, for an analysis of differential status awareness for the American sample.)

Our procedure is designed to discriminate among the various possible patterns of social distance responses an individual might make to occupations differentiated by socioeconomic status. Imagine a graph with social distance scores on the vertical axis (or, in this case, averages of social distance scores) and the three terms "blue-collar occupations," "middle white-collar occupations," and "upper white-collar occupations" placed from left to right on the horizontal axis.<sup>7</sup> If we plot a respondent's averages on such a graph, several broad types of patterns are possible. They are presented in figure 1, together with the number of respondents in Altneustadt who fall into each pattern.<sup>8</sup>

<sup>7</sup> More specifically, we did the following. First we selected from the social distance test booklet three occupations to represent each of the three occupational status levels mentioned above. Upper white-collar occupations were represented by medical doctor, *Diplom* physicist, and top industrial executive; middle white-collar by grocer, bank teller, and registrar; and blue-collar by carpenter, truck driver, and unskilled construction worker. Then we computed for each individual his average social distance scores for each of the three sets of occupations.

<sup>8</sup> In order to obtain sufficient numbers to sustain the following analysis, we included both male and female respondents. To justify such a procedure, we have carefully

Almost 63% of the sample fall into Pattern 1, in which respondents express highest social distance reaction to blue-collar occupations, moderate distance reaction to middle white-collar occupations, and lowest distance reaction to upper white-collar occupations. We hypothesize that this pattern will occur most frequently among higher-class respondents, for whom competitive and corporate status consciousness are indistinguishable in their impact and will decline as one descends the class hierarchy. Somewhat fewer than 9% fall into Pattern 2, in which respondents express lower social distance attitude to both upper-status and lower-status occupations but greater distance concerning middle white-collar occupations. Although this pattern is a logical possibility, it is hard to explain substantively and will be termed "error." The pattern should, therefore, be distributed randomly across the classes.

Twenty-four percent of the sample fall into Pattern 3, in which a higher social distance attitude is expressed with reference to both upper- and lower-status occupations and lower social distance to middle white-collar occupations. We expect this pattern to be displayed primarily by middle-class persons, for whom it is a form of corporate status consciousness. Finally, only 4.7% of the sample fall into Pattern 4, in which low distance is expressed concerning blue-collar occupations, moderate distance concerning middle white-collar occupations, and high distance concerning upper white-collar occupations. As infrequent as this pattern is, it is nevertheless the classic expression of corporate status consciousness of the lower classes and should be found to be associated with varied evidence of class-rooted hostilities toward the higher classes, following the reasoning of our introductory remarks.

Table 5 presents the cross-tabulation of the four patterns with the re-

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examined the question whether there are any systematic differences between men and women in their social distance responses to various occupations (see also Reuband 1974). It is not difficult to think of reasons for expecting such sex differences to exist. It has often been alleged, e.g., that women, being more dependent on men's occupation and socioeconomic status for their own social standing in the community, might be more responsive to prestige differences among occupations purely to defend their status interests (cf. Acker 1973). Moreover, sex differences might be expected to arise in part because the participation of women in the labor force, in Germany as in America (cf. Oppenheimer 1973), is much more restricted and selective than that of men. On the other hand, we should keep in mind the very powerful effect occupational prestige has on social distance attitudes and the general finding that men and women do not, by and large, rank the prestige of occupations very differently (cf. Reiss 1961). After extensive and detailed comparisons of male and female respondents' responses to the social distance scales, we found that, while there were some statistically significant differences in the mean scores by sex for each occupation, they were quite small, even when controlled for social class. Overall, one can detect a weak pattern in which women, in contrast to men, expressed, net of social class effects, attitudes of somewhat greater social distance toward some of the unskilled and semiskilled occupations at the same time that they expressed attitudes of lower social distance toward some of the middle-level white-collar occupations than did men.

## Forms of Status and Class Consciousness

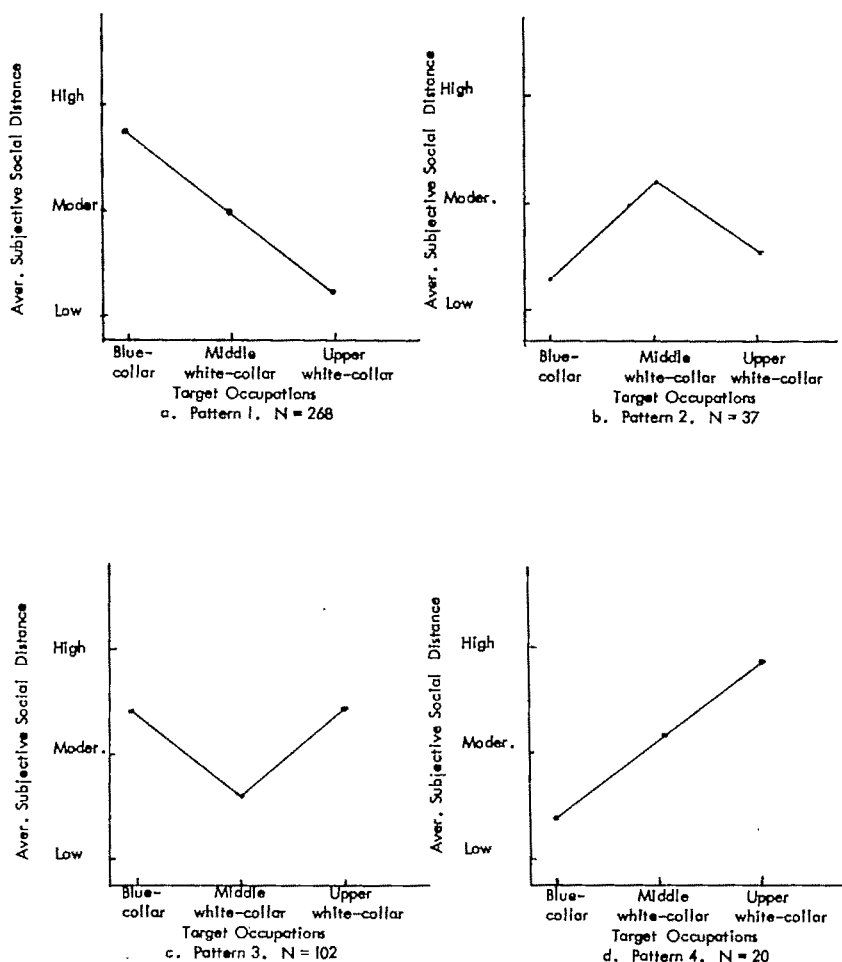


FIG. 1.—Four patterns of response to the social distance booklet

spondents' self-identified social class. The data generally conform to our expectations. Upper-class persons display the first pattern most frequently, with a steady decline in incidence from the highest to the lowest social class. The second pattern is essentially indeterminate, reflecting the fact that it constitutes a substantive "error"; and the third, though more frequent in the working class than we had expected, is nevertheless more common in the middle classes than in the highest class.

Finally, working-class people most frequently display the last pattern, as anticipated; but it must be stressed that the pattern is very infrequent, reflecting the pervasive impact of competitive status consciousness in this class. (Note that for working-class persons, even Pattern 3 represents a rejection of persons in blue-collar occupations for intimate association and

TABLE 5

INFLUENCE OF SELF-IDENTIFIED SOCIAL CLASS ON RESPONDENT'S PATTERN OF  
SOCIAL DISTANCE SCORES, % DISTRIBUTIONS

PATTERN	SELF-IDENTIFIED SOCIAL CLASS				TOTAL	N	M.D.*
	Oberschicht	Obere Teil	Mittelschicht	Arbeiterschicht			
1 .....	78	69	63	44	63	(268)	12
2 .....	10	12	8	6	9	(37)	2
3 .....	12	17	25	35	24	(102)	6
4 .....	0	1	4	15	4	(20)	0
Total N .....	100 (50)	100 (75)	100 (216)	100 (66)	100 (407)	...	...

NOTE.— $P < .001$ .

\* Missing data.

a preference for association with persons above them in the social hierarchy, albeit not with those *too* far above them. It might almost be seen as a version of a competitive status consciousness of "modesty," with persons who recognize their own lower social standing imposing restraint on their social aspirations to associate with higher-status persons.) What are interesting, however, about the 20 persons who express such strong corporate status consciousness are the facts that they are disproportionately both male (65% vs. 45% for the sample as a whole) and members of the Social Democratic party (67% vs. 53% for the sample as a whole). Historically, the Social Democratic party has been the vehicle of a Marxist, working-class-oriented movement dedicated to significant social changes of direct benefit to the working class as a whole. Given the peculiar class composition of Altnestadt, as well as possible biases in returning usable social distance booklets (see Appendix), however, it is not appropriate to draw inferences about the limited extent of corporate status consciousness in the working class in Germany. That is, the conditions for the emergence of corporate status and class consciousness are not especially favorable in a relatively small city with a multiplicity of small-to-intermediate-sized industries and artisan groups operating in a highly favored economic environment (cf. Halbwachs 1958; Bendix and Lipset 1966; Leggett 1968).

Given these suggestive results, we decided to explore the matter further by examining the association of the various patterns of social distance reactions with other aspects of the respondents' class, social, and political views (see Laumann [1966], pp. 47–53, 105–22, for a related analysis of the American sample). In this we faced an embarrassment of riches because so much of the interview schedule was devoted to exploring various features of people's attitudes to the stratification and political

systems. It was therefore difficult to avoid picking and choosing among the results for those which conformed to expectations, thus running the risk of capitalizing on chance findings. While we report only briefly on some results from a wider array that seemed to form a consistent, though not always statistically significant, pattern, we do caution the reader about the exploratory and tentative nature of the findings.

To estimate the net effects of patterns of social distance responses on an attitudinal variable, we calculated a one-way analysis of covariance in which the three patterns of interest (excluding the "error" pattern) were treated as the nominal-level independent variable and self-identified social class was treated as the covariate—thus netting out the effect of social class on the dependent variable. Table 6 summarizes the results. While the table contains no dramatic results, it does suggest that a respondent's pattern of social distance responses is associated with characteristic and, to our way of thinking, expected positions on political and social issues, net of the effect of his class position. Pattern 1 respondents, for example, who we have been arguing have an orientation upwardly deferential to the class hierarchy, tend to think that the unions have too much political influence in Germany. Pattern 4 respondents, on the other hand, seem to think that the unions, traditional supporters of collectively based working-class demands, ought to retain or even increase their political influence at the national level. Pattern 3 respondents are intermediate on this issue. Similar results are found with respect to the general sympathy of respondents for workers or employers in disputes over wages.

With respect to the belief in the legitimacy of the current levels of social inequality in Germany (note in table 6, by the way, the nonsignificant correlation of this attitude with social class position), Pattern 4 respondents are least inclined to accept this view, but this time they are followed by Pattern 1 respondents. The interesting point here is that Pattern 3 respondents are even *more* inclined to believe in the legitimacy of current levels of social inequality than are Pattern 1 respondents. Similar results are observed for belief in the ability of the average citizen to understand politics and governmental affairs, as well as in the legitimacy of the churches' influence on certain matters of public policy. No explanation readily suggests itself for the fact that Pattern 3 respondents depart from expectations by holding the most extreme positive views on the legitimacy of the current stratification system and the role of the churches in public policy and the greatest suspicion of ordinary people like themselves being able to understand political affairs (being implicitly likely, therefore, to depend on others for leadership and innovation). As a pure conjecture, we might appeal to the often alleged need of middling-status persons to conform to what they believe are the conventionally accepted norms relevant to a given situation, in contrast to the greater willingness

TABLE 6

MEANS ADJUSTED TO CONTROL FOR SOCIAL CLASS AND UNADJUSTED MEANS ON FIVE ATTITUDES TOWARD THE WEST GERMAN POLITICAL AND STRATIFICATION SYSTEMS, BY THE THREE PATTERNS OF SOCIAL DISTANCE RESPONSES TO OCCUPATIONS

SELECTED ATTITUDES	PATTERNS OF SOCIAL DISTANCE RESPONSES				SIGNIFICANCE LEVEL*	SAMPLE SIZE	ATTITUDE-CLASS CORRELATION
	1	3	4				
Political Influence of unions:†							
Unadjusted mean	1.6	1.8	2.1		...	...	-.13
Adjusted mean	1.6	1.8	2.0		.02	323	...
General sympathy for workers or employers:‡							
Unadjusted mean	2.4	1.9	1.8		...	...	.16
Adjusted mean	2.4	2.0	1.9		.12	344	...
Justification for social inequality in Germany:§							
Unadjusted mean	14.9	13.8	15.9		...	...	.06
Adjusted mean	14.9	13.9	16.0		.02	351	...
Ability of average citizen to understand politics and governmental affairs:							
Unadjusted mean	1.9	1.7	2.2		...	...	.14
Adjusted mean	1.9	1.7	2.3		.02	365	...
Public role of the churches:¶							
Unadjusted mean	2.4	2.2	2.6		.04	366	.10
Adjusted mean	2.4	2.1	2.5		...	...	...

\* These significance levels refer to the likelihoods that the *adjusted* means are equal. The effects of the covariate, social class identification, have been removed for these means. † Based on answers to the following question: "And how is it with the unions? Do you believe that the unions in general have too much political influence in the Bundesrepublik, or that they have just the right amount, or that they have too little political influence?" The scale ran from 1 for "too much influence" to 3 for "too little influence."

‡ Based on answers to the following question: "How is it with strikes and arguments about wages? Are your sympathies in general more on the side of the unions or on the side of the employers?" The scale ran from 1 for "on the side of the unions" through 2 for neither side to 3 for "on the side of the employers."

§ Based on answers to four Likert-scale items, which were summed to yield a scale ranging from 4 (high agreement on legitimacy of social inequality) to 24 (high disagreement on legitimacy of current levels of social inequality): (a) "If one has a high social or economic position in Germany today, it is a rather good sign that he has exhibited special capabilities or accomplishments." (b) "The rank differences among men are acceptable because they essentially express what one has done with the chances he has had." (c) "Economic profits are by and large justly distributed in Germany today." (d) "Persons with approximately equal social or economic positions ought to stick to themselves." These items were adapted from a more extensive scale designed to measure a person's conception of the legitimacy (justification) of social inequality that was employed in a study of the West Indian political elite shortly after independence (Duke 1967).

|| Based on answers to the following question: "Some people say that politics and governmental affairs are so difficult that the average citizen cannot understand what is going on. In general, do you agree or disagree with this point of view?" The scale ran from 1 for agreement to 3 for disagreement.

¶ Based on answers to the following question: "The churches have an important public role here in Germany. They must, for example, be consulted when there is an issue concerning the school system. Do you regard this as right, or do you believe their influence should be curtailed?" The scale ran from 1 for "right" through 2 for "undecided" or "don't know" to 3 for "curtail influence."

of high- *and* low-status persons to deviate from such norms (cf. Ranulf 1964; Homans 1961).

#### CONCLUDING REMARKS

To summarize, the replication produced substantial evidence for a remarkable degree of cross-national similarity in the subjective social distance responses accorded occupations varying in prestige and socioeconomic status, regardless of the class position and sex of the respondent. It is true, of course, that subjective class identification and sex do have some systematic impact on social distance responses. Some working- and middle-class persons do, indeed, prefer to interact with their peers, rather than with persons in higher- (or lower-) status occupations; and this manifestation of corporate status consciousness appears to be specifically linked to other political and social views consonant with corporate class consciousness as well. But these are relatively minor, albeit systematic, departures from the general picture of prestige- or upward-oriented preferences for intimate interaction at all class levels (however unrealistic in practice these preferences may be [see Laumann 1966, pp. 63-104; Reuband 1974, pp. 20-47]). This orientation, which we have been calling a competitive status consciousness, appears to be pervasive at the lower-status levels in both the American and the German communities studied.

In our judgment, the typology of the forms of class and status consciousness should help to clarify some of the complexities in analyzing people's subjective responses to the class and status order and their relationships with class- and status-linked attitudes and behavior. Strictly speaking, we have only been considering a device for assessing forms of status, rather than class consciousness, although we have noted some potential implications of status consciousness for class consciousness. Moreover, given the character of our data bases, which are drawn from specific communities in various ways atypical of their respective societies, the results reported must necessarily be seen as illustrative of a theoretical and empirical strategy, rather than as definitive of cross-national similarities and differences.

Turning to a discussion of the substantive implications of our investigation that require further work, we should first note that the extensive corporate status consciousness observed among the higher-status respondents in the German sample is, in fact, quite consistent with their actual patterns of intimate ties with higher-status persons like themselves. In other words, these people appear to be acting in consistency with their subjective preferences (see Reuband 1974, pp. 20-47). But even among our higher-status respondents, one observes a large number of intimate ties with persons substantially lower in status. That is, the pattern in no



way approximates a socially exclusive category of people in matters of intimate association.

What are we to make of the preponderant tendency of lower-status persons to aspire to associate with people in higher-prestige occupations and to reject those in lower-status occupations like themselves, when their actual pattern of differential association is much more consistent with the pattern predicted by the like-me hypothesis? We have provided modest evidence to the effect that such subjective preferences are, in fact, negatively associated with relevant corporate class-based political attitudes. Although we have insufficient cases to examine the following hypotheses in detail here, we suspect that further work will find that the status homogeneity of a person's intimates (cf. Laumann 1966, pp. 123-37; 1973, pp. 83-110), perceived and actual experiences with occupational mobility, and other characteristics of his or her personal and social history will be involved in explaining the genesis of corporate forms of consciousness. Their relatively limited incidence in the population may ultimately be traced to the broader institutional features of systems of inequality in industrial societies (cf. Boudon 1974; Giddens 1973).

With all the appropriate caveats concerning the problems of making cross-community and cross-national comparisons, the intrusion of presumed cultural differences between the two communities appears to be minimal. Such a conclusion poses the question of how we are to account for their relative absence. Certainly the explanation most readily at hand is to appeal to some form of the structural-convergence hypothesis which holds that fully modern industrial societies, at least in the West, tend to approximate one another in their basic division of labor; that the differential social evaluations associated with the contributions various occupations make to society's tasks tend to be very similar; and, much more tentatively, that the forms of class and status consciousness most likely to be engendered in such societies tend to take competitive, rather than corporate, form (cf. MacIver and Page 1949; Treiman 1970; Bendix 1974). Needless to say, much more comparative work of the sort attempted here remains to be done in "capitalist" and "socialist," "developed" and "developing" societies, in order to determine the limits of such generalizations.

#### APPENDIX

In this appendix, we describe our procedures in Altnestadt with respect to the construction of a German version of the subjective social distance scale comparable to the American one, its scaling properties, differential rates of completion of the social distance booklet used, and evaluation of possible biases in our results arising from selective returns (see Laumann

[1966], pp. 23–38, for a discussion of these issues for the American survey). We are especially fortunate to have in hand a recently completed doctoral dissertation by Reuband (1974), our research assistant in the design and execution of the German field study, who has done an exceptionally thorough job of examining these and related issues. Because he was substantively concerned primarily with the analysis of the determinants of interaction preferences and actual patterns of differential intimate association in Altnestadt and less with a comparative analysis of the two communities and because his work was conceived and executed at the University of Hamburg independently of our analysis, we are gratified by the extent to which the results of the two studies are consistent. It is especially reassuring in the light of Reuband's decision to rely exclusively on the respondents' occupations as the basis for assigning class categories, in contrast to our use of subjective class identification. There are, to be sure, important differences in procedures, emphases, and interpretations of results (see Reuband 1974, esp. pp. 81–131).

To construct a comparable set of relationships to include in the German version of the social distance scale, our German collaborator, Dr. Franz U. Pappi, and his associates, especially Karl Heinz Reuband at the Zentralarchiv für empirische Sozialforschung of the University of Cologne, after much discussion and pretesting of alternative forms, decided on the following battery of items:

Ich hätte gerne einen *Zimmermann*

- a) als meinen *Schwiegersohn*
- b) als meinen *Schwiegervater*
- c) als sehr *guten Bekannten*
- d) als jemanden, den ich *nach Hause* einladen würde
- e) in meinem *weiteren Bekanntenkreis*
- f) als unmittelbaren *Nachbarn*

Note that only six, instead of seven, relationships are specified in the German scale. The item pertaining to voluntary-association membership was deleted on the grounds that it tended to raise considerations of social selectivity that were not really comparable to those in the United States. The item "in meinem *weiteren Bekanntenkreis*" (i.e., "in my more extended circle of acquaintances") was believed to approximate better the degree of intimacy intended by item (e) in the American scale (cf. Reuband 1974, pp. 52–58). Despite the changes in the number of items in the scales and their content and in the number of occupational stimuli presented (17 in the United States and 18 in Germany), the scoring procedure described in the text insures comparability of the two samples, at least insofar as relative social distance reactions are concerned.

With regard to the scaling properties of the scale, we chose to treat the six items associated with each of the 18 occupations as forming a

Likert scale. To determine whether this could be justified, we calculated, first, Cronbach's alpha for several of the occupations. The alphas ranged from .83 to .92 for four occupations, leading us to think there was reasonable consistency among the items used to build the scale. We also calculated item-to-scale correlations to determine whether any single item correlated poorly with the overall scale. The lowest correlation obtained was .59. This evidence, we feel, justifies our conclusion that the items in the social distance booklet could be combined into the scale we used.

Taking another tack, Reuband (1974, pp. 250-54) subjected the items in the booklet to a Guttman scalogram analysis. Eight of the 18 occupations met the conventional Guttman criteria for scalability for the six social relationships (i.e., coefficient of reproducibility  $\geq .85$ ; coefficient of scalability  $\geq .60$ ). The others failed to meet these criteria, apparently because several of the social relationships in the set of items were essentially interchangeable and therefore could not be ordered without generating many errors. Accordingly, when only three items were used to form a scale order (viz., "son-in-law," "very good acquaintance" [friend], and "in my wider circle of acquaintances"), acceptable coefficients of reproducibility and scalability were obtained for all 18 occupations. These three items not only scaled for the total sample but also for each of three classes (working, middle, and upper middle) as well. Incidentally, a similar finding on the scaling properties of the items is reported by Taylor and Taylor (1971) for an English sample.

Exigencies of data collection in Altneustadt forced a change in procedure for administering the social distance test from that followed in the American study. In both communities, respondents were given a self-administered test booklet containing the social distance questions. In Cambridge and Belmont, the interviewer was present while the respondent completed the booklet, and he took it along with him at the end of the interview. In Altneustadt, the interviewer explained at the close of the interview how to fill out the booklet but left it with the respondent to fill out by himself and return by mail. This seemingly minor modification in procedure, unfortunately, led to a serious loss of properly completed forms; many respondents either failed to return the forms or filled them out improperly (e.g., answering only one of the Likert subscales for a given occupation). Even in the American survey, 21% had refused to complete the social distance booklet (see Laumann 1966, pp. 34-35), often on the simple and perfectly valid grounds that they objected to making such distinctions among people exclusively on the basis of their occupation. Only age was found in the earlier study to be correlated with refusal to answer the questions in the booklet; that is, older men were significantly more likely to refuse than younger ones. The refusals formed no statisti-

cally significant pattern with any other demographic or social characteristic of the respondents.

In Altneustadt, only 54.1% returned usable booklets. In examining the possibility of selective returns, we found, as one might expect, that working-class persons were significantly less likely to return their booklets than were middle-class ones. Fortunately for our comparative purposes, we found that this class bias in the return rate was attributable almost entirely to the relatively lower rate of return among working-class women. For the men in the sample, there was no statistically significant difference in return rates according to subjectively identified class membership.

Since we were going to analyze the data by class anyway, differential return by class was not especially problematic. What was important to determine was whether there were selective returns *within* a class category. Reuband (1974, pp. 241-49) attempted to assess intraclass bias by comparing respondents within each class who returned or failed to return usable booklets on their responses to three questions asked in the main interview that could be regarded as tapping a positive orientation to prestige differences in the class hierarchy. He found a tendency among men in the working class who expressed positive attitudes toward prestige differences in the class hierarchy to be somewhat overrepresented among those who returned usable booklets. This modest tendency is even more attenuated among the middle-class respondents and nonexistent among upper-middle-class ones. This finding suggests that we may overestimate the strength of competitive status consciousness among working-class respondents and, correlatively, underestimate the strength of their corporate status consciousness.

Once again, as in Cambridge and Belmont, age was found to be linked to refusal to complete the booklet—older people more often failed to return it than did younger ones. Interestingly enough, it appears that young people (under 35 years old) are somewhat more likely to have a positive orientation to the hierarchy of occupations as a basis of choosing interaction partners than are older persons (see Reuband 1974, pp. 247-49), perhaps reflecting their greater chances for intragenerational occupational mobility.

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# Herbert Spencer's Four Theories of Social Evolution<sup>1</sup>

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Although there is at present a revival of critical interest in Spencer, more disagreement than agreement exists among scholars regarding the exact nature of his social evolutionism. It is here argued that the single term "social evolution" was actually applied by Spencer to four quite different theories—an inherent source of difficulty for his readers. This essay, which strongly affirms each discipline's need for an accurate history of itself, provides a novel but fully documented analysis of what Spencer himself understood by "social evolution." It is concluded that an entirely new chapter on Spencer is required before either his historical or contemporary relevance can be accurately gauged.

## INTRODUCTION

There is today a resuscitation of interest in the sociology of Herbert Spencer. The present revival of Spencer marks a complete reversal of an earlier and long-lasting consensus among social theorists. In 1937, Parsons (1968, 1:3) expressed the earlier view when he echoed Brinton's query: "Who now reads Spencer?" Spencer's God—evolution—had abandoned him. "Spencer is dead." Several years later, it was proclaimed by Faris (1950, p. 176) that the "evolutionary conceptions of Spencer and his followers have all but disappeared from modern sociology" and by Corwin (1950, p. 187) that "Spencer's influence is today extinct."

The social evolutionists were dismissed for reconstructing history in their quest to establish the origins (not functions) of sociocultural traits and for generally violating the functional integrity of the whole by ripping traits from their systemic context. Social evolutionism, with which Spencer was conspicuously identified, did not seem compatible with an emerging functionalism; it did not seem to lend itself to a rising conception of society as a functionally integrated whole and to a methodological predilection for analyzing observable constitutive traits of the social whole in terms of their functional consequences for its integration and adaptation.

Today, the funeral orations of Brinton, Parsons, and others seem altogether premature. As Nisbet observes (1969, pp. 223 ff., 322 n.), there

<sup>1</sup> I should like to express a warm note of appreciation to Robert A. Nisbet for having initially stimulated a critical interest in reexamining the sociology of Herbert Spencer against the backdrop of current scholarly opinion and to Joseph B. Ford for having helpfully discussed with me the many-sided necessity of having an accurate history of sociology.



is at present a resurgence of evolutionism. Sociologists of this persuasion are now lauding Spencer for his pioneering efforts at building an evolutionary theory of society. Parsons (1961, p. viii) himself suggests that the revival of evolutionary thinking "testifies to Spencer's importance." And Lenski (1970, pp. 60, 110-11) applauds Spencer for having taken an "evolutionary view of history." This renewed interest in Spencer and evolutionary models of change seems to parallel the increasing attention being given to the modernization (or "development") of Third World countries.

In the wider context of a renaissance of interest in Spencer, it is now discovered that "he was also a thoroughgoing functionalist" (Carneiro 1968, p. 124), and, moreover, "between Spencer's evolutionism and his functionalism there is symbiosis not contradiction" (Abrams 1968, p. 68). Was Spencer discarded for not having what he had all along, namely, a functionalism?

Each science needs an accurate history of itself. As Gouldner (quoted in Goldthorpe 1970a, p. 9) has cogently answered Whitehead's famous aphorism about a science being "lost" which "hesitates to forget its founders": "But to forget something, one must have known it in the first place. A science *ignorant* of its founders does not know how far it has travelled nor in what direction; it, too, is lost." The immediate problem of this paper is one of empirical history, for the current written accounts and analyses of Spencer's evolutionary theory are incomplete and often contradictory. Much of this results from a failure to recognize that Spencer had more than one theory of social evolution. Taken in their totality, then, existing accounts of Spencer's thinking make it impossible, first, to determine precisely in what his position consists; second, to raise questions of the relevance—positively or negatively instructive—of any of his formulations for today's evolutionism; and third, to judge, as Gouldner suggests we must, distance and direction traveled—to judge, for example, whether we have indeed, to quote Brinton, "evolved beyond Spencer."<sup>2</sup> It is hoped, then, that the present effort will help bridge the gap between Spencer's actual social evolutionary theory and existent views of it.

#### THE DISARRAY IN THE CURRENT CRITICAL LITERATURE

The social evolutionism appearing, in greater or lesser degrees, throughout most of Spencer's nearly two dozen published volumes has received a very

<sup>2</sup> This is indeed a moot question. For while Parsons (1966, p. 109) claims neo-evolutionism is not just something which belatedly claims the "old social evolutionists were simply right after all" but rather is a theory which is "more than merely reviving old ideas," Nisbet (1969, pp. 227-28) insists he "cannot find the substantive difference": "The differences between contemporary *social* evolutionary theory and the theory of Herbert Spencer do not seem very large or very significant."

wide range of critical interpretation. The following may be taken as a representative survey. LaPiere and Bock maintain that for Spencer evolution resulted from immanent forces: he "assumed that social change comes about through the working of some process that is built into society, some inherent process" (LaPiere 1965, p. 37); it was an "unfolding of potential," something over which "external" causes had but minimal influence, an influence never affecting the "course or direction of change" (Bock 1964, pp. 23-24). On the other hand, for Burrow (1966, p. 202), Spencer was an "'environmental determinist.' He holds that all changes come about through the action of changes in the action of the environment on the subject of change."

According to Abrams (1968, p. 68) and numerous others, natural selection was the key to Spencer's social evolutionism. Conversely, Peel (1972, p. xxii) states that "natural selection, though accepted, was a late and superfluous element in a system that was essentially Lamarckian." For Harris (1968, pp. 107, 129, 130), Spencer, while purporting a belief in universal progress, biologized history. His "fundamental error was that he drastically over-emphasized the importance of hereditary factors as causal elements in the explanation of the behavioral specialties exhibited by *Homo sapiens* populations"; he thus espoused a "racial determinism." In contrast, Carneiro (1968, p. 126) writes that Spencer did not believe "'racial' differences" could account for sociocultural differences: "Almost invariably he explained cultural phenomena primarily by the interplay of cultural and environmental factors."

For Bock (1964, pp. 23-24), Spencer held that change was "inevitable" and had a definite "direction," one determined by "something within society or culture": "The direction of social change is from the homogeneous to the heterogeneous. . . ." This was social evolution, something synonymous with change. Vine (1969, pp. 57-58) reaches a similar view: evolution was "automatic," and Spencer stated that "societies or social systems always tend to become more heterogeneous." For LaPiere (1965, p. 37), Spencer's conception of evolution was decidedly unilinear: "Every society, Spencer thought, goes through a series of fixed and immutable stages." So conclude Moore (1963, p. 7), Roth (1973, p. 78), and, just recently, Denisoff, Callahan, and Levine (1974, p. 6). Timasheff (1967, p. 42), among innumerable others, argues that "Spencer persistently removed culture items from their contexts and fitted them into his own preconceived patterns." But, on the other hand, for Abrams (1968, p. 72), Spencer only employed stages "as an organizing principle," and the "analysis throughout the *Principles of Sociology* tends to be in terms of particular interactions of structure and function in this or that concrete setting." Similarly, Carneiro (1968, p. 126) states that Spencer saw the "process by which societies develop as consisting, by and large, in responses

to particular problems posed by cultural and natural environments, rather than in movement through a universal and necessary series of stages." There was no "unilinearity" (see also Andreski 1971, p. 14). And, according to Burrow (1966, pp. 191-92, 203), Spencer did not see evolution as inevitable nor even as the most common form of change (see also MacRae 1969, p. 29). Evolution was contingent, not necessary or inevitable. Neither did Spencer violate the unity of the whole, for he approached "societies as systems of complex functional relations" and was interested in "types of societies, not the history of isolated institutions or culture traits."

Buckley (1967, pp. 12-13) declares that Spencer was ignorant of (social) species and phylogeny (speciation). Similarly, for Bock (1964, p. 36), Spencer's theory of social evolution bore no true analogy to theories of biological evolution: the latter was concerned with the "problem of differences"—speciation—while the former saw all societies as of one form (species), differing only in their stages of "maturity." Lenski (1970, p. 60), on the other hand, praises Spencer for having recognized real and important "similarities between organic and socio-cultural evolution. . . ." Similarly, in the opinion of Goldthorpe (1970*b*, p. 79), Spencer did attempt to show "that the evolution of societies, considered as entities, was a process essentially akin to that of the evolution of [biological] species."

Vine writes (1969, p. 55), "Because of Spencer's focus on evolution, change, and origins, he gave little attention to the persistence of social structures"; more generally, "he was not concerned with the problems of social control and the perpetuation of social systems." A similar view is expressed in Parsons (1967, p. 30; see also 1968, 1:4, 311 ff., 346), where it is stated that Spencer's view amounted to "the negation of social control." For Gluckman (1965) and Bock (1964, p. 37), functions are ignored in Spencer's evolutionary sociology. Conversely, it seems to Coser (1971, p. 97) and Peel (1971, p. 183) that Spencer did seek out functions of traits for integration, persistence, adaptation, and the like.

Spencer's social evolutionism is many things to many scholars. Few of their interpretations are complete or free from serious inaccuracies; taken together, they are often mutually contradictory. The argument of this paper is that Spencer had not one, but four discrete theories of what he, himself, frequently termed "social evolution" (and sometimes "social development"). Not all, but perhaps a good share of the current exegetic diversity in sociology's empirical history stems from this unrecognized fact.

#### SOCIAL EVOLUTION AS PROGRESS TOWARD AN IDEAL "SOCIAL STATE"

The earliest usage is to be found in Spencer's *Social Statics*, first published in 1850, which spells out the functional prerequisites of an ideal society

or "social state." It is a society based upon amity, individual altruism, an elaborate specialization of functions, criteria which recognize only achieved qualities (as opposed to ascribed ones), and, primarily, a voluntary cooperation among highly disciplined individuals. It is, accordingly, a society in which formal government practices negative regulation only—"ought nots."

In this future society (movement toward which constitutes "social progress" or, as it was later renamed, "social evolution") much, then, depends not merely upon voluntary contractual relations generally but, far more basically, upon a well-diffused (and somewhat puritan) morality (e.g., 1892, p. 106).<sup>8</sup> That is, essentially, the solution to the book's foremost problem, namely, "how an aggregate of citizens may stand without tendency to conflict and disruption" (1904, 1:414). A "system of equity"—rights and duties—and full commitment to it by individuals constitute the *sine qua non*. The question, however, is how this state of affairs comes about.

The crucial assumption is this: "Conduct has to be ruled either from without or from within. If the rule from within is not efficient, there *must* be a supplementary rule from without . . ." (1892, p. 106). For Spencer (1969, p. 106; 1897, 3:553; 1860, p. 5), the essence of society is cooperation and regulation—the "control of individuals." Rules and norms and controlling or constraining agencies exist to the extent that spontaneous behavior does or would threaten social stability. "Controlling agencies" of all kinds, from group customs and diffuse public opinion to formal government and law norms, are seen as functionally indispensable concomitants of collective existence. The extent of "external control" is variable, however, and the ideal society presumes the elimination of the chief conditions which necessitate extensive "rule from without."

Intersocietal hostilities constitute the major factor brutalizing personality, accentuating egoism, and developing within the group behavioral propensities for conflict because of the means which are chosen (e.g., technically efficient, not necessarily normative, ones) to obtain commonly desired but relatively scarce objects and ends. In short, "war fosters anti-social sentiments" (1969, p. 252). Even with the important intragroup consolidating effects which, as Spencer well sees, derive from war and the preparation for it, the aggressive substratum is always a potential threat to the cohesion and stability of the group; its expression against fellow citizens is prevented only by the continued existence of outgroups and the elaborate regulating structures endemic to military societies—for example, ancient Sparta, Peru, and Mexico. In view of this threat, an ideal society must presuppose the cessation of intergroup hostilities and also an even-

<sup>8</sup> Unless otherwise specified, references are to Spencer.

tual stabilization of population, for in dilating population is found the Malthusian source of the "struggle for existence."

In his earliest and most optimistic writings, Spencer derives pacifism (as well as declining fertility rates) from advancing industrialization, itself unexplained beyond being loosely connected with certain phenomena which war alone has produced—namely, large, consolidated, and highly dense aggregates in which population pressure engenders successive functional-structural specializations. History is thus to bequeath societies which are wholly industrialized and differentiated, wholly pacific, and which have fertility levels that, at most, do not exceed the capacity of economic organization for functionally absorbing new increments of population. Spencer naively assumes that the consequences of war—large-scale nations and thus the possibility of extensive economic interdependencies—will aid in putting an end to war.

Apart from threat or military exigency, the activity of work—"peaceful labour"—is to become the setting for the full development of "altruistic sentiments" and the corresponding diminution of "egoistic sentiments." "Moral nature" (or social values generally) varies with "social organization," and the "moral nature proper to a social organization based on contract instead of *status* [as with the military society]—the moral nature fostered by a social life carried on under voluntary co-operation instead of compulsory co-operation, is one . . . which works out political freedom" (1897, 3:139). If positive governmental control ("oughts") subsides, it is because it becomes less necessary for social equilibrium. "The diminution of external restraint" takes place "at the same rate as the increase of internal restraint" (1892, p. 106). Spencer also looks for a long-range change in human nature itself, namely, the securing of altruism as a species trait (via environmental-hereditary adaptation) by the continuation of peaceful cooperation for an indefinite period of time; that is to say, social and cultural changes might be followed by organic changes. On balance, then, it is the conditions of existence which are primary: "Social traits are not peculiar to any variety of men [race] but are dependent on conditions" (quoted in Abel 1970, p. 133).

The older Spencer is less sanguine, seeing not a progressive decline of warfare (together with its consequences for a society's structure, culture, and character) but "rhythms" between war and peace, with the massive industrialized-militarized state cemented by permanent *bureaux*. Spencer's earliest view thus is revamped. Whereas the "industrial type of society" denotes a society which is both pacific and industrialized,<sup>4</sup> the historical heir to the "militant type of society" (thanks to spreading industrialism

<sup>4</sup> At least, this is the case in Spencer's earlier use of the expression. Later on, "industrial" is used to mean peaceful or pacific, but not necessarily industrialized.

and its assumed antimilitary nature), both become, by the 1880s, "theoretically constructed" types to be used strictly in comparative analysis (see 1897, 1, pt. 2, chaps. 10-11; 2, pt. 5, chaps. 17-18). But as the view stands at the beginning of Spencer's long career, it is concerned with "the equilibrium of a perfect society" and connotes a process of change which is the realization of man's altruistic potential, "a development of man's latent capabilities under the action of favourable circumstances" (1892, pp. 233, 237). It is, consequently, a morally progressive change. It is, additionally, necessary and inevitable, given the expected attainment of anterior conditions—war, for large and dense aggregates, these producing in turn internally specialized and interdependent economic relations and, finally, peace. It is change that is more or less organized by stages, hence directional and uniform in large part, and it is unaffected by race. Spencer's view is holistic, that is, it relates social activity, structure, culture, and personality; it is much informed by Spencer's constant awareness of the problem of maintaining social order and control.

#### SOCIAL EVOLUTION AS THE DIFFERENTIATION OF SOCIAL AGGREGATES INTO FUNCTIONAL SUBSYSTEMS

A second distinguishable theory called social evolution by Spencer amounts to, first, the assumption of functional requirements for a society's continued existence and, second, the general proposition that whole societies tend to differentiate into "societal subsystems," the functions of which correspond to these requirements.

From the wider spectrum of Spencer's writings, it is clear that what he alternatively calls "social requirements" and "social needs" are dictated by conditions which are logically more primitive and which are temporally prior or at least coextensive. These conditions are the exigencies posed, first, by the facts of human nature (for instance, mortality, reproduction, sustenance needs); second, by society's external environment (for example, flora, fauna, climate, topography, human life); and, third, by the nature of "social existence" itself (that is, the unique "conditions produced by social aggregation"). Plural existence—the sheer fact of a juxtaposition of individuals in time and space—is a problem insofar as societal persistence presupposes some degree of interindividual cooperation as against a Hobbesian "war of all against all." From the above exigencies, the following requirements emerge, each being closely tied to the idea of interunit cooperation: procreation ("maintenance of the race"), production ("social sustentation"), exchange ("social distribution"—the disposition of scarce resources), communication ("internuncial function"), means for position-role placement ("the transmission of positions and functions"—required because the "maintenance of a society's organiza-

tion implies that the units forming its component structures shall severally be replaced as they die" by the criteria of "personal qualities" or "inheritance," in short, by a principle of stratification [1897, 2:258, 259, 263]), and the control of individual behavior ("social regulation"—the inducement of common beliefs, values, and ideas, which involves political, social, and intraindividual or internalized control and by which "social cohesion" is possible).

This last element points up the essence of social life. For Spencer (1897, 2:244, 263; 3:553), societies presuppose "mutual dependencies" and cooperation. But cooperation, far from being automatic or spontaneous, entails supraindividual forces, namely, "regulation, and an implied submission to regulating agencies" (1969, p. 106). Cooperation presumes cohesion as well as rendering it possible (1897, 2:262-63); and both, along with the empirical realization of specific functions, presuppose regulation. As Spencer (1860, p. 5) affirms, "from the far past even to the present, social needs have subordinated individual needs, [and] the chief social need has been the control of individuals." In his view, there are three distinct facets of social regulation: political control, social control, and socialization.

There is, first, the problem of control for the realization of collective goals. Political organization is required, that is, individual actions and "volitions" must be "constrained" in such ways that "social safety shall not be endangered by the disorder consequent on unchecked pursuit of personal ends" (1897, 2:246-47). Spencer's (1897, 2:606) most frequent illustration of a collective goal is that of defense: "The continued existence of a society implies," for example, "that it shall not be destroyed by foreign foes"; he discusses how this goal empirically constrains the operation (or limits the variability) of other social activities and both structural and cultural processes such as production, distribution, general measures of social control, and sanctioned attitudes and opinions.

There is, second, the necessity for the establishment and maintenance of interindividual relationships which are "cohesive," which show a "tolerable harmony." "Social union," Spencer insists (1897, 2:272), "requires a considerable homogeneity of nature among [individuals]." As such, groups of individuals "made alike in ideas and sentiments, are groups in which the greatest social cohesion and power of co-operation arise" (1897, 2:285-86). The focus here is upon the overall control of a plurality of individuals—the realization of orderly and stable social relationships and patterns by the diffusion and maintenance of common values and beliefs.

There is, finally, the problem of establishing and maintaining a correspondence between the sentiments, beliefs, and values of most individuals and the wider social organization with its associated normative substratum.

This is essentially a problem of legitimizing the social order, of giving it an "ethical sanction." "Unless the mass of citizens have sentiments and beliefs in something like harmony with the social organization in which they are incorporated, this organization cannot continue" (1961, p. 158). The focus here is intraunit, that is, the inculcation in successive generations of individuals of the belief that existing social structure and cultural values are legitimate, are worthy of positive affect and support, quite apart from any sanctions associated with deviation. The relevant process here is socialization.

Spencer's basic explanatory form, in this second theory, is functionalist—"one in which the *consequences* of some behavior or social arrangement are essential elements of the *causes* of that behavior" (Stinchcombe 1968, p. 80). To understand, Spencer argues (1897, 3:3), how *basic* organizations—kinship, religious, economic, ceremonial, political—everywhere originate and persist, it is necessary to understand their functions, the needs subserved. The chief proposition is that, "apart from all their special differences, societies have general resemblances in their modes of origin and development. . . . [S]ocial organization has laws . . ." (1969, pp. 148–49). Confronted with common functional problems, societies display certain basic commonalities. Spencer's (1873–1934) *Descriptive Sociology*, the empirical foundation for the *Principles of Sociology* (1897), demonstrates the existence of universal social institutions and social activities.

None of this means, as Durkheim (1964, chap. 5) and others have charged, that Spencer fails to separate, in sociological explanation, the causes of origin (efficient causes) and functions. Indeed, referring to religion, for example, Spencer writes (1880, pp. 10–12), "we are bound to ask its origin and its function. . . ." What Spencer does mean is that, with respect to social phenomena corresponding to social needs ("vital functions," as he also terms them), generalist causes (i.e., those in which the focus of determinacy resides in the properties of the individual qua individual or in the collectivity qua collectivity) explain what is constant and "necessary," what is functionally essential, and historical causes or "special facts" explain what is "accidental" or a variation (see, e.g., 1961, p. 192). For example, history explains the particular tenets and dogmatics of this or that religion, while sociology—conceived as the "generalizing science" par excellence—explains the universality of religion, with its integral "codes of conduct" and sanctions for deviation, as well as cognitive orientations to what defies rational comprehension. In a fundamental sense, to paraphrase Homans (1950, p. 271), efficient causes often play into the hands of final ones, that is, social functions. No critical understanding of Spencer is possible, then, without a clear idea of his key ob-



jective of discovering "general facts," phenomena "displayed by societies in general, dissociated, so far as may be, from special facts due to special circumstances" (1904, 2:481; 1897, 1:37).

Real societies, Spencer holds, differentiate into "societal subsystems" (regulative, sustaining, and distributive), which he (1897, pts. 2-8) tends to analytically decompose into universal institutions, for example, kinship, religious, political, ceremonial, and economic institutions. A good share of the *Principles of Sociology* consists in an analysis of alternative structural *possibilities* (i.e., different institutional forms) for meeting functional imperatives common to all societies as well as more or less typical ways in which types or species of societies (e.g., simple, complex, military, pacific) *do* meet these problems, such as the structural ways in which social order is characteristically achieved in simple or in complex societies. With respect to procreation, for instance, Spencer (1897, 1:603) notes that "the requirement that a due number of adults shall arise in successive generations, may be fulfilled in variously-modified ways. . . ." Generally, kinship functions for "social self-preservation" ("propagation"), socialization ("the rearing of children"—fostering "moral discipline" and "intellectual culture" through parents' "care of their children's minds"), and, in proportion as "kinships become more definite and extended," for "social cohesion" and "social stability" as well (1897, 1, pt. 3, esp. pp. 273, 717, 718). The basic rubric of functional analysis is this: "Family organizations of this or that kind have first to be judged by the degree in which they help preserve the social aggregate they occur in" (1897, 1:610). A similar logic informs Spencer's treatment of other principal social institutions. Religious institutions, for instance, generally "maintain and strengthen social bonds, and so conserve the social aggregate" (1897, 3:102). But one cannot, *a priori*, transfer assumptions concerning the functioning of an institution in one society or class of societies to another society or class of societies. Compared with what is typical of primitive society, for example, religion in "Christendom has not exemplified in any considerable degree a like consolidating effect" (1897, 3:98). Thus, in the final analysis, religious beliefs and rites must be "consider[ed] solely with reference to the function they fulfil where they are indigenous" (1868, 1:445-46). Generalization follows, does not precede, careful comparative analysis.

In conclusion, this second theory is one of origins and process, of movement toward a first stage of "functional equilibrium." In response to functional requirements, social aggregates tend to evolve what Spencer at one point calls "answering structures." An important part of Spencer's functional analysis consists in relating the latter to the former for each society or species of society surveyed. This second theory is based on the acceptance of immanent causation in that, to put it most abstractly, the focus

of determinacy in the origin of cross-cultural phenomena which are functionally related to basic social needs is seen to reside in the properties of individuals and of pluralities. The theory allows for external causes insofar as environmental factors are seen as facilitating or retarding "social intercourse." It is Lamarckian to the extent that Spencer sees a tendency toward adaptation ("conscious," e.g., production, and "unconscious," e.g., religious beliefs, for those in action) to the general conditions of social existence in a physical environment and Darwinian to the much more limited extent that it is not held that *all* social assemblages become and persist as societies (1897, 1:622). Race does not affect particular adaptations of various groups.

If, for Spencer, general phenomena are owing to general causes, more specific phenomena are owing to more specific causes; for example, some differentiation of authority exists in all societies, but it is more centralized in military societies and more diffused in pacific societies. The process of functional differentiation resembles an ontogenetic movement toward increased heterogeneity of structure and function. However, fixed stages are not suggested by Spencer to describe this process, for it is not inevitable. There is the necessary condition of a permissive environment, and the belief is present if not well expressed in Spencer that arrangements evolved are not always fully efficient for given functional ends (Spencer here cryptically refers to "abortive attempts," i.e., unexplained exceptions to general developmental principles). The broader theory is not atomistic, dealing with isolated phenomena, but holistic, explicating a process relevant to the total social aggregate. While the theory points to what is common across societies, it does not assume all societies are completely alike—of one species—differing only in their stages of maturity or inevitable development. The problems of persistence, adaptation, control, and the like weigh heavily, for they are the warp and weft of the concept of society—"an aggregate presenting multitudinous phenomena" which "are held together as parts of one great combination" (1888, p. 108). In his functional analysis, Spencer makes both general statements (what, e.g., religion typically, as a mode, functions for) and more specific statements (what, e.g., the effects of religion were in this or that society during this or that time period).

#### SOCIAL EVOLUTION AS AN ADVANCING DIVISION OF LABOR

A third identifiable process called "social evolution" by Spencer (1904, 2:297) is that of an "increasing division of labour." This is in essence an equilibrium theory. In point of fact, the concept of equilibrium, not that of automatic and unproblematic serial change, is central to Spencer's general sociology (Perrin 1973, pp. 50–52). Specifically, for instance, an in-

crease in population size constitutes a "perturbation" of a prior social equilibrium: augmented numbers impose new adaptive exigencies on existent social organization which require structural adjustments. By way of general definition, "the evolution of a society [is] at once an increase in the numbers of individuals integrated into a corporate body, an increase in the masses and varieties of the parts into which this corporate body divides, as well as the actions called their functions, and an increase in the degree of combination among these masses and their functions . . ." (1937, p. 464).

The present theory revolves about changes in size, in type of cohesion, and in degree of differentiation. First, unlike the second theory, where a minimum (though never specified) and not necessarily ever-dilating size was presumed, the third theory takes continued population growth as its *sine qua non*. The historical formation of large aggregates has been primarily "by union of groups, and union of groups of groups" (1897, 1:464-65). While natural increase in a group's size is not a universal phenomenon (there may be a population policy), the unions have been made possible, at bottom, by a general "excess of fertility" among the species and the resultant competition for scarce resources (1971, pp. 33-37, 121, 123-24). However, the immediate mechanism of the compounding of groups has been war (1961, p. 176).

Second, this evolutionary process also involves a fundamental change in the principle of social cohesion. Prior to the interdependence wrought by an advancing division of labor in a given society, the "only mutual dependence is that consequent on mechanical union"; societies "primarily consist of many like segments" (1969, pp. 207, 227). The principle of cohesion in noncomplex societies is ideational homogeneity, something expressed through kinship, religion, and custom. In such a society, its "ideas and usages form a kind of invisible framework for it, serving rigorously to restrain certain classes of its actions" (1897, 2:322). "The power of the society over the individual is greatest among the lowest peoples. . . . Inherited rules which the living combine to maintain, and the authority of which no one dreams of questioning, control all actions" (1904, 1:511-12). The ubiquity and potency of what Spencer terms the "aggregate will" are most pronounced in the least complex societies.

Throughout the *Principles of Sociology*, Spencer details how kinship, common rites and beliefs, and ceremonials (customs and usages) generally, both maintain and reflect a "relative homogeneity." But war, and its imperatives and results, ushers in the state, the primacy of political integration and allegiance, and the setting, the requisite prior coherence, for a ramification of economic bonds, themselves multiplied by the immediate cause of competition. Briefly, war historically made large aggregates possible, and the "pressure of population" (size and density)—the "im-

mediate cause [of] competition"—made it "possible for the number of special occupations to increase" (1897, 3:368; see also 1:471 ff.); the latter made further (sustained) growth possible with still greater "demand" and the "intensifi[cation of] the functional activity of each specialized person or class" (1868, 1:52; see also 1897, 2:250; 3:368). The division of labor, then, is a functional adaptation—a re-equilibration—in those societies where an increase in volume and density aggravates the pressure for survival and fosters greater functional specialization—an "advance in organization," as Spencer often puts it. The third basic defining trait of modernity, then, is differentiation of social structure (see esp. 1937, p. 292).

Well-known and commonly accepted critiques notwithstanding (e.g., Durkheim 1933, pp. 200 ff.; Parsons 1968, 1:4, 311 ff.), the principle of cohesion in complex societies is not, for Spencer, one of naked economic "interest" among self-gratifying individuals. With respect to social cohesion, Spencer, throughout his writings, cites not only "pecuniary interests" but also political allegiance ("love of country"), "family bonds" (e.g., the concern with family honor characteristic of upper social strata), the "restraining" and "sacred sanction" of religious values, governmental and judicial superintendency over the execution of contracts, various customary norms not specifically spelled out in formal political and religious creeds, and notably, a general normative ("moral") consensus—a "social opinion," a "social force," which regulates the means through which interest motivation can legitimately operate (1897, 2, pt. 5; 3, pt. 6; 1969, p. 273 ff.). In his "Railway Morals and Railway Policy" and "The Morals of Trade" (1891, 3:52–112, 113–51), for example, Spencer takes considerable note of the disorder which obtains in proportion as self-interest is unrestrained and unregulated. Referring to his own industrialized England, for instance, Spencer unequivocally declares: "A system of keen competition carried on, as it is, *without adequate moral restraint*, is very much a system of commercial cannibalism" (1891, 3:138, italics added). Spencer refers to "cheat and be cheated" and has but little faith in the professional ethics Durkheim later emphasizes (1969, p. 247). In fact, Spencer calls for greatly heightened and extended morality in economic relations and occupational and professional groups (law, the military, trade, banking, transportation, politics, and manufacturing are all mentioned); from society itself is required a "purified public opinion." Economic relations can only become sufficiently moral and stable "by a stern criticism of the means through which success has been achieved" (1891, 3:151). Spencer (1891, 3:147) observes that "with the great majority of men, the visible expression of social opinion is far the most efficient of incentives and restraints." Spencer also turns to government, which is called a "social force," and insists that the "restraining power of the State over individ-

uals, and bodies and classes of individuals," is both "requisite" and in need of extension (1969, p. 288). The bonds effected by the division of labor in modern societies do not, cannot, rest upon a presumed automatic harmony of individual interests: the concomitant must be a well-diffused morality and restraint.<sup>5</sup>

The problem of integration is apparent to Spencer at all levels of analysis. With respect to groups connected with institutions (e.g., military, political, religious, and business institutions), social classes, and organizations of all kinds, "the dominant aim of each is to maintain itself," to resist alteration; "hence parts once formed tend to continue, whether they are or are not useful" (1897, 2:254, 263). ". . . When the function is needless, or even detrimental, the structure still keeps itself intact as long as it can" (1897, 2:255). The advancing functional specialization which is the subject of Spencer's third theory of social evolution sharply points up, then, the inherent problem of reintegration: reestablishing the "social *consensus*" which is the *sine qua non* of social order, according to Spencer's model.

The sociocultural universals Spencer identifies—kinship, religion, "distinctions of duties," etc.—do not of course operate everywhere equally well for social cohesion. Religion in nonliterate societies, for example, is regarded by Spencer as an extremely puissant "controlling agency." But religion in modern industrialized societies, while still seen as a "social bond," is held to be much weaker in controlling individual actions, economic or otherwise. Indeed, in the last work of his "Synthetic Philosophy"—the *Principles of Ethics*—Spencer (1966a, 1:iv) indicates that "moral injunctions are losing the authority given by their supposed sacred origin." The current and future problem, a problem of social order and stability, lies in the filling of the social void. While Spencer clearly sees the problem—a growing morality of *expediency*—he fails to provide any solution.

In conclusion, Spencer's third theory is by no means devoid of attention to the Hobbesian problem of order. Beyond this, change is not seen

<sup>5</sup> Durkheim's (1933, pp. 200 ff.) critique of Spencer's understanding of "contractual solidarity" seems primarily based upon Spencer's (first) theory of evolution toward an ideal "social state" (where individual altruism and an ultimate identity of interests are basic) and his initial enthusiasm for classical laissez-faire economics. In so doing, Durkheim is wide of the mark in two ways. First, Durkheim (1933, pp. 204 ff.) admits that Spencer's view of a so-called "spontaneous accord of individual interests" pertains only to the ideal society not yet in existence; and then he proceeds as if this (utopian) view constitutes Spencer's entire position on the question of the source of solidarity for actually-existing industrialized societies. Second, Durkheim makes no mention of Spencer's later awareness of many of the abuses and excesses of laissez-faire economics and emphasis upon the necessity of a basic "homogeneity" (of ideas, sentiments, values—of morality) to undergird and cincture a "heterogeneous" system of economic cooperation.

as an automatic and inevitable unfolding of inherent potential in a pre-ordained direction. Rather, the "progress of a social organism toward more heterogeneous and more definite structures . . . continues only as long as the actions which produce these effects continue in play" (1937, p. 523). In Spencer's adaptive model, "structural complexity" depends upon the complexity of the exigencies or "forces" under which a society exists. Thus, the "tendency to progress from homogeneity to heterogeneity [of structure] is not intrinsic but extrinsic"; it "is determined by the co-operation of inner and outer factors," equilibration or adjustment, which "works changes until there is reached an equilibrium between the environing actions and the actions which the aggregate opposes to them . . ." (1937, p. 535; 1897, 1:95). In the present model, the impact of new increments of population on existent social organization introduces differentiating forces, for example, strain, intensified competition, and specialization. Spencer's analyses are most concerned not with "external conditions" generally but with those owing to intersocietal relations (hostile, nonhostile) and with the variable of size—the numbers to be sustained—vis-à-vis the strictures of physical environment. In the present regard, Spencer's functional analyses range from assessing the role played by political and economic bonds in consolidating large aggregates (while at the same time undermining or attenuating bonds of region, custom, and kin) to examining the relative inefficiency of modern religious and moral codes and governmental agencies in supplying a fully effective restraint against corruption and individual egoism and aggression.

#### SOCIAL EVOLUTION AS THE ORIGIN OF SPECIES OF SOCIETIES

The final theory concerns the origin of social species. A persistent error among many students lies in the contention that what Spencer called social evolution was in no case a true analogue of biological evolution (phylogeny or speciation) (Bock 1964, pp. 31, 36). Spencer's biology (1966*b*) sought to explain both ontogeny (embryonic development) and phylogeny, and his sociology was similar in principle. To the former (ontogeny) corresponded, first, the primordial differentiation of whole societies into functional subsystems and, second, a continuing ramification of the division of labor under appropriate conditions; to the latter (phylogeny) corresponded the proliferation of social types or species, classified structurally as simple, compound, doubly compound, and trebly compound and then by such further subdivisions as type of political headship and whether settled, nomadic, or mixed (1897, 1, pt. 2, chap. 10).

It is ironic that the exact criticism Bock (1964, pp. 31, 36) launches at both Comte and Spencer is levied by Spencer against Comte. Comte is decisively taken to task by Spencer (1961, p. 300) for the "erroneous

preconception" which holds "that the different forms of society presented by savage and civilized races all over the globe, are but different stages in the evolution of one form." The "truth," Spencer (1961, p. 300) argues, is "that social types, like types of individual organisms, do not form a series, but are classifiable only in divergent and re-divergent groups." Societies have differentiated (from a supposed "original unity of the human race") and spread over the face of the earth, and "the multiplying groups have tended ever to acquire differences, now major and now minor"; thus, "there have arisen genera and species of societies" (1897, 3: 331; see also 1961, p. 53).

Any discussion of "species of societies" necessarily presupposes some definition for each one, as well as an account of how all the species have come to be. Here, to oversimplify, Spencer combines Malthus (surplus fertility), Lamarck (adaptations to local environments and conditions with subsequent transmission through culture or tradition), and aspects of Darwin (variation and intersocietal competition with extinction or loss of political autonomy involved in the compounding of small societies into larger societies—more generally, a "survival of the fittest"). Fertility, "geologic or climatic alterations" of habitats, migrations in "many directions," "perpetual adjustments to conditions perpetually changing" with "numerous divergences and re-divergences of structures"—"branching and re-branching of species"—struggle and war, consolidation and extinction, are the key concepts (1868, 1:379; 1897, 1:95–97; 2:241, 280; 3:609–10; 1904, 1:587; 1937, p. 477; 1966*b*, 1:521). Functional analysis is a populational approach in that it assesses the effects of certain data—differences in size, fertility levels, efficiency of communication; degrees of public control of resources, military cooperation, coordination, and organization; and the like—on the "probability of success" or "social survival" of differentially endowed societies.

This fourth theory is in all important respects a populational theory. The "entire aggregate of societies which the Earth supports, from primitive hordes up to highly civilized nations," Spencer points out (1937, p. 526), "has been becoming more various in the forms of societies it includes, and is still becoming more various." The population has become more diversified; the proportion of smaller, simpler societies has gradually declined vis-à-vis larger, highly compounded societies; the internal differentiation of the "average type" (statistical mode) has increased; still "larger nations" are likely, and there has been "human progress" over time, something "measured by the degree in which simple acquisition is replaced by production; achieved first by manual-power, then by animal power, and finally by machine-power" (1897, 1:96–97; 3:362, 609–10; see also Lenski [1970, chap. 5], whose conclusions are quite similar). Finally, taking the population of societies as a whole, Spencer (1971, p.

81; 1897, 3:610) identifies, first, a progressive trend toward economic integration, with the population "growing ever more heterogeneous in respect of the separate functions assumed by separate nations" and, second, a trend toward political integration, beginning with a "federation of the highest nations," which aims at minimizing wars.

While "taking the entire assemblage of societies, evolution may be held inevitable as an ultimate effect of the co-operating factors . . . acting on them all through indefinite periods," it is not "inevitable in each particular society, or even probable"; nor is there "some intrinsic proclivity in every species towards a higher form" (1897, 1:96; 1937, p. 522). In fact, this kind of evolution, which involves the compounding of some societies and extinction of others, is largely irrelevant to individual societies (1897, 2:280, 555). What is commonly taken as Spencer's postulate of linear or unilinear development of every society, something he explicitly rejects (1897, 1:226; 2:609), is really the assumption that small and simple societies originate before larger and more complex ones (see, e.g., 1897, 1:550).

Social isolation and stability of environment mean "fixity." There are no autogenous forces in social structure or culture which necessarily impel a given society through fixed stages of change. As Spencer (1937, p. 522) once answered his critics, "all who have fully grasped the argument of this work [*First Principles*]"—and most did not—"will see that the process of Evolution is not necessary, but depends on conditions. . . ." It is only in Spencer's second theory that we find anything approaching immanent causation: that is, the development which springs from "associated men" with biopsychological properties existing in a physical setting.

While Spencer talks about species of societies and suggests a wide range of differences, his own grappling with the "problem of differences" is largely confined to his two classifications of social species—from simple through trebly compound (with internal functions and structures becoming increasingly interdependent, or more "organic," at each successive "degree of composition") and, later, into military and pacific types. Spencer's macroevolutionism also contains and is supported by a social structuralism: he hopes to show that the morphological principle of classification is also an important causal variable. That is, Spencer (see, e.g., 1897, 1:686) is interested in discovering whether cultural phenomena and institutional arrangements—kinship, religion, custom, etc.—as well as modal personality characteristics, vary with overall morphology (simple, compound, etc.) and also with main "social activity" (a predominance of war or peaceful labor). That Spencer finds few cultural phenomena or institutional arrangements to consistently vary by structurally defined social type, or that his interest, while writing the various installments of the *Principles of Sociology* (over a 20-year period), shifts more and more to

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the military-pacific dichotomy, need not concern us here. What is important to note is Spencer's clear recognition of social species and his attempt to embrace, in both bases of classification, strictly social—not biological, racial, or psychological—causal principles. Spencer's chief shortcoming is a common one: namely, no classification of societies ever does justice to the "problem of differences" or the multiplicity of causation.

In conclusion, this fourth theory is strictly analogous to biological evolution and populational thinking. Spencer's phrase "survival of the fittest," coined in his "A Theory of Population" of 1852 (reprinted in 1966*b*, vol. 1), antedates Darwin's "natural selection" by several years. The latter concept, though reinforcing Spencer's own convictions, represented nothing really new for Spencer (1966*b*, 1:548): the "process of Natural Selection is literally a survival of the fittest." What Spencer did borrow from Darwin—and apply, in conjunction with the Lamarckian "direct adaptation" or "use-inheritance," to the evolution of both organic and social species—was "spontaneous variation" (1904, 1:587; 2:116; 1897, 1:95–98). With respect to social species, "favourable variations" became an escape clause for speculating about differential military successes among societies more or less equal in size, resources, and environmental context.

#### GENERAL CONCLUSION

While Spencer is correctly understood by most critics to have applied the single term "evolution" to all orders of data (from inorganic to super-organic), it is not recognized that four separate theories or views governed the use of the single term "social evolution." (Nearly anything that seemed to display an increase in complexity was, confusingly, seen by Spencer as "evolving" or "developing.") This is doubtless responsible for much of the incongruity in today's critical literature. Also, of course, Spencer attempted to link all his theories to lofty, all-embracing "first principles"; however, each stands or falls on its own merit and immediate (physical, biological, or social) matrix.

With respect to his social theories, we may conclude that the first theory of social evolution (as initially formulated) is an example of the hoary Western belief in progress (see Nisbet 1969, pp. 160 ff., on Spencer's affinity with Comte and other 19th-century believers in progress). It has no real consistency with the others. The second, third, and fourth theories, however, are interdependent, by virtue of their all being analogues of biological processes: namely, early and advanced ontogenesis (initial functional differentiation, advancing division of labor) and phylogenesis (social speciation).

Although Spencer wrote over a 63-year period (1839–1902), added to and revised many of his views, and filled numerous volumes, the fact that

these four theories have not been distinguished before and that so many baseless criticisms have stood for so long points up the continuing need for a much-improved Spencer chapter in the history of sociology. While Spencer has been ignored and, generally speaking, dealt out of the sociological tradition, much of what he had to say is with us today. His first theory, which evolved into a model of rhythmic alternation of militarism and pacifism and which includes his laws of "sequence" and "co-existence" respecting structure, culture, and personality, directly inspired Andreski's military sociology (Fletcher 1971, p. 2) and Sorokin's (1961) theory of emergencies—indeed, Spencer's (1897, 1:739) and Andreski's (1954, p. 33) definitions of the military society are identical. The second theory, though its influence is uncertain, seems remarkably similar to current views of "functional problems" and the "general proposition" that "total societies *tend* to differentiate into sub-systems" which subserve them (Parsons and Smelser 1956, p. 47). Spencer's functional analysis, passed on in part through Durkheim, Radcliffe-Brown, and Malinowski, seems, in its essentials, but little different from the modern variant. The same may be said of the related equilibrium model, passed on, principally through Pareto, to Henderson, Parsons, Homans, and others (see Lopreato 1965, pp. 3 ff.). The third theory, taken over and much improved by Durkheim (whose understanding of the nature of contract was far superior), continues to inspire interest. The fourth theory hardly differs from the structure of much of today's neoevolutionism, as witness Parsons's (1966, p. 2) basic vocabulary (variation, selection, adaptation, differentiation, and integration) and his definition of sociocultural evolution as proceeding "by variation and differentiation from simple to progressively more complex forms" (all this is said while arguing that "the early social evolutionists fell far short of developing a truly evolutionary perspective" [Parsons 1966, p. 2 n.]). Spencer's views need to be gingerly reexamined and his relevance, both historical and contemporary, carefully reassessed. Only by such a judicious reappraisal can the demand for an accurate empirical history of sociology be satisfied and the ultimate question of Spencer's current utility be answered.

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# Social Integration and the Status-Attainment Process<sup>1</sup>

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Spady's hypothesis that level of participation in high school extra-curricular activities has a lagged effect on educational attainment is tested on a sample of 17-year-old Lenawee County, Michigan, males studied in 1957 and again in 1972 ( $N = 340$ ). The hypothesis is extended to estimate a model of the effects of adolescent social integration on occupation, income, and adult social integration. The major finding with respect to occupation and income is that incorporating adolescent social integration enhances the predictive power of the models only marginally, but such integration functions as an important mediator in the process whereby background socioeconomic status is transmitted to sons. Adolescent social integration is a strong predictor of adult social integration, and an estimate is made of the stability of the social integration construct over a 15-year interval. Analysis is by multiple regression based on correlations corrected for unreliability, a strategy relating measured variables to their latent constructs so as to make explicit the differential reliability among indicators and the bearing of validity on causal systems.

Because education has been highly valued in the United States—either for its intrinsic worth as a vehicle for occupational and financial achievement or for its symbolic value as a panacea for social ills—there is a popular concern with equality of educational opportunity and a continuing research interest in specifying the process of educational attainment (Sewell and Hauser 1972, 1975; Duncan, Featherman, and Duncan 1972). The research has produced a general model of the status-attainment process which links background socioeconomic statuses and academic ability to educational, occupational, and income attainments by means of three sequentially intervening social psychological processes, namely, academic performance, the influence of significant others, and aspirations. Other studies have focused on *within*- and *between*-school variability in an effort to isolate school effects on eventual attainments (Coleman et al. 1966;

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Jencks 1972; Alwin 1974; Sewell and Hauser 1975). The general finding from this research is that the major portion of explainable variance in educational and occupational attainments is attributable to within-school effects. Differences among high schools appear to have small effects on what happens to students after they matriculate, and differences among colleges have even less effect except, perhaps, on income differences among individuals.

The adolescent peer culture literature which, among other things, has studied the effects of high school peer groups in shaping the educational plans of students (Haller and Butterworth 1960; McDill and Coleman 1965; Duncan, Haller, and Portes 1968), points to another component of the more general status-attainment model. Although the student role is undoubtedly multidimensional, two student cultures—the one emphasizing scholarship, the other peer acceptance—have received most attention in the literature. Coleman (1961) has argued that the two roles are often mutually exclusive; Stinchcombe (1965) has argued that alienation from the academic aspects of school is associated with lack of participation in extracurricular activities and with claims for adult prerogatives; and Rehberg and Schafer (1968) have argued that athletic participation is strongly associated with educational expectations. Whether the student cultures are viewed as competitive or compatible, these studies have generally reported that the student's role in high school and the recognition he receives from his peers are important sources of success goals. Spady (1970) reports that level of student participation in high school extracurricular activities accounts for more variability in educational attainment than do background socioeconomic statuses, academic ability, or academic performance.<sup>2</sup> Otto (1975) confirms an effect of participation in high school extracurricular activities on educational attainments, statistically controlling background socioeconomic statuses, mental ability, and academic performance.

The present study extends Spady's hypothesis concerning the role of adolescent social integration in the status-attainment process.<sup>3</sup> A structural

<sup>2</sup> Spady's (1970) sample consisted of 297 male seniors from two neighboring West Coast high schools who were studied in 1963 with follow up in 1967. The schools were located in a rapidly developing suburban area adjacent to a city of half a million population. Each high school had an enrollment of nearly 1,300 students. Spady characterized the setting as a mixture of small town, semirural, and suburban modes of life, noting that the residential diversity and the wide range of paternal occupations and family incomes made the sample difficult to stereotype in terms of social class. Data analysis was by bivariate correlation and cross-tabulations.

<sup>3</sup> In the present study, which includes a stability estimate, it is useful to think of the phenomenon in terms of the generic construct, social integration, rather than in terms of the measure by which the construct is operationalized, extracurricular activities. This is not to obscure the fact, however, that the present analysis, like Spady's, operationalizes the construct with a measure of participation in high school extracurricular activities.

equations model is specified for estimating the effect of adolescent social integration on occupational and income attainments, thereby establishing the generalizability of Spady's hypothesis to alternative socioeconomic status indicators. The model also provides estimates of the effectiveness of adolescent social integration as a mediating mechanism by which the total effects of socioeconomic background statuses, academic ability, and academic performance lead to attainments. Moreover, the model provides estimates of the effect of each component on adult social integration, a noneconomic consequence of the status-attainment process. Finally, an estimate of the stability of the social integration construct over the 15-year interval from adolescence to adulthood is offered.

#### THE PROCESSUAL MODEL

Spady's thesis (1970) is that extracurricular activities, like the academic curriculum, provide opportunities for acquiring, developing, and rehearsing attitudes and skills from which status goals evolve and upon which future success is grounded. Participation in the extracurriculum provides peer group recognition which, Spady suggests, stimulates a desire for further recognition after high school. Because the most visible and widely accepted form of success striving is college attendance, educational aspirations become for many a proxy for high status and personal recognition. Spady provides evidence for the hypothesis that extent of participation in extracurricular activities accounts for variance in eventual educational attainments, independent of that explained by background socioeconomic statuses, academic ability, and academic performance. The present study goes beyond estimating additional proportions of variance accounted for in socioeconomic attainments, however. It examines the role of adolescent social integration in the *process* of status attainment (see fig. 1).

Beginning with Blau and Duncan (1967), the literature on social mobility has consistently demonstrated that family socioeconomic statuses influence son's eventual attainments (e.g., Sewell and Hauser 1972, 1975; Duncan et al. 1972). The question is by what process this effect is transmitted. I specify a structural equations model which estimates the role of adolescent social integration as an intervening variable in the status-attainment process. Past studies have reported that family background socioeconomic statuses influence academic ability and academic performance (Sewell and Hauser 1972) and that these are positively associated with levels of adolescent social integration (Spady 1970). It has been argued both that student culture emphasizes on scholarship and peer acceptance are largely mutually exclusive (Coleman 1961) and that they are causally linked (Stinchcombe 1965; Rehberg and Schafer 1968). The present specification, which indicates that academic performance (the scholastic dimension) is causally antecedent to adolescent social integra-

## Social Integration and Status Attainment

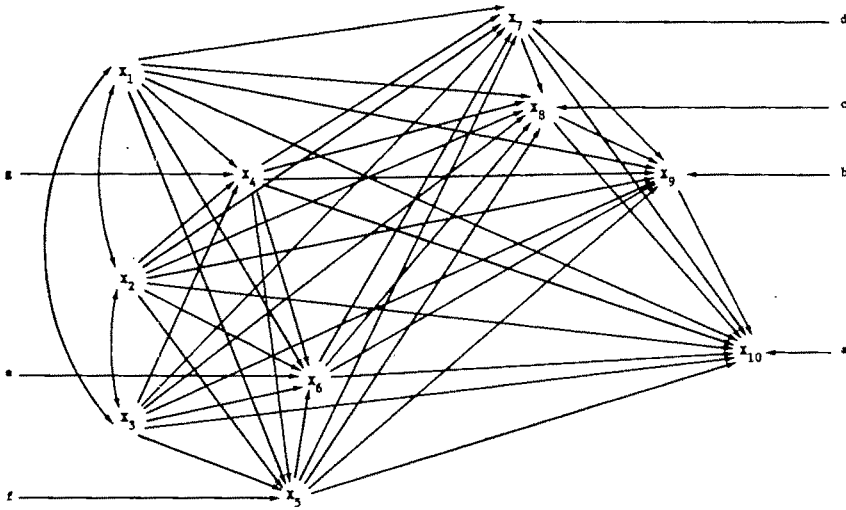


FIG. 1.—Causal diagram for a fully recursive model incorporating social integration as an antecedent and consequence in the status-attainment process.  $X_1$  = father's occupation;  $X_2$  = father's education;  $X_3$  = mother's education;  $X_4$  = academic ability;  $X_5$  = academic performance;  $X_6$  = adolescent social integration;  $X_7$  = education;  $X_8$  = occupation;  $X_9$  = income;  $X_{10}$  = adult social integration.

tion (the peer acceptance dimension), is governed by the temporal sequence of the measures: in the course of the study, "academic performance" referred to a grade-point average established over the preceding four years, whereas the adolescent social integration indicator was a present-tense operationalization. The relationship between academic performance and adolescent social integration may be enhanced by high school policies that require minimal academic performance levels as eligibility requirements for participation in some extracurricular activities. Additional evidence for the relationship between background socioeconomic status and adolescent social integration is provided by Hyman and Wright (1971), who report that children from families of higher statuses are considerably more likely to be members of youth associations than children from lower-class milieus. In summary, past research indicates that adolescent social integration is in part a function of family socioeconomic statuses and that adolescent social integration has a salutary influence on respondents' eventual educational attainments. I reason that if adolescent social integration provides socialization experiences for attitudes and skills that have lagged educational attainment payoffs, these benefits might also influence occupational and income attainments. I estimate the additional proportions of variance in these statuses accounted for by adolescent social integration and estimate how effectively adolescent social integration mediates the total effects of family background socioeconomic statuses,



academic ability, and academic performance on respondents' occupation and income.

The positive relationship between adult socioeconomic statuses and membership in voluntary organizations is well documented in the sociological literature (standard references include Wright and Hyman [1958] and Hyman and Wright [1971]). The model of that relationship was refined by Hodge and Treiman (1968; see also Hauser 1969), who revealed differences in the estimated effects of various measures of socioeconomic status on different aspects of social participation; specifically, education and income exert strong influences on organizational memberships but occupation does not. Having estimated the lagged effects of adolescent social integration on occupation and income, I extend the model further by specifying adult social integration as the final dependent variable. Following Hagedorn and Labovitz (1968, p. 274), I reason that the socialization process, which includes both formal and informal education, instills societal values and norms in the individual. I assume, moreover, that certain aspects of the socialization process are more important than others for inculcating specific behavioral patterns. For example, formal education is a crucial factor in determining levels of adult social involvement because it instills the value that community participation is socially desirable. Education also provides knowledge of opportunities for participation and information on such practicalities as how to seek and carry out the duties of an office (Foskett 1955). Thus, education uniquely influences subsequent levels of social participation in ways not necessarily attributable to the companion status indicators, occupation and income.

The model also specifies that income and possibly occupation influence adult social integration, although, as indicated earlier, Hodge and Treiman (1968) failed to confirm a relationship between occupation and social integration. My estimates will provide independent evidence concerning this relationship. I expect to find a relationship between income and social integration inasmuch as the forms of social integration studied here, organizational memberships and political participation, commonly require at least minimal financial expenditures in the form of initiation fees, annual dues, or campaign contributions. Hence, income adequacy may be viewed as a necessary condition for social integration (Shuval 1956; Hodge and Treiman 1968), and increased discretionary income may be viewed as facilitating these forms of social integration.

Finally, given indicators of adolescent and adult social integration, I estimate the stability of the construct over the 15-year interval from the time 1 adolescent measure to the time 2 adulthood indicator. The estimate is made statistically controlling for all antecedent variables and processes specified in the model: background socioeconomic statuses, academic ability, and academic performance. Therefore, the estimate is free of

spurious association due to common antecedents. Moreover, because all estimates are based on correlations corrected for attenuation, the stability coefficient is an estimate of true parameters, free of unreliability.

#### DATA, VARIABLES, AND METHODS

The analysis is based on a sample of 17-year-old males who were enrolled in Lenawee County, Michigan, high schools in 1957 and participated in a follow-up study in 1972. The original sample ( $N = 442$ ) included all but that 12% of the entire age cohort who had taken full-time jobs and were no longer enrolled in high school in 1957.<sup>4</sup> In 1972 a concentrated tracking effort located 352 of the original respondents, of whom 327 consented to a telephone interview and provided data on life-cycle histories and current levels of socioeconomic attainments. Respondents were also asked to complete and return a mailback questionnaire which included measures of adult social integration. Thirteen respondents who were inaccessible by telephone completed a special questionnaire providing the same information. The procedures yielded data from 340 respondents (79% of the eligible 1957 participants).<sup>5</sup> Analyses are based on bivariate data-present subsamples.

Background socioeconomic statuses appear in the model as exogenous variables. Father's occupation was coded into the Duncan (1961) Socioeconomic Index. Father's education and mother's education were scored as follows: 0 for less than eight grades of formal education completed, 1 for eight grades, 2 for 9–11 grades, 3 for 12 grades, 4 for some college, and 5 for a college degree or more education. The model allows background socioeconomic statuses to influence academic ability, which was assessed by the Cattell IPAT Test of G-Culture Free-Scale 3A (Cattell and Cattell 1950). Academic performance is influenced by family background statuses and academic ability and was assessed by high school grade-point average calculated on a four-point scale and retrieved directly from high school records. Adolescent social integration was indicated by the kind of activities in which the respondent regularly participated. Respondent's score consisted of the total number of activities noted on a 14-item check list.<sup>6</sup> Background socioeconomic statuses, academic ability, academic performance, and adolescent social integration were measured in 1957.

<sup>4</sup> To the extent this exclusion introduces bias, the 1957 data are not truly representative of the entire age group residing in Lenawee County at the time of the original study. Follow-up studies showed that about five of six dropouts were sons of farmers (Jarrett and Haller 1964, p. 167). Thus, strictly, the findings of this study are generalizable to the 17-year-old male youth enrolled in Lenawee County, Michigan, high schools in the spring of 1957.

<sup>5</sup> Four of the 1957 respondents were excluded from the analysis because of invalid data and eight were known to be deceased, leaving a total of 430 eligible respondents in 1972.

<sup>6</sup> The checklist of high school extracurricular activities included athletics, band-

Socioeconomic attainments included respondent's level of education, occupational prestige, and income, which were reported in 1972. Education referred to number of years of formal schooling completed. Respondent's occupation was coded into the Duncan (1961) Socioeconomic Index. Income referred to respondent's reported total annual earnings for the preceding year, 1971.

Adult social integration is unobserved, entering the model as the final dependent variable. It was reflected in two indicators, both taken in 1972. Extent of membership was ascertained from a checklist of 15 voluntary organizations whereby respondents indicated the total number of their memberships.<sup>7</sup> A second social integration measure consisted of six political participation items eliciting the extent of respondent's involvement at various levels in the political process.<sup>8</sup>

Before estimating the structural equations implicit in the conceptual model, validity coefficients<sup>9</sup> for all variables were estimated or assumed<sup>10</sup>

orchestra, chorus-vocal, dramatics, debate, 4-H or FFA, school paper, annual, student government, hobby club, and four opportunities for designating "other."

<sup>7</sup> The membership list included church groups, labor unions, veterans' organizations, fraternal organizations or lodges, business or civic groups, parent-teacher associations, community centers, organizations of people of the same nationality, sport teams, country clubs, youth groups, professional groups, political clubs or organizations, neighborhood improvement associations, charity or welfare organizations, and two open-ended response possibilities.

<sup>8</sup> The six questions on political activity were (a) In recent elections did you make a campaign contribution, buy tickets, go to any political rallies, barbecues, fish fries, or anything like that in support of a candidate? (b) Did you talk to people trying to get them to vote for or against any candidate? (c) What about recent elections for local or county offices—like sheriff or mayor? How often have you voted in these elections? (d) In recent months have you ever tried to influence the opinions of others on some local issue by calling a meeting, circulating a petition, testifying at a hearing, or doing something like that? (e) In recent months have you ever written a letter to a newspaper or public official, telephoned or talked directly with a public official regarding a public issue that concerned you? (f) Have you ever contacted an elected official to seek an adjustment on a personal matter? A score of 1 was assigned for each category of participation, and the scores were summed to provide respondent's score.

<sup>9</sup> Validity, in this sense, is defined as the correlation between a true score and an observed score. This definition of validity presupposes the definition of reliability as the squared correlation between the true and observed scores (see Lord and Novick 1968, pp. 55–63). These definitions assume two sources of variation in observed scores, a reliable component representing the true underlying dimension and an unreliable component representing random error of measurement. However, it is possible that the observed score reliably measures something in addition to or instead of the underlying true score of interest, e.g., some kind of nonrandom response bias. Unfortunately, such sources of variation can rarely be specified, with the result that validity estimates may be exaggerated. This possibility prompts consideration of the square root of reliability as a ceiling estimate of validity (see Lord and Novick 1968, p. 72). Alwin (1973, pp. 259–60) has discussed these definitions and relationships at greater length.

<sup>10</sup> I assumed the following validity coefficients: for current socioeconomic status vari-

for the purpose of establishing reliabilities and estimating the intercorrelations of the unobserved and observed variables, corrected for unreliability.<sup>11</sup> In estimating the intercorrelations I estimated an overidentified factor-analytic model using Jöreskog's confirmatory factor-analysis procedures which provide maximum-likelihood estimates (Jöreskog 1970; Jöreskog, Gruvaeus, and van Thillo 1970). Thus, all estimates are based on correlations corrected for attenuation, a strategy that relates measured variables to their (unmeasured) latent constructs so as to make explicit the differential reliability among indicators and the bearing of validity on causal systems. Validity coefficients, means, standard deviations, and zero-order correlations, both corrected and uncorrected for attenuation, are presented in table 1.

## RESULTS

My procedure is to examine the estimates of the conceptual model displayed in figure 1 and in that process to analyze the parameters addressed in this study. I first briefly summarize the pattern of antecedent effects on education that are common to the later stages of the model (these were reported in detail in Otto [1975]). Thereafter, I systematically and sequentially examine the total, indirect, and direct effects on the dependent variables of particular interest to this study: occupation, income, and adult social integration. The data are displayed in tables 2 and 3, following the conventions for interpreting causal theories in sociology suggested by Alwin and Hauser (1975). (Readers who are unacquainted with the distinction between associations and effects, or with the decomposition of effects into direct and indirect components, may find it useful to consult this work.) In presenting the analysis, use of the qualifier "significant" is reserved for relationships that are statistically significant by the conventional criterion that the absolute size of the coefficient must be at least twice as large as its standard error.

Table 2 presents a résumé of the antecedent effects on  $X_7$  (education). The three family background socioeconomic statuses— $X_1$  (father's occu-

ables, the ones provided by Siegel and Hodge (1968, p. 37); for son's report of parental statuses, those provided by Mason et al. (1972); and for the mental ability test, that reported by Cattell and Cattell (1950). Academic performance was assumed to be valid inasmuch as the information was retrieved directly from school records. All other coefficients, except that for adolescent social integration, were calculated as the square root of the index reliabilities estimated by the formula for Cronbach's  $\alpha$  (Nunnally 1967, p. 223). So that the estimates for the effects of adolescent social integration would be *conservative*, I assumed that the reliability of the report on adolescent social integration was at least as high as the reliability of son's report of father's occupation and education. Validity coefficients are reported in table 1.

<sup>11</sup> The construction of unobserved variables for use in path analysis was introduced to the sociological literature by Hauser and Goldberger (1971), was elaborated by Burt (1973), and is illustrated by Goldberger and Duncan (1973) and others.

TABLE 1

ZERO-ORDER CORRELATIONS, MEANS, STANDARD DEVIATIONS, AND VALIDITY COEFFICIENTS FOR ALL VARIABLES UNCORRECTED  
(UPPER RIGHT OF DIAGONAL) AND CORRECTED (LOWER LEFT OF DIAGONAL) FOR UNRELIABILITY

Variables	$X_1$	$X_2$	$X_3$	$X_4$	$X_5$	$X_6$	$X_7$	$X_8$	$X_9$	$X_{10}$	$X_9$	$X_8$
$X_1$ .....	956*	460	312	227	252	126	311	308	164	...	022	073
$X_2$ .....	481	958*	562	220	247	230	346	240	199	...	040	181
$X_3$ .....	340	611	883*	254	309	255	360	233	174	...	-010	233
$X_4$ .....	278	268	329	837*	484	193	419	362	099	...	-004	129
$X_5$ .....	258	252	329	578	1*	345	621	539	127	...	042	243
$X_6$ .....	136	250	293	247	366	960*	442	344	183	...	299	300
$X_7$ .....	342	379	406	537	666	483	966*	658	196	...	185	325
$X_8$ .....	367	285	288	502	626	415	817	928*	184	...	154	280
$X_9$ .....	197	239	217	139	149	217	247	251	923*	...	121	268
$X_{10}$ .....	093	207	264	165	271	406	406	374	343	...	...	...
$X_6$ .....	...	...	...	...	...	...	...	...	...	...	530*	439
$X_8$ .....	...	...	...	...	...	...	...	...	...	...	...	829*
$\bar{X}$ .....	32.65	2.27	2.58	20.95	2.09	2.36	13.48	47.05	12.894	...	3.21	1.87
SD .....	22.05	1.36	1.29	5.23	0.84	1.48	2.50	25.21	6.993	...	2.74	1.64

NOTE.—The variables are  $X_1$  = father's occupation;  $X_2$  = father's education;  $X_3$  = mother's education;  $X_4$  = academic ability;  $X_5$  = academic performance;  $X_6$  = adolescent social integration;  $X_7$  = education;  $X_8$  = occupation;  $X_9$  = income;  $X_{10}$  = adult social integration;  $X_9$  = organizational memberships;  $X_8$  = political participation. Decimals are omitted.

\* Validity coefficients.

TABLE 2

ANTECEDENTS FOR EACH STAGE OF THE EDUCATIONAL ATTAINMENT PROCESS; TOTAL, INDIRECT, AND DIRECT EFFECTS  
ON EDUCATIONAL ATTAINMENT; AND MULTIPLE CORRELATION COEFFICIENTS

VARIABLES	$X_4$	$X_5$	$X_6$	$X_7$	VARIABLES	Total	EFFECT			
							Via $X_4$	Via $X_5$	Via $X_6$	Direct
$X_1$ .....	179* (060)	070 (052)	-043 (060)	093* (043)	$X_1$ .....	192*	076 (39)	034 (18)	-010 (-5)	093* (48)
$X_2$ .....	029 (071)	-004 (061)	107 (070)	090 (051)	$X_2$ .....	125	012 (10)	-002 (-2)	025 (20)	090 (72)
$X_3$ .....	251* (066)	139* (058)	141* (067)	058 (049)	$X_3$ .....	264*	106 (40)	067 (26)	033 (13)	058 (22)
$X_4$ .....	...	514 (049)	013 (064)	169* (046)	$X_4$ .....	422*	...	250 (59)	003 (1)	169* (40)
$X_5$ .....	...	...	297* (064)	416* (048)	$X_5$ .....	486*	...	...	070 (15)	416 (86)
$X_6$ .....	...	...	...	237* (041)	$X_6$ .....	237*	...	...	...	237* (100)
$R^2$ .....	140	360	174	572	$R^2(X_1, X_2, X_3) = 220$ $R^2(X_1, X_2, X_3, X_4) = 374$ $R^2(X_1, X_2, X_3, X_4, X_5) = 525$ $R^2(X_1, X_2, X_3, X_4, X_5, X_6) = 572$					

NOTE.—For variables, see table 1; on the left, standard errors are shown in parentheses; on the right, percentages are shown in parentheses; decimals omitted. Summary of effects reported in Otto (1915).

\* Absolute value for the coefficient is at least twice as large as the standard error and the relationship

pation),  $X_2$  (father's education), and  $X_3$  (mother's education)—account for 22% of the variance in the dependent variable. The same group of regressors explains 14% of the variance in  $X_4$  (academic ability). Incorporating academic ability into the reduced-form model produces a third significant total effect and nearly doubles the variance in education accounted for ( $R^2 = .374$ ). These antecedents account for 36% of the variance in  $X_5$  (academic performance), which also has a significant total effect on educational attainment and increases the proportion of variance accounted for to 53%. In combination, academic ability and performance mediate nearly two-thirds of the total effect of father's occupation and mother's education on educational attainment. Regressing  $X_6$  (adolescent social integration) on background socioeconomic statuses, academic ability, and academic performance accounts for 17% of the variance, with positive effects contributed by mother's education and academic performance. Finally, incorporating adolescent social integration into the structural equation increases the variance in education accounted for to 57%, an increment of nearly 9% over that accounted for apart from adolescent social integration. Moreover, inspection of the pattern of indirect effects reveals that adolescent social integration mediates 15% of the total effect of academic performance on education.

These findings (*a*) confirm earlier research demonstrating the significance of academic ability and performance as predictors of educational attainment and the role of the two as mediators of background socioeconomic status effects on educational attainment (Sewell, Haller, and Portes 1969; Sewell, Haller, and Ohlendorf 1970; Sewell and Hauser 1972, 1975; Duncan et al. 1972); (*b*) replicate Spady's (1970) finding—that extracurricular activities contributed an independent effect on educational attainment, controlling for background socioeconomic statuses, academic ability, and academic performance—using a less restrictive sample and measures separated by a longer time interval; and (*c*) demonstrate that extracurricular activities also serve a mediating function in the process of status transmission.

### Occupational Attainment

The pattern of antecedent effects and the role of adolescent social integration in the  $X_8$  (occupational attainment) process is displayed in table 3. The reduced-form model regressing occupational attainment on background socioeconomic statuses accounts for 17% of the variance in occupational attainment, which is the total effect for which I first seek to account by the conceptual model. That alternative indicators of socioeconomic status may respond differentially to particular dependent variables has been noted and discussed by others (Hauser 1970; Hauser, Lutterman,

TABLE 3

VARIABLE	EFFECT							Direct
	Total	Via $X_4$	Via $X_5$	Via $X_6$	Via $X_7$	Via $X_8$	Via $X_9$	
$X_9$ —Occupation								
$X_1$ .....	290*	074 (26)	033 (11)	—009 (—3)	062 (21)	...	...	127* (44)
$X_2$ .....	048	012 (25)	—002 (—4)	022 (45)	062 (130)	...	...	—046 (—97)
$X_3$ .....	160*	104 (65)	066 (41)	029 (18)	040 (25)	...	...	—078 (—48)
$X_4$ .....	414*	...	244 (59)	003 (1)	117 (28)	...	...	050 (12)
$X_5$ .....	474*	...	...	060 (13)	288 (61)	...	...	125* (27)
$X_6$ .....	203*	...	...	...	164 (81)	...	...	039 (19)
$X_7$ .....	693*	...	...	...	...	...	...	693* (100)
$R^2(X_1, X_2, X_3) = 166$								
$R^2(X_1, X_2, X_3, X_4) = 313$								
$R^2(X_1, X_2, X_3, X_4, X_5) = 457$								
$R^2(X_1, X_2, X_3, X_4, X_5, X_6) = 491$								
$R^2(X_1, X_2, X_3, X_4, X_5, X_6, X_7) = 697$								
$X_9$ —Income								
$X_1$ .....	100	009 (9)	004 (4)	—006 (—6)	013 (13)	020 (20)	...	061 (61)
$X_2$ .....	126	001 (1)	000 (0)	016 (13)	012 (10)	—007 (—6)	...	104 (83)
$X_3$ .....	106	012 (12)	008 (7)	022 (20)	008 (7)	—012 (—12)	...	070 (66)
$X_4$ .....	049	...	028 (56)	002 (4)	023 (46)	008 (16)	...	—011 (—23)
$X_5$ .....	054	...	...	044 (82)	056 (103)	020 (37)	...	—067 (—122)
$X_6$ .....	150*	...	...	...	032 (21)	006 (4)	...	111 (75)
$X_7$ .....	135	...	...	...	...	111 (82)	...	024 (18)
$X_8$ .....	160	...	...	...	...	...	...	160 (100)
$R^2(X_1, X_2, X_3) = 073$								
$R^2(X_1, X_2, X_3, X_4) = 075$								
$R^2(X_1, X_2, X_3, X_4, X_5) = 077$								
$R^2(X_1, X_2, X_3, X_4, X_5, X_6) = 095$								
$R^2(X_1, X_2, X_3, X_4, X_5, X_6, X_7) = 103$								
$R^2(X_1, X_2, X_3, X_4, X_5, X_6, X_7, X_8) = 111$								



TABLE 3 (Continued)

VARIABLE	EFFECT							
	Total	Via $X_4$	Via $X_3$	Via $X_6$	Via $X_7$	Via $X_8$	Via $X_9$	Direct
		$X_{10}$ —Adult Social Integration						
$X_1$ .....	—022	016	016	—014	027	022	014	—103
		(—73)	(—72)	(64)	(—125)	(—101)	(—64)	(472)
$X_2$ .....	082	003	—001	035	027	—008	023	004
		(3)	(—1)	(42)	(32)	(—10)	(29)	(5)
$X_3$ .....	221*	022	031	046	017	—014	016	103
		(10)	(14)	(21)	(7)	(—6)	(7)	(46)
$X_4$ .....	089	...	116	004	050	009	—003	—087
			(130)	(5)	(57)	(10)	(—3)	(—98)
$X_5$ .....	225*	...	...	096	123	022	—015	—001
				(43)	(55)	(10)	(—7)	(—1)
$X_6$ .....	325*	...	...	...	070	007	025	223*
					(22)	(2)	(8)	(69)
$X_7$ .....	296*	...	...	...	...	121	005	170
						(41)	(2)	(57)
$X_8$ .....	174	...	...	...	...	...	037	138
							(21)	(79)
$X_9$ .....	228*	...	...	...	...	...	...	228*
								(100)
$R^2(X_1, X_2, X_3) = 074$								
$R^2(X_1, X_2, X_3, X_4) = 080$								
$R^2(X_1, X_2, X_3, X_4, X_5) = 113$								
$R^2(X_1, X_2, X_3, X_4, X_5, X_6) = 200$								
$R^2(X_1, X_2, X_3, X_4, X_5, X_6, X_7) = 237$								
$R^2(X_1, X_2, X_3, X_4, X_5, X_6, X_7, X_8) = 246$								
$R^2(X_1, X_2, X_3, X_4, X_5, X_6, X_7, X_8, X_9) = 292$								

NOTE.—For variables, see table 1; percentage estimates shown in parentheses; decimals omitted.

\* Absolute value for the coefficient is at least twice as large as the standard error and the relationship is defined as statistically significant.

and Sewell 1971; Hodge 1970) and need not be pursued here. Adding academic ability to the equation nearly doubles the variance in occupational attainment accounted for ( $R^2 = .313$ ). The effectiveness of academic ability in transmitting background socioeconomic status effects is evident in that it mediates 26% of the total effect of father's occupation and 65% of the effect of mother's education on occupational attainment. Incorporating academic performance into the equation increases the variance in occupational attainment accounted for to 46%. The importance of academic performance in mediating antecedent influences on occupation is also evident in that, for example, it mediates 59% of the effect of mental ability on occupation. Academic ability and performance combine to transmit from 37% to 106% (discussed below) of background socioeconomic status effects on occupation. As in the educational attainment process,

the two variables function as key linkages by which the advantage or disadvantage of parental statuses is transferred to son's early career occupational attainment. These results support those earlier reported by Sewell and Hauser (1972, 1975).<sup>12</sup>

That the total effect of adolescent social integration on occupational attainment is significant addresses the first question posed by this analysis: whether adolescent social integration influences socioeconomic attainments other than education. Incorporating the variable into the model increases the predictive power of the equation for occupational attainment ( $R^2 = .491$ ). Adolescent social integration selectively mediates background socioeconomic effects, transmitting 18% of the effect of mother's education and 13% of the influence of academic performance on occupational attainment. I conclude from this analysis that adolescent social integration contributes to the occupational attainment process both as a predictor of variance and as a mediator of antecedent effects.

The key role of education in the occupational attainment process (consistently noted by Sewell et al. [1969, 1970] and Sewell and Hauser [1972, 1975]) is confirmed by the present study. Incorporating the effects of educational attainment into the equation predicting occupational attainment increases the variance explained by 42% over that explained apart from education ( $R^2 = .697$ ). Moreover, education transmits at least one-fifth and as much as four-fifths of the total effect of each significant antecedent on occupational attainment. As predicted, education is an effective medium by which the effects of background socioeconomic statuses are transmitted to respondent's occupational attainments, and education conveys substantial proportions of the total effect of academic performance and adolescent social integration on occupational attainment.

The direct effects indicate the proportion of total effects that remain unmediated by the process specified. Forty-four percent of the total effect of father's occupation on son's occupational attainment is direct, which may be interpreted as the amount of status inheritance operating in the sample. The model is especially efficient in providing linkages for all but 12% of the total effect of academic ability on occupational attainment. All but one-fifth of the influence of adolescent social integration is mediated by the model, as is more than two-thirds of the total effect of academic performance on occupational attainment. In summary, the model is an

<sup>12</sup> No attempt is made to compare systematically estimates produced in this analysis with those reported from the Wisconsin data for several reasons: the variables are operationalized differently; estimates reported by Sewell and Hauser (1972, 1975) and those reported here are standardized rather than unstandardized regression coefficients; the present estimates are made based on correlations corrected for unreliability whereas the Wisconsin estimates are not; and the specifications of the models differ in important respects. Analysis of a thorough comparison of the two data sets is under way and will be reported upon completion.

efficient specification of the occupational attainment process, and adolescent social integration is a noteworthy mechanism operating within the process.

The performance characteristics of two variables in the model require special consideration. The first is father's education, which does not exhibit a significant total effect on occupational attainment. Consequently, no analysis of the mechanisms by which its effect is transmitted is warranted. (Although the effect of father's education on son's occupational attainment is not significant, the reader is cautioned against concluding that, therefore, mothers have a greater effect on sons' occupational attainments than do fathers, for the influence of fathers upon their sons is averaged across two variables, father's occupation and father's education, whereas the influence of mothers is represented by a single variable, mother's education.)<sup>13</sup> The second variable requiring further attention is mother's education, which has a significant positive total effect on occupational attainment but a negative direct effect. Otherwise stated, more than the total effect of mother's education on occupation is mediated by components of the model, that is, 148%. This occasional occurrence is an example of the generic problem of suppressor effects (Alwin and Hauser 1975, p. 43, n. 3) evident where direct and indirect effects counteract one another and the sum of the unsigned components is larger than unity. Interpretation of this occasional circumstance is problematic, for suppressor effects make salient the fact that the variables operate within a more complicated context which is excluded from consideration by the decomposition of total effects into direct and indirect effects. Following the suggestion of Alwin and Hauser (*ibid.*), it is "possible" to express the various components as proportions of the sum of their absolute values. Thus calculated, 33% of the absolute effect is attributable to academic ability, 21% to academic performance, 9% to adolescent social integration, 13% to education, and 25% to mother's education. The latter is negative, of course. Although the solution removes the incongruity of giving a positive total effect to a sizable negative direct effect, the resolution is not a totally satisfactory expedient. In essence, it abandons the decomposition method of interpreting causal systems, which governs the rest of the analysis, and substitutes a quantification of the relative impact of the various components based on the sum of their absolute values rather than on the total effect of the independent variable on the dependent variable.

In conclusion, the pattern of antecedent effects on occupational attainment was estimated. Total effects, indirect effects, and direct effects of background socioeconomic statuses, academic ability, academic perfor-

<sup>13</sup> For a discussion of the averaged effects of father's occupation and education compared with the effect of mother's education on academic motivation, see Featherman and Carter (1976).

mance, adolescent social integration, and education were calculated. The structural equation accounted for 70% of the variance in occupational attainment. Father's occupation, mother's education, academic ability, academic performance, adolescent social integration, and education all exhibited significant total effects. The efficacy of the model as an explanation of the process of occupational attainment can be assessed in part by the fact that of the six variables exhibiting significant total effects, only three exhibited significant direct effects when antecedents were statistically controlled. The mediating role of each intervening variable was analyzed. Academic ability, academic performance, and, especially, education were key variables in the status-transmission process, as previous research has demonstrated. With respect to adolescent social integration, excluding the variable from a structural equation including education (data not reported here) does not minimize the predictive power of the model. However, adolescent social integration did mediate from one-eighth to one-sixth of the total effect of mother's education and academic performance on occupational attainment. I conclude from this analysis that although incorporating adolescent social integration does not increase the predictive power of the model, the variable does function as a mediator of indirect antecedent effects in the process of occupational attainment.

### Income

Background socioeconomic statuses alone account for 7% of the variance in  $X_9$  (income), though father's occupation, father's education, and mother's education do not exhibit significant total effects on the dependent variable. Academic ability and academic performance do not have significant total effects on income, and considering them in combination does not add to the predictive power of the model. Similarly, neither education nor occupation has a significant total effect on income. The only significant total effect in the income equation is the variable of special interest in this analysis, adolescent social integration. The effect of adolescent social integration is primarily direct. Seventy-five percent of the total effect is unmediated by the model; 21% is transmitted by education, and 4% is mediated by occupation. Incorporating adolescent social integration into the equation predicting income increases the variance explained ( $R^2 = .095$ ). Although the absolute increase in variance accounted for is less than 2%, the increase in the proportion of variance explained attributable to adolescent social integration is 23%. Adding the effect of education and income to the structural equation increases the predictive power of the model ( $R^2 = .111$ ).

Models of the status-attainment process have been comparatively ineffective as predictors of income variability and as specifications of the

process whereby background socioeconomic effects on income are mediated (Sewell and Hauser 1972, 1975). This is partly due to the fact that antecedents have been selected for their potential as predictors and mediators of effects on education and occupation but not on income. Further, the dependent variable has not been conceptualized and operationalized in terms of income curves that allow for the fact that different occupations provide highest earnings at different stages in the occupational career. Thus, manual workers may have their peak earnings early in their career whereas professionals and managerial types may have highest earnings at later stages. It is true also of my model that it accounts for considerably less variance in income than in either education or occupation. It is noteworthy, however, that the variable of interest in the present study, adolescent social integration, exhibits the only significant total effect on income, and that effect is primarily direct. The 25% mediated by education and occupation, while small, is sufficient to render the remaining direct effect statistically insignificant, however. I conclude from this analysis that adolescent social integration is an important component of the income-predicting model. Sewell and Hauser (1975) have demonstrated an as yet unexplained significant direct effect of parental income on son's earnings. Whether adolescent social integration has a significant total effect on son's income—controlling for parental income—or functions instead as a mechanism whereby parental economic advantage is transmitted to sons, is a question which must await the development of new data sets: the data at hand do not include a parental income variable and the Wisconsin data do not include a measure of adolescent social integration. The efficacy of adolescent social integration as a mediating variable could not be assessed because none of the other variables exhibited a significant total effect on income that might be transmitted to attainments via the intervening mechanisms specified.

### Adult Social Integration

The final dependent variable is  $X_{10}$  (adult social integration). The reduced form of the model, regressing adult social integration on background socioeconomic statuses, accounts for 7% of the variance, with mother's education exhibiting the only significant total effect. Academic ability does not have a significant total effect but does mediate 10% of the total effect of mother's education on the dependent variable. Academic performance has a significant total effect and mediates 14% of the total effect of mother's education on adult social integration; taking it into account increases the predictive power of the model ( $R^2 = .113$ ). Adolescent social integration has a significant total effect on adult social integration; it mediates 21% of the total effect of mother's education and 43% of the

effect of academic performance. Correspondingly, the proportion of variance accounted for by the model is nearly doubled ( $R^2 = .200$ ). Education exhibits a significant total effect on adult social integration; the variance explained by the model is increased to 24%. Moreover, the key role of education as a mediator of effects is again illustrated in that, for example, it transmits 55% of the total effect of academic performance on adult social integration. The total effect of occupation on adult social integration is not significant. Finally, income has a significant total effect on adult social integration, as predicted. It serves modestly as a mediator of significant total effects on adult social integration and acts as a suppressor with respect to academic performance. Incorporating income into the structural equation increases the variance in adult social integration accounted for to 29%.

The final purpose of this study is to estimate the stability of the social integration construct over the interval from adolescence to adulthood. The strongest total effect on adult social integration is that produced by adolescent social integration. In estimating the stability of the social integration construct, one is interested in the relationship of the time 1 indicator to the time 2 indicator when antecedent effects are controlled. Statistically controlling background socioeconomic statuses, academic ability, and academic performance, the  $\beta$  coefficient is .325, which is an estimate of stability free of spurious associations due to common causes. Inasmuch as they are made on correlations corrected for unreliability, the estimates are of true parameters, free of unreliability. Moreover, the stability estimate is conservative inasmuch as the reliability of the measure is assumed to be as high as that of any other variable in the model.<sup>14</sup> The stability coefficient is not a high one, nor would I expect it to be for an interval of 15 years. However, the coefficient is the largest influencing adult social integration, and although considerably less than perfect (unity), the comparative magnitude of the coefficient is not inconsistent with the assumption that the time 1 and time 2 indicators are tapping the same underlying dimension.

In conclusion, adult social integration was specified as a noneconomic consequence of the status-attainment process. Antecedents estimated were background socioeconomic statuses, academic ability, academic performance, adolescent social integration, education, occupation, and income. The model accounted for 29% of the variance in the final dependent variable. Mother's education, academic performance, adolescent social integration, education, and income each exhibited significant total effects on adult social integration. About half of the effect of mother's education and nearly one-third of the effect of education were transmitted by the

<sup>14</sup> For a discussion of the consequences of under- and overestimating measurement reliability, see Duncan, Featherman, and Duncan (1972, chap. 6).

linkages specified. Sixty-nine percent of the total effect of adolescent social integration and, of course, the total effect of income remained direct. All of the total effect of academic performance was mediated by the model. The largest contributor to adult social integration was adolescent social integration, which was interpreted as a stability coefficient for the underlying social integration construct over the 15-year interval from adolescence to adulthood. In addition, the variable mediated a fifth of the total effect of mother's education and two-fifths of the total effect of academic performance on adult social integration. These results support the findings of Hodge and Treiman (1968), who demonstrated that education and income have significant effects on adult social integration but that occupation does not. The analysis also identifies adolescent social integration as a key mechanism by which the effects of mother's education and academic performance on adult social integration are transmitted.

With respect to the parameters of special interest to this study, the analysis revealed the following results. First, incorporating adolescent social integration increased only marginally the predictive power of the structural equations for occupational attainment and income. Second, adolescent social integration functioned as a mediator in the process whereby background socioeconomic statuses were transmitted to sons. Third, of current socioeconomic statuses, education and income exhibited significant total effects on adult social integration, but occupation had no effect. Fourth and finally, estimating the antecedents of adult social integration revealed that adolescent social integration was the strongest predictor from among the several components of the status-attainment process. The independent effect of adolescent social integration on adult social integration, statistically controlling for background socioeconomic statuses, academic ability, and academic performance, was interpreted as an indicator of the stability of the construct over a 15-year interval.

#### DISCUSSION AND CONCLUSIONS

Since Blau and Duncan (1967) documented the status-transmission phenomenon in contemporary U.S. society, research has been proceeding on several related fronts in an effort to explicate the achievement process especially as it relates to education and occupation, but also as the process applies to economic and noneconomic consequences of attainments. Representative of these efforts have been attempts to identify the mechanisms by which parental statuses influence son's attainments (Sewell et al. 1969, 1970; Sewell and Hauser 1972, 1975); to specify and estimate alternative explanations of the process (Featherman 1972); to estimate the influence of additional exogenous variables and the impact of various intervening variables, including career contingencies (Duncan et al. 1972); to examine

school effects (Alwin 1974); to refine measurement and estimation procedures (Hauser 1970; Hauser and Goldberger 1971); and to begin to explore the noneconomic consequences of attainments (Otto and Featherman 1975), as suggested by Sewell and Hauser (1972). This analysis contributes to that broader effort in two important ways. First, the analysis has provided estimates of the effect of adolescent social integration on attainments and as a mediator of background socioeconomic statuses on attainments. Second, the study has examined the antecedents of adult social integration as a noneconomic consequence of the status-attainment process. Compared with the attention that has been lavished on the antecedents of attainments, relatively little has been given to examining the consequences of attainments.

This analysis has examined the generalizability of Spady's (1970) hypothesis that participation in the extracurriculum is positively associated with eventual educational attainments. The hypothesis was that if adolescent social integration facilitates the acquisition, development, and rehearsal of achievement-related attitudes and skills, the benefits should be generalizable to attainments other than education. The results support the hypothesis inasmuch as adolescent social integration is the only variable specified by the structural equations that exhibits a significant total effect on all three socioeconomic attainment indicators. In reduced-form models, incorporating adolescent social integration contributes to the predictive power of the equations. However, four-fifths of the total effect of adolescent social integration on occupation is mediated by education, and one-fourth of the total effect on income is mediated by education and occupation, with the result that, according to the fully specified equations, adolescent social integration does not have a significant direct effect on either occupation or income; taking it into account does not contribute to the predictive power of the occupation equation, but does increase predictability in the income equation.

Apart from examining the predictive power of attainment models incorporating the effect of adolescent social integration, the analysis has also reported the operation of the variable as a mechanism by which parental socioeconomic statuses are passed to sons. According to my model, adolescent social integration is not as effective in transmitting background socioeconomic effects on education, occupation, and income as is the Wisconsin specification. Indeed, substantial proportions of the influence of background variables on attainments is direct compared with the 30%–45% direct effects reported by Sewell and Hauser (1972, 1975). The increased efficiency of the Wisconsin model as a specification of the process of status attainment is due to the fact that it incorporates the effects of significant others and aspirations as intervening linkages. Though the conceptual model embodied in this study differs from those previously



estimated (by Sewell et al. 1969, 1970; Sewell and Hauser 1972, 1975; and Duncan et al. 1972), the results confirm the major conclusion of earlier studies that education is a key variable with respect to its total effect on occupation and with respect to its function as a mediator of antecedent effects on subsequent attainments.

Concerning adult social integration, the results support the earlier finding of Hodge and Treiman (1968) that education and income affect adult social integration but occupation does not. Education presumably instills in individuals a sense of community responsibility and political awareness which results in high levels of social involvement; and income apparently provides the resources for such activity, which is denied the economically less advantaged. These results also support the finding by Hauser (1970), Hodge (1970), and others that alternative socioeconomic status indicators may differentially affect dependent variables, in this case adult social integration. Finally, on the assumption that adolescent and adult social integration indicators were tapping the same behavioral dimension, an estimate has been provided for the stability of the construct free of spurious associations attributable to common antecedents. Although the coefficient is far from unity, it is strong compared with other antecedent effects. Though one cannot prove by the association that the two indicators were measuring the same underlying behavioral dimension, the magnitude of the coefficient makes that interpretation plausible.

I have argued that adolescent social integration provides socialization experiences for attitudes and skills that have later-life payoffs not only for education, but also for occupation and income. Adolescent social integration provides a communication and interaction context within which facility in interpersonal relations is learned and cultivated. These skills have later-life benefits inasmuch as jobs with high prestige and high income require ease in working with others. Early mastery of interpersonal skills provides a sense of confidence in adapting to new social situations which frees the individual to search more broadly for a choice position. It makes him geographically mobile, which in turn facilitates his social mobility. Moreover, being socially integrated provides a network of "contacts" who share information about employment possibilities and may provide entry by way of personal recommendations.

An alternative explanation for the finding is that the social integration indicator, particularly in adolescence, may be a surrogate for some other variable or process, perhaps educational and occupational aspirations. This interpretation raises the question of whether the product of socialization is an attitude or skill—though, of course, some combination of the two is not necessarily precluded. If the social integration effect were to remain evident when educational and occupational aspirations are controlled, one might rule out the possibility that adolescent social integration merely

serves as a proxy for aspirations and conclude that interpersonal skills, for example, are learned in adolescent interactive situations and have later-life attainment payoffs. I plan a reestimation of the model, respecified to incorporate the effects of educational and occupational aspirations, to provide a test of the aspirations hypothesis. A second alternative explanation is that the relationships with social integration are due to spurious associations with common but unspecified antecedents. One might reason, for example, that a personality characteristic causes both social integration and educational attainment, and that identifying the trait and incorporating it into the models would reduce the relationships with attainments. Future analysis will examine this possibility. Finally, Coleman (1961) and others have argued that high school athletics is an independent dimension of adolescent social integration and ought to be considered separately for analytical purposes. Unfortunately, the 1957 variable was coded into a summary score, and a more refined breakdown by kind of extracurricular participation is not readily attainable. A future effort will be made to recode the data in order to examine the antecedents and consequences of specific kinds of extracurricular activity, including nonacademic indicators of social integration.

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# Social Structure from Multiple Networks.

## II. Role Structures<sup>1</sup>

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Role structures in small populations are given operational meaning as algebras generated from the sociometric blockmodels of Part I by Boolean multiplication (matrix multiplication employing binary arithmetic). Many different sociometric structures can yield the same algebraic multiplication table, which captures a different level of social structure. Elements of the algebras are interpreted concretely as compound roles, and interlock among these roles is studied through investigation of their algebraic properties (equations and inclusions). Similarities and differences among algebras from six case studies are explored by means of homomorphisms as well as by multidimensional scaling on a derivative numerical distance measure. Results for particular populations, including reliability and stability tests, are summarized through simple target tables reporting aggregations of more complicated role structures.

Roles have long fascinated sociologists and lay people alike. Particular kinds and classes of role have received meticulous and often insightful treatments. Many of these treatments have been by professional sociologists and anthropologists (for sick people, Parsons [1949]; for Italian immigrants in an American city, Whyte [1943]; for members of the International Typographical Union, Lipset, Trow, and Coleman [1956]). Some of the best analyses have been by amateurs (for diplomats, Nicolson [1932]; for federal judges, Wyzanski [1952]; for soldiers, Hackett [1963]; for bureaucrats, Tullock [1965]; for scientists, Davis [1968]).

Within this vast body of work, almost all useful insights have remained particular. There is no model for roles comparable with the fundamental models in other areas of analytic sociology: the industrial mobility of

<sup>1</sup> Support from the National Science Foundation under grant GS-2689 is gratefully acknowledged. Phipps Arabie supervised the multidimensional scalings. François Lorrain contributed an elegant algorithm which serves as an independent check on the  $\gamma$ -algorithm. Nicholas and Carolyn Mullins supplied helpful critiques of an earlier draft, as did the anonymous referees and our coauthor for Part I, Ronald L. Breiger. Thanks are also owed to Luis Boza of Bell Telephone Laboratories for the opportunity to present this material at a seminar there in May 1974. Part I of this article, by White, Boorman, and Breiger, appeared in the January 1976 issue of this *Journal*.

labor (Blumen, Kogan, and McCarthy 1955); social hierarchies (Landau 1965); population dynamics (Lotka 1925); attitude change (Coleman 1965); social networks (Moreno and Jennings 1945). In dealing with roles, anthropologists more than sociologists have tried to break beyond the limits of their specific examples (Rivers 1924); however, their chief success in constructing general role theories has been in the specialized areas of kinship and kinship semantics (e.g., Lévi-Strauss 1949; Romney 1965; but see Schneider 1965). Almost alone, Nadel (1957) attempted to set up a formalism for describing roles and role interlock in greater generality. Nevertheless, his effort also remains unsatisfying, mainly because his formal descriptions have little bearing on concrete populations.<sup>2</sup>

Any attempt to develop a general theory of roles is audacious by definition. An excellent *a priori* case can be made that such generality is in fact not possible: for quite different reasons economists, social psychologists, and lawyers would feel sympathetic to such a viewpoint. By comparison, the vision of the classical sociologists, particularly Durkheim and Simmel, is notably more optimistic. These early theorists were committed to the possibility of a unified treatment of roles, crystallized from the multiple relationships among men in groups.

This paper is a direct theoretical attack on the problem of role structure. As in the tradition of French structural anthropology through Lévi-Strauss and Lorrain (and as also influenced by Jakobson and the linguists), the approach is algebraic.<sup>3</sup> Role structure is modeled as an algebra, the elements of which are compound roles formed from a fixed set of primitive roles; different role structures are compared by algebraic mappings (homomorphisms). The quest is for a dual description of social structure of the kind which classical balance theory suggests in principle but is unable to extract from data. On one level are networks, which Part I aggregates into blockmodel images. On a second and distinct level are roles. Connecting the two levels are statements (equations and inclusions) in the appropriate algebra. These statements formally describe role interlock. Transitivity is a familiar example, though a very restrictive one.

Throughout the paper, all developments proceed from a central belief that generalizations about role structure should be inductively obtained

<sup>2</sup> Nadel's formalism shows him to have been deeply influenced by symbolic calculi in the tradition of Russell and the young Keynes (1921). This bias was unfortunate, since it distracted him from the development of techniques with computational power. Poincaré (1913) is a biting criticism of formal calculi possessing chiefly definitional uses; see also Lewis (1960) for a fascinating historical review of classical systems of logic which bears directly on the problems of constructing novel formalisms.

<sup>3</sup> The French structuralist tradition is filled with attempts to translate structural ideas into some kind of algebraic or quasi-algebraic language. In addition to Lévi-Strauss (1949), see Courrège (1965), Foucault (1966), and Lacan (1957). Lorrain's (1975) developments are by far the most sophisticated.

by comparing results based on concretely presented social structures. The key is the word "concrete": adoption of a concrete viewpoint sets the present work apart from the long tradition of algebraic models of kinship which may be grounded on purely cultural constructs not tied to any particular population (Weil 1949; White 1963).<sup>4</sup> Below, we always derive role structure starting from networks of social relations in a concrete population.

This paper is more complicated technically than Part I. Several levels must be kept in view: (1) the raw networks on a concrete population; (2) blockmodels of those data, obtained by the methods of Part I; (3) role structures (semigroups) formed from the blockmodels; (4) comparisons of role structures, using homomorphisms and derived measures of structural distance; and (5) sets of algebraic equations (target tables) obtained from the semigroups, which may be given substantive interpretations with reference to the original data. At all times it is crucial to separate formal constructions from their associated interpretations. One of the problems with sociological language is that it is inherently rich, so rich as to evade the capacity of any formalism to capture it entirely (Boudon 1968). We will speak of roles, role structures, and positions both in a specific technical sense and in a broader empirical one. The context should make the intention clear.

In a strictly ancillary (though extremely important) way, we will also make use of certain additional technical methods. Most important is our use of the MDSCAL-5 algorithm based on the work of Shepard and Kruskal (as a means of representing similarities among role structures in a Euclidean space) as well as our use of inclusion partial orders (to suggest simplifications of role structures).

#### SUMMARY OF BLOCKMODELS (SYNOPSIS OF PART I)

Each of several networks of ties on a concrete population is reported as a square binary (0-1) matrix reporting the incidence of the given type of tie. The rows and columns of each matrix are then identically permuted in a self-consistent search with two objectives. First, the population of

<sup>4</sup> An additional difference of great technical importance is that the algebraic work of Weil and of White is strictly group theoretic. As such, it applies only to a very restricted class of social structures, chiefly represented by the Australian classificatory systems. The crucial step from groups to semigroups was taken in the late 1960s, in independent contributions by Boyd (1966, 1969a, 1969b) and White (1969). This transition relinquishes the great power of group theory as a branch of mathematics in exchange for the far wider substantive applicability of semigroup algebra. (For mathematical definitions see the section on methods below.) To some extent, a parallel course of evolution has been independently followed by mathematical linguistics, which has increasingly come to trade mathematical power for breadth of application (see Harris 1968; Chomsky and Halle 1968).

persons is to be partitioned into *positions*; second, for each type of tie there is to be a specified network of *bonds* connecting positions. Following rearrangement, the rectangular submatrix reporting ties from persons in one position to persons in a second position is termed a *block* for that type of tie. The specific aim of rearrangement is to reveal *zeroblocks*, blocks containing no ties; all other blocks correspond to bonds between positions. The *image* of a type of tie is a square binary matrix reporting the bonds. A *blockmodel* for a population is the set of images obtained from the separate types of tie. The term *block* will also be used for the set of members of a position.

Mathematically, a blockmodel is a homomorphic image of the original multigraph (Ore 1962).

Thus in Part I the concept of a position (in a social structure) is given operational content as a set of persons who are structurally equivalent in a blockmodel. Furthermore, to each position is associated a well-defined *role set*, which is that position's row and column across all blockmodel images; this definition is consistent with Merton's (1957, p. 369) usage. Both ideas take *simultaneity* into account: the fact that all positions and role sets are determined relative to one another. But the blockmodels of Part I fail to deal with interrelations among different perspectives. A role set may be seen as one position's view of the interlock of roles. But each position has its own viewpoint, and one may hope for global regularities which reflect a structure of roles not limited to any one position.

The task of Part II begins here. A number of case studies are treated, involving six small populations: (1) an industrial work group, (2) the management of an industrial firm, (3) a contemporary American monastery, (4) a research network of biological scientists, (5) and (6) two different experimental college fraternities. Five of the populations were discussed in Part I; one of the fraternity cases is new. Positive and negative affect, in various guises, are the types of tie most commonly represented in these case studies. A major part of our results hence concerns role structures based on sentiment.

Alternative blockmodels for a given case are suggested in Part I (see also Breiger, Boorman, and Arabie 1975), depending on the strictness with which the sociometric data are assessed and on the level of aggregation sought. A major concern of the present paper, and a principal cause of its length, is identifying a nucleus of role structure which is robust across all blockmodels suggested for a given case (see figs. 13 and 14).

Three distinctive properties of the blockmodels of Part I are crucial in the development of role structure models. First, a bond may obtain from one position to another on each of several types of ties—"ambiguity" in the overall quality of relations from one position to another is considered normal. Second, no special weight is given to reciprocated choices—a bond



may or may not be reciprocated by a bond of that or another type from the other position. Third, a bond from a position to itself is treated on the same footing as one to another position—"reflexivity" is a separate substantive issue for each position on each type of tie.

The organization of Part II is conventional: Theory, Methods, Results, and Conclusion. Each section is as self-contained as possible.

## THEORY

The main ideas build from one axiom, the Axiom of Quality, together with one definition, that of role structure (see below). Mathematical details are deferred to the section on methods; here, only minimal formalism is introduced to allow initial discussion of examples.

### The Basic Ideas

*Introduction.*—Several different contributions to role theory serve as points of reference. Our approach resembles that of Gross, Mason, and McEachern (1958) in its commitment to deriving roles from data on concrete populations. However, they studied only a single, focal position (that of school superintendent) possessing a single incumbent. Their problem was the nature and degree of role consensus among occupants of certain other positions with formally prescribed ties to the focal position (e.g., teacher, principal, school-board member, finance-committee member, selectman). They collected varied data, often in metric form, on a few selected roles vis-à-vis the focal position.<sup>5</sup> In contrast, our own emphasis is on *closure*: we will be concerned with setting up a concept of a closed system of roles and with exploring the properties of such closed systems.<sup>6</sup>

Kinship suggests a second contrast. The present approach has its roots in kinship (specifically, in the Australian classificatory systems), and several of our major themes will reflect that origin (see also White 1963, chap. 1). However, those classificatory systems are less descriptions than brilliant ideologies of social structure evolved by aboriginal civilizations. In them, as in all viable kinship systems, the whole point of the system is to provide a set of rules and nomenclature for the benefit of participat-

<sup>5</sup> Observe how ordinary language tends to confuse differences between positions on the one hand and relations between positions on the other. Many of the operative features of social structure are lost thereby: it is possible to think of a worker as a person who works, independently of his relationship to a foreman; it is not possible to think of a vassal independently of his relationship to a lord (see Dibble [1972, p. 156], as well as Sim [1966]).

<sup>6</sup> In this respect, the present developments follow the pattern set by economic theory, where closed systems provide the basic model, rather than of classical sociological role theory.

ing members. Anthropologists have sensed this utilitarian point when they have talked of coding and of the information content of a kinship system.<sup>7</sup> By contrast, the kind of role structures which interest us here are descriptions of actual overall structure not accessible to any unaided observer, whether participant or not. Upon reflection, this should not be surprising: there is no inherent reason why the global properties of a social structure should be transparent to its members, though each may know his own position well enough.<sup>8</sup>

Recent developments in sociological role theory are surveyed by Mirra Komarovsky in her "Presidential Address" (1973). She differentiates sharply between "formal, Simmelian" and "substantive" aspects of roles, the latter of which enters through an accounting of prevailing "rights and obligations." Such a distinction is in fact well founded in the literature. However, it is also largely artificial and is perhaps best viewed as another manifestation of the heritage from kinship (where formal terminological studies may represent an exercise in formalism which is almost totally content free).<sup>9</sup> As in Part I, we try explicitly to do away with the distinction. In contrast with Komarovsky's "rights and obligations," substantive content now enters as the incidence of bonds in a blockmodel, which in turn is based on the incidence of ties in a concrete population. The present work then builds role structures in a formal way but always with this substantive foundation.

Like Gross et al., Komarovsky rightly emphasizes the importance of recognizing as a variable the degree of consensus on role expectations. However, she is handicapped by the absence of basic phenomenology. Consensus is a concept with strong connotations of equilibrium, but sociology has never developed any counterpart to the general equilibrium conditions forming the basis of economic theory (which have been operationalized through input-output tables, among other methods). Below, we treat the weakness of phenomenology as fundamental, the problem of defining and measuring consensus as secondary. Only when the concept of role structure has been operationalized can issues of expectations be approached.

<sup>7</sup> See Wallace (1961) for attempts to estimate upper bounds to the complexity of folk taxonomies.

<sup>8</sup> Once again, this observation will seem a commonplace to economists, who take for granted that the interactions of economic forces transcend the information-processing capabilities of any single agent. This is the traditional explanation of why centralized economic planning is so difficult to make efficient (Koopmans 1957; see also Kornai 1971). For a sensitive analysis of the limits of observer perception in a complex role setting, see Ekvall (1960), which is a description of the author's experiences as U.S. army interpreter for the United Nations at the Korean cease-fire talks.

<sup>9</sup> See, for example, Kay (1965) on Dravidian systems; Buchler (1966) on Omaha systems; and Romney (1965) for the Kalmuk case, which is a lineal type of Omaha classification.

Komarovsky also poses three main criticisms of existing role analysis. The first concerns neglect of individuality. We do not deal with this directly, but it should be pointed out that blockmodels suggest a highly natural way of measuring individual differences. Only rarely will bonds in a blockmodel image correspond to completely filled matrices in the original data; pulling back from images to data accordingly suggests that individuality may be captured by the pattern of the variability.

She directs a second criticism against the static flavor of role analyses, the awkwardness of discussing change and manipulation. Here again we treat as fundamental the descriptive problem: that of comparing blockmodels over time, and perhaps across different numbers of blocks as well. This problem is approached using the algebraic concept of homomorphism and the JNTBOM algorithm (see section on methods below).

Komarovsky's third criticism of role analysis is phrased in terms of a denial that norms determine role behavior and thus explain conformity and the social order (but see Rommetveit 1953). Our agreement with her on this point should already be clear. Returning, however, to our starting emphasis on multiple levels of structure, it should be stressed that our present approach does not exclude norms and in some instances may actually serve as a search procedure for cultural regularities. Certain aspects of role interlock (algebraic equations) will be given a significance as abstract regularities emerging from particular blockmodels. Where such equations arise and are interpretable, they may fulfill the same function as the analogous equations in classificatory kinship, of which they are reminiscent: the equations generated from a blockmodel may represent nascent cultural roles, perhaps even positive norms. More complex models will obviously be needed, but the principle is an important one: abstract cultural regularities are patterns emergent from concrete networks among particular persons.<sup>10</sup>

*Role reciprocity.*—In the sociological literature the concept of role has long been associated with a second concept, role reciprocity; one might almost say that the idea of reciprocity has been the main contribution to role theory which is distinctly sociological and not psychological in content. Included in it is the notion that alter should accept ego's expectations of him (and conversely), as well as the further notion that the converse expectations alter holds of ego should mirror in some sense (though perhaps not duplicate) ego's expectations of alter. In our terms,

<sup>10</sup> This agrees with the view expressed by Zetterberg (1965) that the primitive terms of sociology should arise directly from human agents and their actions. The views of Homans on this subject are well known (see Homans 1962). It is interesting that certain areas of contract law have recently begun to move in a direction suggestive of blockmodels, particularly the zeroblock concept (see the preface to Reitz 1975).

"ego" is to be thought of as a representative incumbent of one position and "alter" as that of a second (perhaps the same) position.

What has already been said suggests that reciprocity is far too restrictive and also too slippery a notion on which to build a theory of roles. Reciprocity focuses on two positions in isolation. As such, it fails to account for indirect causal effects arising from the wishes and expectations of third positions, perhaps many of them.<sup>11</sup> Also, the usual concept of role reciprocity remains heavily tied to subjective perceptions by the parties which are at once very difficult to establish and often not relevant to the objective structure.<sup>12</sup>

Suitably formulated, however, the concept of reciprocity does suggest the two main ideas of the present theory. The first is that one role generates another: A's expectations toward B generate reciprocal (though perhaps very different) expectations toward A. The second is the idea of *chaining*: taking the generation process one step further, B's expectations toward A may interact with A's original expectations toward B to produce new expectations. The difficulty with reciprocity lies in the fact that it does not push the implications of either of these ideas to their natural conclusion on a social structural level, as we now proceed to do.<sup>13</sup>

*Compound images.*—In Part I, a separate image was found for each type of tie. This image reported as a binary matrix the incidence of bonds of that type among positions. Now generate compound images: each

<sup>11</sup> Lévi-Strauss makes much the same point in distinguishing "restricted exchange" from "generalized exchange" (see discussion in Ekeh 1974).

<sup>12</sup> Is my liking for James a social fact or only a psychological quirk if any of the following hold true: (i) he does not reciprocate my liking; (ii) he does not reciprocate with any other type of tie; (iii) he is not aware of my sentiment toward him; (iv) he does not know my name; (v) he does not know whom I am tied in to; (vi) he does not recognize me; (v) no one else in the population is aware of my liking for James? Can such questions be answered by either observation or questionnaire? (see Brown 1965). For roles defined culturally, on the other hand, in abstraction from particular populations, the meaning of reciprocity may be clear. It is inconceivable that a person can be kin to me without my being kin to him. So my kinship role to him necessarily implies some reciprocal role from him to me. In addition, each of the two roles is recognized, and is known by all to be recognized, by me and by him and by all other members of our society. This is role reciprocity in the full sense. Other sorts of cultural roles imply reciprocity. It is hard to think of lawyers without clients. However, once a whole set of roles and positions is included, the issue of reciprocity becomes murkier. How many of us know the court's clerk is the bailiff's superior in the legal role frame?

<sup>13</sup> It is instructive to point up a contrast with the model of Friedell (1969). That model has a very similar starting point in a concern with unfolding the implications of reciprocity. However, the idea of longer chains is developed at a purely psychological level in a two-party interaction, with the chains being of the form "he thinks that I think that he thinks . . .," etc. This kind of analysis is Schelling's forte.

such image is again a binary matrix reporting which positions are joined by a *chain* of bonds of specified types. Formally:

*A compound image  $1*2$  is represented by a square binary matrix, whose  $(i,j)$ th entry is 1 if and only if there is a bond of type 1 from  $i$  to some third position  $k$  (which may coincide with  $i$  or  $j$ ) and a bond of type 2 from  $k$  to  $j$ .*

In an obvious way, this definition may be extended to compounds of type  $1*2*3 \dots$  through more than one intermediate position. Much has been written about the sociological meaning of compound relations in kinship (Lévi-Strauss 1949) and in more general kinds of formal and informal social structure (White 1969; Lorrain and White 1971; Fararo 1973). The innovation is that compounds are now being formed starting with blockmodel images, not with raw sociometric or observer-reported network data (e.g., Luce and Perry 1949).

Various features of the definition deserve emphasis:

1. Any position  $k$  may be a "middleman" with respect to any other position on any type of compound image.

2. The binary coding ignores the absolute number of possible intermediaries  $k$  between a given  $i$  and a given  $j$ . This is consistent with the way in which the underlying blockmodel has already imposed a binary coding of block densities (discriminating only between bonds and zero-blocks); it is also consistent with any prior disregard of the "strength" of ties in coding the original data.

3. The constituent images of compound images need not be of different types, and any number of repetitions is allowed; for example,  $3*3*1$ ,  $2*1*2$ ,  $1*1*1*1$ , etc.

4. In particular, there will be a reflexive entry from position  $i$  to itself in the compound image of type  $1*1$  if  $i$  has a reciprocated bond of type 1 with some other position  $j$ . Thus reflexive entries in certain compound images may indicate a weak form of reciprocity (see also Lorrain 1975).

5. A position may appear repeatedly as an intermediary in a chain contributing to a given entry in a compound image. Indeed, a position may appear at successive steps of the chains. This is legitimate, since a bond from a position to itself in a blockmodel is comparable with a bond to a different position (contrast the sociometric level of the original data, where the diagonal of a sociomatrix is inherently meaningless).

6. The order of images in the compound is material:  $1*2$  need not equal  $2*1$  and usually will not (contrast the well-known principle of balance theory that my friend's enemies are my enemy's friends,  $PN = NP$ ).

7. On the other hand, in computing the compound of several images, say  $3*1*2$ , it does not matter whether one first computes the incidence of

bonds for  $3*1$  and then for  $(3*1)*2$ , or instead first finds  $1*2$  and from that finds  $3*(1*2)$ .

In mathematical terms, the binary matrix representing a compound image of type  $1*2*3 \dots$  is the Boolean matrix product of the specified image matrices (see section on methods).

Just as we earlier examined different types of tie, each type of compound image is now to be examined separately in terms of the matrix which represents it. It is natural to adopt the following:

*Axiom of Quality.*<sup>14</sup>—Two types of compound image are to be identified if and only if their associated matrices coincide.

Given this axiom, there can be only a finite number of distinct types of compound image for a given blockmodel; usually, there are but a handful for the blockmodels in our case studies. Of particular importance is the special case in which one or more compounds are identical with the initial images; we now turn to such a case as our first illustration.

*An example.*—Go back to Sampson's (1969) study of an American monastery, and adopt Part I's three-block split of his 18-man population into Loyal Opposition, Young Turks, and Outcasts. From among Sampson's four types of sociometric choice, select the Sanction choice and impose the three-block split on two separate matrices reporting top three choices on Positive and Negative Sanction, respectively. Weighting entries by +3 (top choice), +2 (second choice), and +1 (third choice), obtain the following density matrices (see also table 1 below):

$$\begin{aligned} \text{(Positive Sanction)} & \begin{bmatrix} .548 & .020 & 0 \\ 0 & .905 & .036 \\ .179 & .179 & .333 \end{bmatrix} \\ \text{(Negative Sanction)} & \begin{bmatrix} 0 & .204 & .536 \\ .286 & .048 & 1 \\ .107 & .143 & .417 \end{bmatrix} . \end{aligned}$$

(Both row and column orderings follow the sequence: Opposition, Turks, Outcasts.) Observing that there are no entries in the range (.05, .1), it is natural to convert to binary form using a .1 cutoff for coding a block as a zeroblock; thus (following Part I's notation) obtain<sup>15</sup>

$$K = \begin{bmatrix} 1 & 0 & 0 \\ 0 & 1 & 0 \\ 1 & 1 & 1 \end{bmatrix} \quad B = \begin{bmatrix} 0 & 1 & 1 \\ 1 & 0 & 1 \\ 1 & 1 & 1 \end{bmatrix} .$$

<sup>14</sup> In axiomatic set theory, this same principle goes under the name of the "Axiom of Extensionality."

<sup>15</sup> Detailed notation is developed below in table 1. Later we show that role structure is robust with respect to refinement of blocks and to the binary coding of density matrices.

Thus each block directs a positive bond to itself, and the Outcasts also direct positive bonds toward each of the other two factions. At the same time, each pair of different blocks reciprocates a negative bond, and the Outcasts direct a negative bond to themselves.

Now form compound images:  $K$  is transitive,<sup>10</sup>

$$K^2 = K,$$

but this statement is comparatively vacuous since there are only three distinct blocks and  $K$  is not a linear hierarchy. Also uninteresting are the further equations

$$BK = B^2 = \begin{bmatrix} 1 & 1 & 1 \\ 1 & 1 & 1 \\ 1 & 1 & 1 \end{bmatrix},$$

the universal pattern in which all pairs of blocks exchange bonds.

Finally, however, one also has

$$KB = B,$$

and this last equation is neither a tautology nor trivial. It represents the intersection of two separate statements about images:

$$KB \subseteq B; \quad (1)$$

$$B \subseteq KB. \quad (2)$$

Statement (1) may be taken to imply a kind of loyalty: I will support my "friends" by taking on their "enemies." Statement (2), on the other hand, describes a pattern of social reinforcement: whoever my enemy is, I will have a friend or friends who share my enmity.

Of course, these cultural interpretations with their implicit time-ordered dynamics centered on some individual are not the only ones possible, and it is also possible to interpret  $KB = B$  without cultural or psychological premises, simply as a convenient summary of certain properties of  $K$  and  $B$  at the blockmodel level. Statement 2 is then seen as reporting a pattern of social reinforcement among positions: every position which has enmities also is friendly to positions which share those enmities.

In any case, the fundamental importance of the equation lies in its *invariance* (Stevens 1975): the social structure is described in a way which is independent of the limited viewpoint of any particular ego

<sup>10</sup> Because reflexivity is an important substantive variable (see earlier), we adopt a strict mathematical definition of transitivity for blockmodel images. An irreflexive graph  $K$  is transitive by the standard definition (Ore 1962, p. 191) if  $K^2 \subseteq K$ . Many earlier investigators (notably Davis 1970; Holland and Leinhardt 1976) have demonstrated that graphs representing positive affect tend to be transitive (see also the explicitly hierarchical models derived by Bernard [1974] for Coleman's high schools [1961]).

position. Such invariance means that equations will be a natural approach to comparing blockmodels with different numbers of blocks and on different populations (see below). Yet an equation need have no direct incidence upon a particular position (save possibly as a middleman): the role structure will not usually be homogeneous.

Whether  $KB = B$  is in some sense fundamental, as suggesting a general interlock pattern basic to the Sampson social structure, will be apparent only later, once the formalism has been fully developed. In the section on results,  $KB = B$  (suitably reinterpreted employing homomorphisms) will be shown to be a robust characteristic of the Sampson structure. This characteristic will be used to distinguish the monastery from Newcomb year 2, among other data cases.

### Role Structure in a Population

*Multiplication tables for compounds.*—We seek evidence of role structure in a population with two or more reported types of tie. Assume a particular blockmodel, which will remain fixed throughout subsequent analysis. Call the blockmodel images *generators* when they are used to form compound images; refer to both generators and compound images as *words* (drawing terminology from the theory of generative grammars). Thus, in the  $KB$  example,  $KB$  is a word; so also are  $K$ ,  $BK$ ,  $BK^2$ ,  $BK BK$ ,  $B^2$ . In classical sociometry, the concept of word formation is implicit in the notion of forming the transitive closure of a single relation  $R$ :

$$\text{tr}(R) = R \cup R^2 \cup R^3 \cup \dots \cup R^n \cup \dots$$

The classical approach, however, never formed compounds on more than a single generator in this way, and it never distinguished words as separate entities (but see Moon and Pullman [1967], following up the phenomenology of Landau [1951]).

A *role structure* is the set of all identifications among words obtained by applying the Axiom of Quality to the compound images formed by multiplying generators.

Mathematically, a role structure is therefore simply the Boolean matrix semigroup formed by taking Boolean matrix products of the specified generators (see section on methods).

In the  $KB$  example already introduced, the role structure has only three distinct words:  $K$ ,  $B$ , and the universal element  $U$ . We have already studied role structure for this case, and our findings may be systematically summarized by means of what algebraists call the *multiplication table* of the algebra. This is the array showing all possible products of the words  $K$ ,  $B$ , and  $U$ . Figure 1 displays this table in two alternative forms. The first is concrete and represents the structure as a set of matrices together



## (a) Matrix products

×	$\begin{bmatrix} 1 & 0 & 0 \\ 0 & 1 & 0 \\ 1 & 1 & 1 \end{bmatrix}$	$\begin{bmatrix} 0 & 1 & 1 \\ 1 & 0 & 1 \\ 1 & 1 & 1 \end{bmatrix}$	$\begin{bmatrix} 1 & 1 & 1 \\ 1 & 1 & 1 \\ 1 & 1 & 1 \end{bmatrix}$
$\begin{bmatrix} 1 & 0 & 0 \\ 0 & 1 & 0 \\ 1 & 1 & 1 \end{bmatrix}$	$\begin{bmatrix} 1 & 0 & 0 \\ 0 & 1 & 0 \\ 1 & 1 & 1 \end{bmatrix}$	$\begin{bmatrix} 0 & 1 & 1 \\ 1 & 0 & 1 \\ 1 & 1 & 1 \end{bmatrix}$	$\begin{bmatrix} 1 & 1 & 1 \\ 1 & 1 & 1 \\ 1 & 1 & 1 \end{bmatrix}$
$\begin{bmatrix} 0 & 1 & 1 \\ 1 & 0 & 1 \\ 1 & 1 & 1 \end{bmatrix}$	$\begin{bmatrix} 1 & 1 & 1 \\ 1 & 1 & 1 \\ 1 & 1 & 1 \end{bmatrix}$	$\begin{bmatrix} 1 & 1 & 1 \\ 1 & 1 & 1 \\ 1 & 1 & 1 \end{bmatrix}$	$\begin{bmatrix} 1 & 1 & 1 \\ 1 & 1 & 1 \\ 1 & 1 & 1 \end{bmatrix}$
$\begin{bmatrix} 1 & 1 & 1 \\ 1 & 1 & 1 \\ 1 & 1 & 1 \end{bmatrix}$	$\begin{bmatrix} 1 & 1 & 1 \\ 1 & 1 & 1 \\ 1 & 1 & 1 \end{bmatrix}$	$\begin{bmatrix} 1 & 1 & 1 \\ 1 & 1 & 1 \\ 1 & 1 & 1 \end{bmatrix}$	$\begin{bmatrix} 1 & 1 & 1 \\ 1 & 1 & 1 \\ 1 & 1 & 1 \end{bmatrix}$

## (b) Letter products

×	(K)	(B)	(U)
(K)	(K)	(B)	(U)
(B)	(U)	(U)	(U)
(U)	(U)	(U)	(U)

FIG. 1.—KB3 role structure. In the notation introduced in table 1, the generators are  $SM(4)-3-K(3)B(3) > .1$ .

with their matrix products (by closure, these must be elements of the original set). The second is symbolic and employs letters (or later, numerals) to represent the three distinct elements of the structure. Although obtained directly from the concrete matrix table, the symbolic table retains only the abstract pattern of role interlock implied by the former one. *We employ the symbolic table as our central tool for operationalizing a new level of social structure.* This level may be inferred from block-models, but we will see below that recovery in the reverse direction is not necessarily or even normally possible: very different blockmodels may generate role structures giving rise to identical symbolic tables (figs. 5 and 6 below).

Before developing this point, first consider a second and much richer example. Taking all eight Sampson relations, consider our usual three-block split and code entries as bonds using only the top two choices and a strict zeroblock criterion. This leads to seven distinct generators, since Disesteem and Antagonism produce identical images and are therefore identified (fig. 2). Forming all possible compounds now produces only *four* new compound roles whose matrices are also shown (together with the informationally vacuous *U* pattern, which is generated as no. 10).

When there is tight-knit role structure, there should be a high incidence of compound words having multiple representations as products of generators; various alternatives are shown in figure 2. Because of this characteristic nonuniqueness, figure 3 shows the symbolic form of the multiplication table using an arbitrary integer coding for the distinct words. We have identified the generator rows in figure 3, in anticipation of the distinctive significance of generators in later comparison of tables (see section on methods).

Examine the figure 3 table more carefully. It is a complete statement of role structure in the given data; however, much of the information is redundant and only a few equations in the table convey interesting structural descriptions. We already saw this in the figure 1 table, where only one equation in nine is of interest. The following general points apply:

1. Many equations are simply definitions of a new word (although this will depend on the order in which compounds are generated). Thus, starting from words 1 and 2, the equations  $2*1 = 8$ ,  $2*2 = 10$  simply define two new words which are not in the generator set. Once these words have been generated, however, the further equations  $2*8 = 10$  and  $1*10 = 2*10 = 10*1 = 10*2$  are no longer definitional (although, returning to the graph level, equations involving 10, the filled matrix, are interpretively uninteresting).

2. Many more equations will simply be logical consequences of ones already derived, and therefore redundant. Thus if one has  $L^2 = L^3$  (a weak form of transitivity found in certain of the Newcomb year 1 cases) he must also have  $L^2A = L^3A$ ,  $L^2AL = L^3AL$ , etc., and these apparently new equations contain no new information. It is a mathematical fact that the complete table of any role structure with  $g$  generators and  $e$  words can be filled out from information contained in the  $g \times e$  subtable corresponding to the generator rows. This is inductively clear, since any word can be built up from generators through multiplying generators by words already formed.

3. Finally, equations involving zeros assume a special significance. Formally, a (structural) "zero" in a multiplication table is an element 0 whose product by any element is itself:  $0*x = x*0 = 0$ . A structural zero in this sense need not correspond to a *Z* (empty) image pattern at the

<u>Words</u>	<u>Matrix</u>
1. $E=E^2=EK=KE$	$\begin{bmatrix} 1 & 0 & 0 \\ 1 & 1 & 0 \\ 1 & 1 & 1 \end{bmatrix}$
2. $D=A$	$\begin{bmatrix} 0 & 1 & 1 \\ 1 & 1 & 1 \\ 1 & 1 & 0 \end{bmatrix}$
3. $I=I^2=IK=IL=KI$	$\begin{bmatrix} 1 & 1 & 0 \\ 0 & 1 & 0 \\ 1 & 1 & 1 \end{bmatrix}$
4. $N$	$\begin{bmatrix} 0 & 1 & 1 \\ 1 & 0 & 1 \\ 1 & 1 & 0 \end{bmatrix}$
5. $K=K^2$	$\begin{bmatrix} 1 & 0 & 0 \\ 0 & 1 & 0 \\ 1 & 1 & 1 \end{bmatrix}$
6. $B=ED=EN=EB=KD=KB$	$\begin{bmatrix} 0 & 1 & 1 \\ 1 & 1 & 1 \\ 1 & 1 & 1 \end{bmatrix}$
7. $L=KL$	$\begin{bmatrix} 1 & 1 & 1 \\ 0 & 1 & 1 \\ 1 & 1 & 1 \end{bmatrix}$
8. $DE=DI=DK=NE=NI=NK$	$\begin{bmatrix} 1 & 1 & 1 \\ 1 & 1 & 1 \\ 1 & 1 & 0 \end{bmatrix}$
9. $IE=EI$	$\begin{bmatrix} 1 & 1 & 0 \\ 1 & 1 & 0 \\ 1 & 1 & 1 \end{bmatrix}$
10. $L\{E, D, I, N, K, B, L\}=$ $B\{E, D, I, N, K, B, L\}=$ $B^2=D^2=N^2=DN=DB=ID=IB=EL$	$\begin{bmatrix} 1 & 1 & 1 \\ 1 & 1 & 1 \\ 1 & 1 & 1 \end{bmatrix}$
11. $IN$	$\begin{bmatrix} 1 & 1 & 1 \\ 1 & 0 & 1 \\ 1 & 1 & 1 \end{bmatrix}$
12. $KN$	$\begin{bmatrix} 0 & 1 & 1 \\ 1 & 0 & 1 \\ 1 & 1 & 1 \end{bmatrix}$

FIG. 2.—Sampson role structure on eight generators. In table 1 notation, this is  $SM(4)-3-E(2)D(2)I(2)N(2)K(2)B(2)L(2)$ . Each compound word is labeled by its representations as a product of two generators; there will be additional representations as products of three or more generators.

## Social Structure from Multiple Networks. II

		Generators											
		1	2	3	4	5	6	7	8	9	10	11	12
Generators	1	1	6	9	6	1	6	10	10	9	10	10	6
	2	8	10	8	10	8	10	10	10	8	10	10	10
	3	9	10	3	11	3	10	7	10	9	10	11	11
	4	8	10	8	10	8	10	10	10	8	10	10	10
	5	1	6	3	12	5	6	7	10	9	10	11	12
	6	10	10	10	10	10	10	10	10	10	10	10	10
	7	10	10	10	10	10	10	10	10	10	10	10	10
	8	8	10	8	10	8	10	10	10	8	10	10	10
	9	9	10	9	10	9	10	10	10	9	10	10	10
	10	10	10	10	10	10	10	10	10	10	10	10	10
	11	10	10	10	10	10	10	10	10	10	10	10	10
	12	10	10	10	10	10	10	10	10	10	10	10	10

FIG. 3.—Multiplication table for Sampson generators shown in fig. 2. Numbering follows fig. 2.

blockmodel level, although any such empty pattern will act as a zero in a table. Thus 10 is a zero in figure 3 even though the graph corresponding to 10 is in fact the full graph (*U* pattern). From the standpoint of the structural information conveyed, zeros in a table are effectively "garbage" elements, since they are capable of generating no novel patterns of role interlock. If a table contains a zero, the frequency of the zero element is usually high, as is therefore the frequency of uninteresting equations. For example, in figure 1 the *U* pattern is a zero and accounts for 78% of the table; in figure 3 the frequency of the zero is about 70%.

These are three reasons to expect rather few interpretable equations in any particular table. Much of the apparent structure formally implied in a multiplication table is thus actually bogus; only a part of the table is operative in defining role interlock. At the same time, it is not always easy to exclude particular equations in preference to others, and we have seen that the outcome may be affected by purely arbitrary factors such as the order in which words are generated. These are strong grounds for rejecting a case-by-case approach to equations such as was followed above in the *KB* example and instead developing ways of treating entire tables

as integrated structures. This shift in emphasis is developed below through the concept of a homomorphism.

For the moment, it should be stressed that there is no necessity that all tables be equally informative or even that a particular table reveal any significant role interlock at all. An extreme example is the degenerate table shown in figure 4*a*, where every element multiplies to give a single

(a) Week 4, Newcomb Year 2

	<u>1</u>	<u>2</u>	<u>3</u>
<u>1</u>	3	3	3
<u>2</u>	3	3	3
<u>3</u>	3	3	3

(b) Week 8, Newcomb Year 2

	<u>1</u>	<u>2</u>	<u>3</u>	<u>4</u>	<u>5</u>
<u>1</u>	3	3	3	3	3
<u>2</u>	4	5	3	3	3
<u>3</u>	3	3	3	3	3
<u>4</u>	3	3	3	3	3
<u>5</u>	3	3	3	3	3

FIG. 4.—Role tables with complete or partial degeneracy. In table 1 notation, *a* is  $NF2(4)-3-L(3)A(3) > .1$ ; generators are

$$L = \begin{bmatrix} 1 & 1 & 1 \\ 1 & 1 & 0 \\ 1 & 1 & 1 \end{bmatrix}, \quad A = \begin{bmatrix} 0 & 1 & 1 \\ 1 & 0 & 1 \\ 1 & 1 & 1 \end{bmatrix}.$$

Similarly, *b* is  $NF2(8)-3-L(3)A(3) > .1$ ; generators are

$$L = \begin{bmatrix} 1 & 0 & 1 \\ 1 & 1 & 0 \\ 1 & 1 & 0 \end{bmatrix}, \quad A = \begin{bmatrix} 0 & 1 & 1 \\ 1 & 0 & 1 \\ 1 & 0 & 1 \end{bmatrix}.$$

garbage element, which is also a structural zero. In figure 4*b*, again the zero fills out most of the table. This makes excellent sense: these tables are for blockmodels obtained from weeks 4 and 8 in the Newcomb year 2 sequence, imposing a three-block split from week 15 (the last week). As was already argued in Part I, the social structure remains amorphous at the blockmodel level until about week 4 or 5. One would therefore be justified in expecting trivial role structure in figure 4*a* and perhaps rather little in figure 4*b*.

## Social Structure from Multiple Networks. II

*From blockmodel to role structure.*—Figure 5 brings out another aspect of the kind of structural information contained in role tables. Figure 5a, of course, is the table familiar from classical balance theory (Abelson and Rosenberg 1958). In our scheme, every word is identified with one or the other of the generators by the Axiom of Quality. The table is unusual in

(a) Multiplication table

$\times$	(L)	(A)
(L)	(L)	(A)
(A)	(A)	(L)

(b) Classical blockmodel

$$L = \begin{array}{cc} 1 & 0 \\ 0 & 1 \end{array} \quad A = \begin{array}{cc} 0 & 1 \\ 1 & 0 \end{array}$$

(c) Another blockmodel

$$L = \begin{array}{cccccc} 0 & 1 & 0 & 0 & 1 & 0 \\ 0 & 1 & 0 & 0 & 0 & 0 \\ 0 & 0 & 1 & 0 & 0 & 0 \\ 0 & 0 & 1 & 0 & 1 & 0 \\ 0 & 0 & 0 & 0 & 1 & 0 \\ 0 & 0 & 0 & 0 & 0 & 1 \end{array} \quad A = \begin{array}{cccccc} 0 & 0 & 1 & 0 & 0 & 1 \\ 0 & 0 & 1 & 0 & 0 & 0 \\ 0 & 1 & 0 & 0 & 0 & 0 \\ 0 & 1 & 0 & 0 & 0 & 1 \\ 0 & 0 & 0 & 0 & 0 & 1 \\ 0 & 0 & 0 & 0 & 1 & 0 \end{array}$$

FIG. 5.—Two blockmodels, each yielding the multiplication table of classical balance theory.

that all equations are interpretable and none is excluded on any of the three grounds given above. Each individual equation has been discussed extensively in the literature (e.g., for  $N^2 = P$  see Aronson and Cope 1968); collectively, the equations are always associated in the literature with a two-clique structure of the kind shown in figure 5b (and are interpreted at the level of individuals, not blocks).

Surely this algebra contains no surprises, although its applicability to real data may be questioned. But now look at figure 5c. Here are two graphs which may be interpreted as Like and Antagonism images in a six-block model. Taking these graphs as generators, proceed to fill out a compound role structure as in earlier examples. Astonishingly, for the two graphs bear little resemblance to a balanced graph, *the role structure they generate satisfies exactly the familiar equations of figure 5a!* This proposition may be made tighter: using the notation of figure 2 in Part I, and applying the BLOCKER algorithm, there is no way of collapsing the given six blocks into a  $(P, N)$  model at the two-block level (although  $[V, F]$  blocking is possible). In a very strong sense, the figure 5c graphs are inherently different from the 5b ones.<sup>17</sup>

A parallel finding arises in many cases generated by actual data: distinct—often highly distinct—blockmodels may generate identical role tables. Figure 6 gives two examples. For instance, in figure 6b two different Sampson pairs of generators for three blocks give the same role table. This table is also given by a pair of generators on four blocks for Newcomb's first fraternity (shown in fig. 6b, 3). *Identical equations may be satisfied by social structures that appear distinct at the blockmodel level.* Such coalescence may appear reminiscent of the way in which different networks may produce identical blockmodels. However, the latter possibility is simply an obvious by-product of aggregation. The present instance is different: a role table is a kind of "unfolding" of the generators, and it is correspondingly a strong assertion to say that two "unfoldings" coincide while the generators do not.

Passing to the role table therefore involves a fundamental loss of information, in exchange for obtaining a level of description which is invariant across positions in the sense discussed above. Assessing the net value of this transaction must await the section on results.

### Similarity of Role Structures

*Orientation.*—Is a given role table reliable, that is, will alternate blockmodels from different codings of sociometric indicators yield essentially the same table? Is there a way to distill just the essential features of role interlock from a large table, however reliable it is? Can one determine what two role structures from different cases have in common? To develop the theory, in this section we propose answers to these three interrelated questions of robustness, aggregation, and similarity for role structures. The answers build upon one concept, homomorphism, and will lead us to

<sup>17</sup> This example was contributed by Mr. Kazuo Seiyama (1975). As in most actual cases studied, not all bonds are reciprocated.

## Social Structure from Multiple Networks. II

(a) Newcomb Year 1, Weeks 8 and 13 ( $NF1(i)-4-L(3)A(3) > 0.1, i=8,13$ )

### 1. Week 8

$L = \begin{matrix} 1 & 1 & 1 & 0 \\ 1 & 1 & 1 & 0 \\ 0 & 1 & 1 & 0 \\ 0 & 0 & 1 & 1 \end{matrix}$	$A = \begin{matrix} 1 & 1 & 1 & 1 \\ 1 & 0 & 1 & 1 \\ 1 & 0 & 0 & 1 \\ 1 & 1 & 0 & 0 \end{matrix}$	$\begin{matrix} \underline{1} & \underline{2} & \underline{3} & \underline{4} & \underline{5} & \underline{6} & \underline{7} \\ \underline{1} & 3 & 5 & 7 & 6 & 6 & 7 \\ \underline{2} & 4 & 6 & 4 & 6 & 6 & 4 \\ \underline{3} & 7 & 6 & 7 & 6 & 6 & 7 \\ \underline{4} & 4 & 6 & 4 & 6 & 6 & 4 \\ \underline{5} & 6 & 6 & 6 & 6 & 6 & 6 \\ \underline{6} & 6 & 6 & 6 & 6 & 6 & 6 \\ \underline{7} & 7 & 6 & 7 & 6 & 6 & 7 \end{matrix}$
--	--	--

### 2. Week 13

$L = \begin{matrix} 1 & 1 & 0 & 0 \\ 1 & 1 & 0 & 0 \\ 0 & 1 & 1 & 0 \\ 0 & 0 & 1 & 1 \end{matrix}$	$A = \begin{matrix} 1 & 1 & 1 & 1 \\ 1 & 0 & 1 & 1 \\ 1 & 0 & 0 & 1 \\ 1 & 1 & 0 & 0 \end{matrix}$	$\begin{matrix} \underline{1} & \underline{2} & \underline{3} & \underline{4} & \underline{5} \\ \underline{1} & 1 & 4 & 5 & 4 & 5 \\ \underline{2} & 3 & 5 & 5 & 5 & 5 \\ \underline{3} & 3 & 5 & 5 & 5 & 5 \\ \underline{4} & 5 & 5 & 5 & 5 & 5 \\ \underline{5} & 5 & 5 & 5 & 5 & 5 \end{matrix}$
--	--	--

(b) Monastery and fraternity models

1.  $SM(4)-3-I(2)N(2)$  (identical to  $SM(4)-3-I(3)N(3) > 0.1$ )

$I = \begin{matrix} 1 & 1 & 0 \\ 0 & 1 & 0 \\ 1 & 1 & 1 \end{matrix}$	$N = \begin{matrix} 0 & 1 & 1 \\ 1 & 0 & 1 \\ 1 & 1 & 0 \end{matrix}$	$\begin{matrix} \underline{1} & \underline{2} & \underline{3} & \underline{4} & \underline{5} \\ \underline{1} & 1 & 4 & 5 & 4 & 5 \\ \underline{2} & 3 & 5 & 5 & 5 & 5 \\ \underline{3} & 3 & 5 & 5 & 5 & 5 \\ \underline{4} & 5 & 5 & 5 & 5 & 5 \\ \underline{5} & 5 & 5 & 5 & 5 & 5 \end{matrix}$
---	---	--

2.  $SM(4)-3-E(2)D(2)$

$E = \begin{matrix} 1 & 0 & 0 \\ 1 & 1 & 0 \\ 1 & 1 & 1 \end{matrix}$	$D = \begin{matrix} 0 & 1 & 1 \\ 1 & 1 & 1 \\ 1 & 1 & 0 \end{matrix}$	$\begin{matrix} \underline{1} & \underline{2} & \underline{3} & \underline{4} & \underline{5} \\ \underline{1} & 1 & 4 & 5 & 4 & 5 \\ \underline{2} & 3 & 5 & 5 & 5 & 5 \\ \underline{3} & 3 & 5 & 5 & 5 & 5 \\ \underline{4} & 5 & 5 & 5 & 5 & 5 \\ \underline{5} & 5 & 5 & 5 & 5 & 5 \end{matrix}$
---	---	--

3.  $NF1(12)-4-L(3)A(3) > 0.2$

$L = \begin{matrix} 1 & 1 & 0 & 0 \\ 1 & 1 & 0 & 0 \\ 0 & 0 & 1 & 1 \\ 0 & 0 & 1 & 1 \end{matrix}$	$A = \begin{matrix} 1 & 1 & 1 & 1 \\ 1 & 0 & 1 & 1 \\ 1 & 0 & 0 & 1 \\ 1 & 1 & 0 & 0 \end{matrix}$
--	--

FIG. 6.—Identical role tables from distinct blockmodels

identify a handful of small target tables with simple substantive interpretations.

*The concept of homomorphic reduction.*—Start with a given role structure such as that for which the table is shown in figure 7a. We seek a concept of aggregation which is consistent with the compounding of roles through which the table was originally generated. The following definition translates this consistency requirement into formal terms:

A *homomorphic reduction* of a role table is a partition of its words into equivalence classes  $C_1, C_2, \dots, C_k$  in such a way that class membership



is consistent with the operation of forming the compound. That is, if  $e_1, e_2 \in C_i$  and  $f_1, f_2 \in C_j$ , then  $e_1 f_1$  and  $e_2 f_2$  fall in the same class  $C_m$ .

Thus, a homomorphic reduction aggregates a role table by imposing new equations, those between the words in a class of the partition, in a way consistent with the equations in the full table.

An example is shown in figure 7. Here the original role structure is based on a five-block model for Sampson *I* (Influence) and *N* (Negative Influence). Following what will later be our standard convention, 1 is taken to represent the positive generator here (*I*) and 2 is taken to represent the negative generator here (*N*). There are nine further compounds, so that the entire structure contains 11 roles. There is a structural zero (element 8) which fills out much of the table, but the upper left corner contains nondefinitional structure not involving 8 (thus  $1*1 = 1*3 = 3*1 = 3*3 = 3$ ).

Figure 7*b* now shows a homomorphic reduction of the structure. There are three classes  $C_i$ ,

$$\begin{aligned} C_1: & (1, 3) \\ C_2: & (2, 5, 9) \\ C_8: & (4, 6, 7, 8, 10, 11), \end{aligned}$$

and it is directly apparent that the aggregation preserves consistency of compounds. Relying on this consistency, we may in turn replace the 7*b* table with the three-element table in figure 7*c*. Note that this last table is very simple in structure and that we have already seen it in a different context, through the *KB* example introduced in figure 1.

We will refer to the 7*c* table as a *homomorphic reduction* of the original table in 7*a*.

Homomorphic reduction will serve as our operational concept of role structure aggregation. Two comments should be made:

1. Aggregation is not unique: role structures will always possess numerous alternative reductions (compare the general observations on aggregation in Leijonhufvud [1968]). Each reduction may be viewed as one possible simplified description of the original structure, and not all such simplifications need be equally useful. For example, it is obviously possible to reduce the 7*a* table down onto the degenerate table

$$\begin{array}{ccc} & 1 & 2 \\ 1 & 1 & 2 \\ 2 & 2 & 2, \end{array}$$

but this further reduction is clearly uninformative.

Later, in the section on results, we will compare Sampson and Newcomb role tables by examining their alternative aggregations into non-degenerate three-word tables.

(a) Role table -- original form

	<u>1</u>	<u>2</u>	<u>3</u>	<u>4</u>	<u>5</u>	<u>6</u>	<u>7</u>	<u>8</u>	<u>9</u>	<u>10</u>	<u>11</u>
<u>1</u>	3	5	3	8	9	11	8	8	9	8	11
<u>2</u>	4	6	7	8	10	8	8	8	8	8	8
<u>3</u>	3	9	3	8	9	11	8	8	9	8	11
<u>4</u>	7	10	7	8	8	8	8	8	8	8	8
<u>5</u>	8	11	8	8	8	8	8	8	8	8	8
<u>6</u>	8	8	8	8	8	8	8	8	8	8	8
<u>7</u>	7	8	7	8	8	8	8	8	8	8	8
<u>8</u>	8	8	8	8	8	8	8	8	8	8	8
<u>9</u>	8	11	8	8	8	8	8	8	8	8	8
<u>10</u>	8	8	8	8	8	8	8	8	8	8	8
<u>11</u>	8	8	8	8	8	8	8	8	8	8	8

(b) Role table -- permuted and partitioned

	<u>1</u>	<u>3</u>	<u>2</u>	<u>5</u>	<u>9</u>	<u>4</u>	<u>6</u>	<u>7</u>	<u>8</u>	<u>10</u>	<u>11</u>
<u>1</u>	3	3	5	9	9	8	11	8	8	8	11
<u>3</u>	3	3	9	9	9	8	11	8	8	8	11
<u>2</u>	4	7	6	10	8	8	8	8	8	8	8
<u>5</u>	8	8	11	8	8	8	8	8	8	8	8
<u>9</u>	8	8	11	8	8	8	8	8	8	8	8
<u>4</u>	7	7	10	8	8	8	8	8	8	8	8
<u>6</u>	8	8	8	8	8	8	8	8	8	8	8
<u>7</u>	7	7	8	8	8	8	8	8	8	8	8
<u>8</u>	8	8	8	8	8	8	8	8	8	8	8
<u>10</u>	8	8	8	8	8	8	8	8	8	8	8
<u>11</u>	8	8	8	8	8	8	8	8	8	8	8

(c) Image table

	(1)	(2)	(3)
(1)	(1)	(2)	(3)
(2)	(3)	(3)	(3)
(3)	(3)	(3)	(3)

FIG. 7.—Homomorphic reduction: mapping a Sampson *IN5* table down onto a three-word table. In *a*, the role table is shown in unpermuted form as originally generated. In later notation, *a* is for  $SM(4)-5-I(3)N(3) > .1$ . In *b*, the same table is shown in permuted and partitioned form. In *c*, the multiplication table of the reduction is shown: this is the same table directly obtained in the *KB3* example of fig. 1.

2. Aggregation by homomorphic reduction is a purely algebraic concept: the reduction possibilities make use of structural information in the role table only, and there need not be any consistent parallel reduction at the level of blockmodel graphs.<sup>18</sup> In particular, it is not usually true that a homomorphic reduction may be generated as a Boolean matrix semigroup from matrices which are unions of words mapped into generators under the homomorphism. A more abstract way of making the same observation is to lay emphasis on the decoupling of structural levels. There is a first level of graphs, starting with blockmodel images and later forming compounds in Boolean arithmetic. There is a second level of equations, summarized in the role table. In all cases we have met thus far, the abstract form of a role table may be introduced merely as a shorthand for the matrix form of the table (see again fig. 1). In defining the concept of homomorphic reduction, we are making the promised transition to a genuinely separable level of social structure—one where certain operations may be possible that need have no counterpart at the level of graphs.

*The joint table of two role structures.*—Continuing to develop this point of view, we turn to the problem of comparing two role tables. The two tables may come from blockmodels for the same case derived by different codings of the data, or they may be from entirely distinct cases. It must be possible to identify generator symbols across cases to be compared. Thus, in figure 8, cases *II* and *III* are each generated by elements 1 and 2 (which, by reference to the data, correspond, respectively, to positive and negative sentiment). Now define:

- The *joint reduction* of two role structures on the same set of generators is the largest table consistent with both original role tables, that is, the most refined role table which is a homomorphic reduction of both given tables.

In substantive terms, the joint reduction is the outcome of abstracting structure common to both original role systems; it is the common denominator of the two role structures being compared. In mathematical terms, the joint reduction may be characterized as the intersection (great-

<sup>18</sup> There is such a parallel reduction in the example of fig. 7. Consider the Boolean union of the words in each of the three classes,  $C_i$ . This gives:

$$\begin{array}{lll}
 \begin{array}{ccccc}
 \bar{1} & 1 & 1 & \bar{1} & 0 \\
 1 & 1 & 1 & \bar{1} & 0 \\
 C_1 = 0 & 0 & 1 & 1 & 0 \\
 0 & 0 & 1 & 1 & 0 \\
 1 & \bar{1} & 1 & 1 & 1
 \end{array}
 &
 \begin{array}{ccccc}
 \bar{1} & \bar{1} & 1 & \bar{1} & 1 \\
 \bar{1} & \bar{1} & 1 & 1 & 1 \\
 C_2 = 1 & 1 & 0 & 0 & 1 \\
 \bar{1} & 1 & 0 & 0 & 1 \\
 \bar{1} & 1 & 1 & \bar{1} & \bar{1}
 \end{array}
 &
 \begin{array}{ccccc}
 1 & 1 & 1 & 1 & 1 \\
 1 & 1 & 1 & 1 & 1 \\
 C_3 = 1 & 1 & 1 & 1 & 1 ; \\
 1 & 1 & 1 & 1 & 1 \\
 1 & 1 & 1 & 1 & 1
 \end{array}
 \end{array}$$

a bar has been added to the four 1's in  $C_1$  which are not already present in the first generator, and similarly for  $C_2$ , to show the impact of unioning. It is easy to verify that  $C_1$  and  $C_2$  as generators yield exactly the three-word table of fig. 7c.

est lower bound) of two given semigroups in the reduction lattice (see section on methods). In figure 8, table IV is in fact the joint reduction of tables II and III (and of I and III as well). Thus the simple three-word table of figure 1 is just what the monastery and fraternity cases of figure 8 have in common.

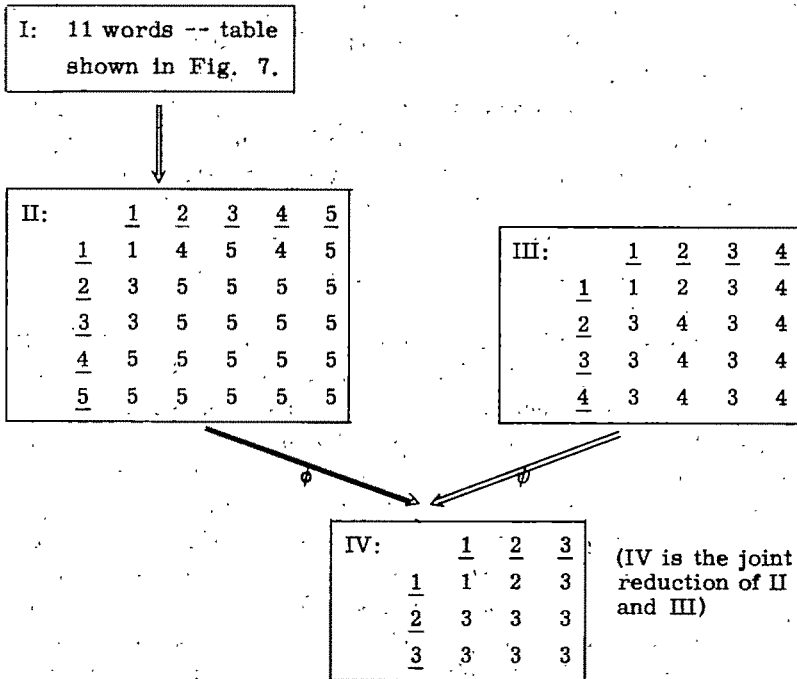


FIG. 8.—Homomorphisms and the joint table. In each of I-III, 1 is the positive sentiment generator and 2 is the negative sentiment generator. Table II is  $SM(4)-3-I(2)N(2)$  and also  $SM(4)-3-I(3)N(3) > .1$ . Table III is  $NF2(10)-3-L(3)A(3) > .1$ . Partitions:

$$II \rightarrow IV: (1)(2,4)(3,5) \leftrightarrow (P_\phi)$$

$$III \rightarrow IV: (1)(2)(3,4) \leftrightarrow (P_\psi)$$

In the section on methods, we develop a numerical measure ( $\delta$ ) of how far apart two role tables are through their joint reduction (see Boorman and Olivier [1973] for the mathematical basis of this strategy). The distance is used to assess reliability across different codings of one case, as well as to measure the similarity of different cases. When reliability is high, the joint reduction table is the robust statement of role structure.

*Inclusion orders and generators.*—Before the joint reduction of role tables from two cases can be obtained, the respective generators must be identified with one another. The face definitions of sociometric indicators

can be misleading, and their various labels are often incomparable. A generalization of the Axiom of Quality suggests one objective criterion: if one image wholly contains another, the former type of tie is a weakened form of the latter and a candidate for homomorphic identification with it. This criterion cannot be directly used to compare different cases, the blockmodels for which may be on different numbers of blocks. Instead, the overall structure of inclusions among distinct words in one blockmodel can be compared with that for another. The rationale will be further developed in the next subsection.

Figure 9 reports inclusion orderings typical for two generators representing positive and negative affect. They are for the fraternity data Newcomb designed to pick up precisely this affect dimension and come from the blockmodels of the last five successive weeks of the first experiment (year 1), when clear social structure had emerged among the four blocks. The similarities among these five orderings are striking, and there seems to be a progression over time.

Now consider figure 10, the inclusion order for the monastery blockmodel in figure 2. There are *eight* generators, as defined by Sampson (or seven, since *D* and *A* coincide). Yet in its gross features figure 10 resembles the typical inclusion ordering for a *pair* of generators: *D*, *A*, and *B* are weakened forms of *N*; *I*, *E*, and *L* are weakened forms of *K*. The positions of the compounds fit with the former four representing negative affect and the latter four representing positive affect: cross-products appear as additional weakened forms of *N* whereas powers and products of the positive four are weakened forms of *K*.

In the section on results we exploit this simplification of the Sampson case. A large role table by itself can be opaque: it may not only be hard to identify the nature of the generators relative to other cases but may also be difficult to see the main features of role interlock within the single table itself.

*Inclusions, equations, and social economy.*—There may be no comparable cases, and no alternate blockmodels for a given case, from which to derive a joint homomorphism. Imposing a single additional equation upon a role table defines a homomorphism, sometimes a sweeping one with just a few equivalence classes because of the large number of other equations implied by the given one. In such an attempt to identify main features of role interlock, one wishes to equate two words which are similar. A word *W* just above another word *X* in the inclusion order is a weakened form of it, as suggested earlier, and more similar to *X* than words elsewhere in the inclusion order.

Such a procedure can also help to identify which of the equations imposed on a role table by a joint reduction are the strategic ones sufficient to imply the rest. The likely candidates are equations of compound words

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<u>Week</u>	<u>Description</u>	<u>Ordering</u>
15	NF1(15)-4-L(3)A(3) > . 1	
14	NF1(14)-4-L(3)A(3) > . 1	
13	NF1(13)-4-L(3)A(3) > . 1	
12	NF1(12)-4-L(3)A(3) > . 1	
11	NF1(11)-4-L(3)A(3) > . 1	

FIG. 9.—Inclusion orderings for Like and Antagonism in final weeks of Newcomb year 1. Notation follows table 1.

to generators. (Note that the words high in the inclusion ordering are "garbage" words in the sense discussed earlier for  $U$ .) If an imposed equation is inconsistent with the detailed interlock in the full table, the resulting reduced table will be degenerate.

Take the inclusion orderings of figure 9 as an example. One obvious

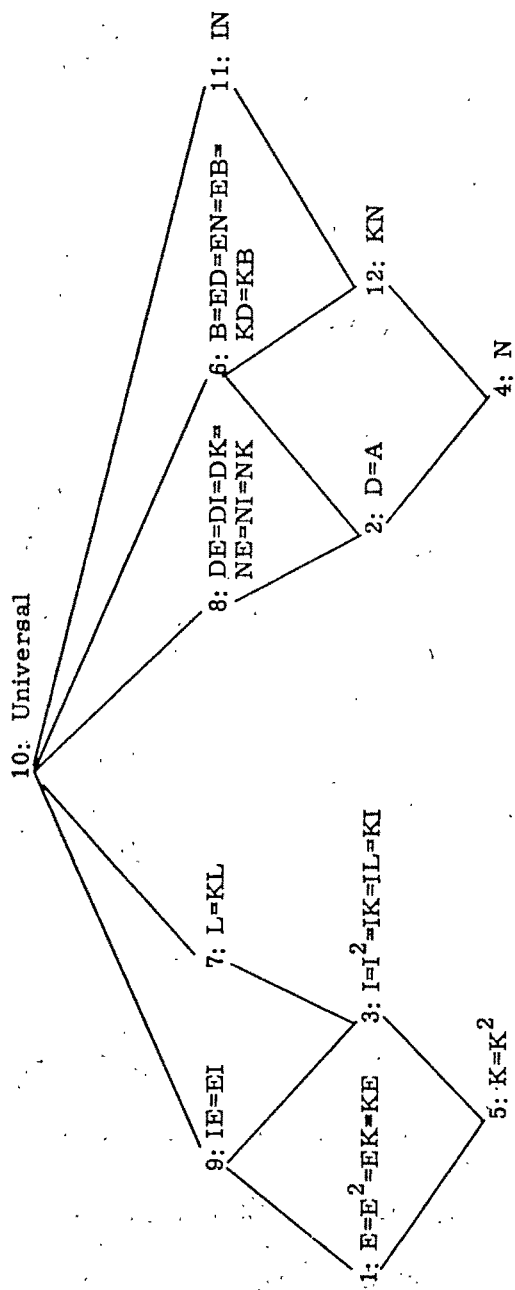


FIG. 10.—Inclusion ordering for words in Sampson eight-generator case (fig. 2). Note the “butterfly” pattern. The ordering forms an upper semilattice.

candidate for a simplifying equation is transitivity of the positive generator:  $L^2 = L$ . There are two competing candidates for the other strategic equation:

$$AL = A, \text{ and } LA = A.$$

The latter is the one observed in the full role table of figure 1, the one imposed successfully in figure 7, and the one resulting from the joint reduction in figure 8. In the discussion of results we shall always see degenerate results from imposing both at once, for all Sampson and Newcomb cases.

As a second example, recall the Bank Wiring Room blockmodel which splits the population into three hierarchical strata. The role structure generated by Like and Antagonism has 11 words, and figure 11 shows both the multiplication table and the inclusion ordering. The latter is strikingly different from figures 9 and 10. There is no overall division into words of positive and of negative quality; there is a word,  $LAL$ , with empty image ( $Z$ ); and  $AL$ , as well as  $LA$ , is contained in  $A$  instead of the reverse. The only equation clearly suggested is transitivity of  $L$ ; the main features of interlock are otherwise not obvious.

An equation is the intersection of two inclusions. The equation in the first example (fig. 1) was discussed in those terms, and the two inclusions were named. In default of the equation, does an inclusion hold, and if so is it always the same one? The typical inclusion orderings for friendship and enmity (figs. 9 and 10) show that the inclusion called social reinforcement holds, but not the one called loyalty. That is, typically any position antagonistic to some given position can count on having friends also antagonistic to the latter position ( $A \subseteq LA$ , but not the reverse). Figures 9 and 10 also show another, less auspicious, face of this social reinforcement: a position can also count on having other enemies who are friends with a given enemy ( $A \subseteq AL$  but not the reverse).

Against this background the equation  $LA = A$  can be interpreted as a development of social economy in affective structure, a tightening of the pattern of social reinforcement into the discipline of a role structure. In the example of figure 1, not only do friends share one's own enmities but also each position shares all the enmities of its friends; the latter tightening could be sought by a particular ego either through adopting as his own enemies all enemies of friends, and/or by dropping friends who have enemies not already his own.

It does not follow that the cognate equation,  $AL = A$ , need develop from its pattern of social reinforcement. Not only can one equation hold without the other, as illustrated by figures 1, 7c, and 8IV, but also their developments would be quite different. An ego does not control the bonds sent by intermediary positions; so in achieving the economy of  $AL = A$



(a) Generators (BW-3-LA)

$$L = \begin{bmatrix} 1 & 1 & 0 \\ 1 & 0 & 0 \\ 0 & 0 & 0 \end{bmatrix} \quad A = \begin{bmatrix} 0 & 0 & 1 \\ 0 & 0 & 1 \\ 1 & 1 & 1 \end{bmatrix}$$

(b) Role table

	<u>1</u>	<u>2</u>	<u>3</u>	<u>4</u>	<u>5</u>	<u>6</u>	<u>7</u>	<u>8</u>	<u>9</u>	<u>10</u>	<u>11</u>	<u>12</u>
<u>1</u>	3	5	3	7	5	10	7	3	7	10	5	7
<u>2</u>	4	6	4	8	9	6	7	8	11	12	11	6
<u>3</u>	3	5	3	7	5	10	7	3	7	10	5	7
<u>4</u>	4	9	4	7	9	12	7	4	7	12	9	7
<u>5</u>	7	10	7	3	7	10	7	3	5	7	5	10
<u>6</u>	8	6	8	8	11	6	7	8	11	6	11	6
<u>7</u>	7	7	7	7	7	7	7	7	7	7	7	7
<u>8</u>	8	11	8	7	11	6	7	8	7	6	11	7
<u>9</u>	7	12	7	4	7	12	7	4	9	7	9	12
<u>10</u>	3	10	3	3	5	10	7	3	5	10	5	10
<u>11</u>	7	6	7	8	7	6	7	8	11	7	11	6
<u>12</u>	4	12	4	4	9	12	7	4	9	12	9	12

(c) Inclusion ordering

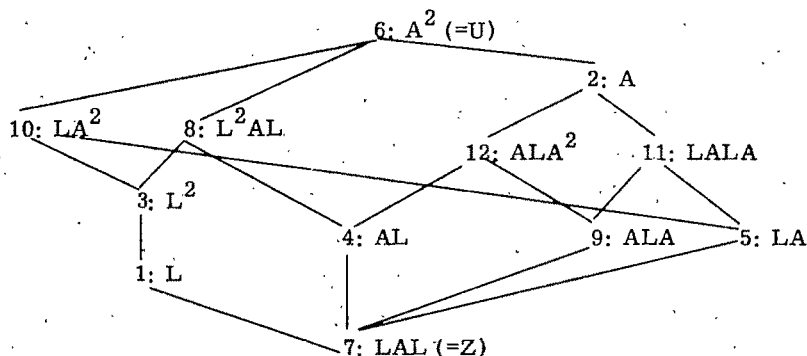


FIG. 11.—Bank Wiring Room: Like and Antagonism on three blocks (stratified model) with zeroblock cutoff.

from the base of  $A \subset AL$ , each ego would have a most delicate synchronization task whether he added or dropped enemies. Nor is the motivation apparent for making enemies of all friends of one's enemies.

The previous paragraphs illustrate a general point: the interpretation of any one equation in a role table depends on what other equations also

hold—and thus on what equations do not hold. The phrase “role interlock” refers to just this point. The main kinds of role interlock are brought out next through systematic examination of possible small role tables.

*Target tables.*—We construct a guide to the outcomes of joint reductions and other homomorphisms for role tables with two generators. It takes the form of an annotated set of eight tables, each with just three words. Two of the tables are typical in cases with positive and negative affect, three occur where neither generator is of negative quality, and three may result from data sets of either sort.

Joint reductions to tables with *no* distinct compound words mean there is nothing, or very little, in common between the original role tables. This is so if the tables are

$$\begin{array}{cc} 1 & 1 \\ 1 & 1, \end{array} \quad \begin{array}{cc} 1 & 2 \\ 2 & 2, \end{array} \text{ or the equivalent.}$$

(Hereafter we suppress the row and column headings for these multiplication tables in the symbolic form introduced by figure 3, since these will always be the integers in ascending order.) The first table above, for example, means that all compounds had to be equated to a generator in order to find a common, and nil, role table.

There is a very strong structure in common, however, if the joint reduction is to the table shown in figure 5a: viz.,

$$\begin{array}{cc} 1 & 2 \\ 2 & 1. \end{array}$$

This is the table for classical balance theory, with 1 the positive generator, which is transitive, and 2 the generator for negative affect. It has not emerged from any of the joint or other reductions applied to our collection of empirical cases; in particular, there is rarely any basis for equating 2\*2 with 1 (enemies' enemies with friends).

There are two other significant possibilities for tables of two words alone:

$$\text{First Letter} = \begin{array}{cc} 1 & 1 \\ 2 & 2 \end{array}, \text{ and } \text{Last Letter} = \begin{array}{cc} 1 & 2 \\ 1 & 2 \end{array}.$$

By the First Letter table, any compound word of whatever length is equated to the generator which occurs first in the word (that is, leftmost). A substantive interpretation is that any type of indirect bond takes on the quality of the direct tie to the first intermediary. The Last Letter table is, formally, simply the dual in which the last generator determines the quality of each type of compound. The First Letter table, or a refine-

ment of it, is often found when one generator is for an objective, though positive, sort of tie (e.g., similar policy), whereas the other denotes positive affect: thus one's tie to any kind of contact of one's business associate takes on the color of a business association, whereas one views in affective terms any kind of contact through a friend. This kind of ego-oriented interpretation does not extend to the Last Letter table, where the compound tie is colored by the kind of tie from the last middleman in the compound.

When not all compounds are equated to one or another of the generators, a broader spectrum of role interlock can be specified through tables with just one distinct compound. In particular, significant refinements of the two useful smaller tables (First Letter and Last Letter) may be distinguished. The resulting tables will be assigned labels  $T_1$ – $T_8$  for reference in the section on results. At least one generator, for convenience the first, will always be assumed transitive:  $1*1 = 1$ .

Two of these tables, found for positive and negative affect cases, have already been discussed and contrasted at some length:

$$T_1 = \begin{array}{ccc} 1 & 2 & 3 \\ 3 & 3 & 3 \\ 3 & 3 & 3 \end{array}, \text{ and } T_2 = \begin{array}{ccc} 1 & 3 & 3 \\ 2 & 3 & 3 \\ 3 & 3 & 3 \end{array}.$$

In all eight tables, the distinct compound, labeled 3, acts as the garbage word discussed earlier: entries of 3 in a table do not convey positive information but permit and contrast with equations to the generators. The natural contrast to both  $T_1$  and  $T_2$  is

$$T_8 = \begin{array}{ccc} 1 & 2 & 3 \\ 2 & 3 & 3 \\ 3 & 3 & 3 \end{array},$$

in which the two generators commute. In classical balance the generators also commute, but  $T_8$  does not have the classical balance table above as a homomorphic reduction, because  $2*2$  in this table cannot be mapped into 1 without inducing total degeneracy.

James A. Davis (1968) has developed an alternative to classical balance in which, in our terms, he rejects the equation of  $2*2$  with 1 but does not specify a unique alternative. The population must be split into at least three blocks, and with three blocks the extreme form of the Davis theory, expressed as a blockmodel, is

$$L = \begin{array}{ccc} 1 & 0 & 0 \\ 0 & 1 & 0 \\ 0 & 0 & 1 \end{array}, \text{ and } A = \begin{array}{ccc} 0 & 1 & 1 \\ 1 & 0 & 1 \\ 1 & 1 & 0 \end{array}.$$

The role table of this extreme Davis blockmodel is precisely  $T_8$ . We shall

see that  $T_3$  also does not emerge from our empirical reductions, so that commutativity of the generators is suspect, even without the mapping of  $2 \times 2$  into  $1$ .<sup>19</sup>

When neither generator represents negative affect, a role table showing no interlock between the generators, but only transitivity for each, is  $T_8$ , shown below with four other possibilities:

$$\begin{array}{ccccc}
 \begin{array}{ccc} 1 & 2 & 3 \\ 3 & 2 & 3 \end{array} & , & \begin{array}{ccc} 1 & 3 & 3 \\ 3 & 3 & 3 \end{array} & , & \begin{array}{ccc} 1 & 1 & 1 \\ 3 & 3 & 3 \end{array} & , & \begin{array}{ccc} 1 & 3 & 3 \\ 1 & 3 & 3 \end{array} & , & \begin{array}{ccc} 1 & 3 & 3 \\ 3 & 3 & 3 \end{array} \\
 T_4 = & T_5 = & T_6 = & T_7 = & T_8 =
 \end{array}$$

The other four are variants of the First Letter table ( $T_5$  and  $T_6$ ) or the Last Letter table ( $T_4$  and  $T_7$ ):  $T_4$  is like  $T_1$  but with an additional important equation, namely transitivity of 2, which changes the significance of the existing equation;  $T_4$  will be found to result also from cases with negative generators, where the negative bonds are so directed toward one block as to make the negative image transitive (compare fig. 16 below for Newcomb year 2).<sup>20</sup>

It is not necessary to turn to tables with two or more distinct compound words in order to distinguish the main kinds of role interlock found in our cases. There are other tables on three words than the eight above, but none have been found useful. Associativity, a requirement imposed

<sup>19</sup> Like classical balance, Davis balance assumes, in our terms, that positive and negative images have no overlap; there is no ambiguity. Other blockmodels consistent with Davis's theory yield the multiplication table

$$\begin{array}{ccc}
 1 & 2 & 3 \\
 2 & 3 & 2 \\
 3 & 2 & 3
 \end{array}$$

but so does the blockmodel

$$L = \begin{array}{ccc} 1 & 0 & 0 \\ 0 & 1 & 0 \\ 0 & 0 & 1 \end{array}$$

and

$$A = \begin{array}{ccc} 0 & 1 & 1 \\ 1 & 0 & 1 \\ 0 & 0 & 1 \end{array}$$

the generators of which do overlap. This table, in contrast to  $T_3$ , has the classical balance table as a reduction, because the table equates  $2 \times 2 \times 2$  with 2 (semitransitivity) although not  $2 \times 2$  with 1. Thus some cases of Davis's theory are consistent with the multiplication table of classical balance, but so are other blockmodels exhibiting ambiguity.

<sup>20</sup> As emphasized earlier, many blockmodels can yield any given role table. Each of these eight tables of three words can be produced by some blockmodel on just two blocks: in the notation of fig. 2 in Part I, such pairs of generators are  $T_1(V, F)$ ;  $T_2(V, E)$ ;  $T_3(P, F)$ ;  $T_4(V, T)$ ;  $T_5(V, G)$ ;  $T_6(G, E)$ ;  $T_7(H, E)$ ; and  $T_8(V, W)$ .

by the nature of matrix multiplication, precludes many possibilities. For example,  $T_3$  with the transitivity entry replaced by  $1*1 = 3$  is impossible: commutativity of generators is possible only if in addition at least one is transitive (or if one is semitransitive; see n. 19).

# METHODS

Our models for role structure are *semigroups* (Clifford and Preston 1961; Kurosh 1965).

*Definition 1.* An *abstract semigroup* is an ordered pair  $(S, *)$  where  $S$  is a set and  $*$  is a binary operation  $*: S \times S \rightarrow S$  which satisfies associativity, that is, for which

$$a*(b*c) = (a*b)*c$$

for all  $a, b, c \in S$ .

As long as  $S$  is finite, the entire structure of the semigroup can be summarized through the *multiplication table* of the operation  $*$ . We have already seen examples (e.g., fig. 1).

We make use only of concretely presented semigroups in which *generators* have been identified.

*Definition 2.* A *semigroup with generators* is a triple  $(S, G, *)$  where  $(S, *)$  is a semigroup,  $G \subseteq S$ , and any element  $a$  of  $S$  may be expressed as a product of elements in  $G$ :

$$a = g_1 * g_2 * \dots * g_k, \quad g_i \in G.$$

Thus in the examples of figure 1,  $G = \{K, B\}$ , while in figure 2,  $G = \{E, D, L, A, I, N, K, B\}$ , and in figure 6a,  $G = \{L, A\}$ , etc. As those examples made clear, the selection of generators is a substantive matter.

A useful alternative way to characterize the structure of a semigroup with generators proceeds through the intermediate concept of a *free semigroup*:

*Definition 3.* The *free semigroup* generated by a set  $G$  consists of all possible finite strings of elements  $g_i \in G$  with multiplication  $*$  given by the operation of concatenating strings.

Thus if  $W_1 = ALA$ , and  $W_2 = AL^2$  in the free semigroup on  $G = \{L, A\}$ ,  $W_1 * W_2$  is  $ALA^2L^2$ , the result of concatenating the two sequences.

Designate the free semigroup on  $G$  by  $FS(G)$ . Note that  $FS(G)$  will always be infinite even though  $G$  contains only one letter. In category theory (Mitchell 1965),  $FS(G)$  may also be characterized as a universal object in the relevant semigroup category.

Then if  $(S, G, *)$  is any semigroup with generators, the structure of  $(S, G, *)$  is uniquely characterized by the partition  $P$  of  $FS(G)$  obtained by putting two words in  $FS(G)$  in the same equivalence class of the partition if and only if they evaluate to the same element of  $S$  in the multiplication table of  $(S, G, *)$ .

Note that the partition  $P$  will have a finite number of cells as long as  $S$  is finite, but certain cells must then have an infinite number of members (pigeonhole principle). This means that although some words may have unique expressions as a product of generators, at least one must have an infinite number of alternative expressions. Accordingly, the letter form of the multiplication table is in part arbitrary. This is a basis for preferring the numerical form of tables (e.g., fig. 3), even though it is more abstract and less immediately interpretable.

Our semigroups result from computations with matrices.

*Definition 4.* A *Boolean matrix semigroup* is a semigroup generated by one or more specified binary matrices under Boolean matrix multiplication.

In all our examples, the generator matrices are blockmodel images rather than matrices representing raw network data; this is a basic difference between the point of departure of the present paper and the earlier approaches of White (1969) and Lorrain and White (1971).

It is important to distinguish between the *matrix representation* of a particular role structure obtained from Definition 4 and the purely algebraic interpretation of the same structure under Definition 2. The matrix representation obviously subsumes all information contained in the abstract multiplication table. It also contains additional information associated with the matrices representing particular compound words. Both levels should be clearly kept in view at all times, since certain interpretations consistent with the algebraic level will be inconsistent with the matrix level, as was discussed in the section on theory.

After some brief comments on the mathematical and statistical properties of Boolean matrix semigroups, we will move to the concept of a homomorphism and its application to measuring distance between semigroups.

### Boolean Matrix Semigroups

Computation of all distinct product words generated from a particular blockmodel, and thereby the multiplication table of the associated role structure, was speeded by use of a computer program named TABWDL.<sup>21</sup> Normally, all distinct compounds were found using only words of three or fewer letters, and never of more than five letters. The semigroups obtained are small.<sup>22</sup> In cases involving three to five blocks, most sizes are in the

<sup>21</sup> TABWDL, in the APL language, is available upon request. It was adapted by the second author from an annotated package of APL programs created by Mr. G. H. Heil (see Heil and White 1976) to analyze homomorphisms of blockmodel semigroups.

<sup>22</sup> Working in the context of the old approach using sociometric matrices as generators (Lorrain and White 1971), we performed a Monte Carlo investigation of semigroup sizes. Taking populations of sizes about 10, two random square matrices of fixed dimension were formed on each trial, using a fixed probability  $p$  for a 1 in

range 3–15, with a mild tendency for more blocks to generate larger semigroups (see the several series of semigroup sizes in the second columns of figs. 15–18). As is also reasonable, semigroups on three generators are somewhat larger than those on two, but sizes are still in the indicated range. We have seen one example (fig. 3) in which the semigroup of an eight-generator model gives only 12 words; various alternative cutoff densities for the same generators give semigroups of sizes up to 25.

*Inclusion orderings.*—Given any Boolean matrix semigroup, the associated inclusion ordering is the partial ordering obtained from the natural ordering of matrices:

$$M_1 \leq M_2 \iff M_1(i, j) \leq M_2(i, j) \text{ for all } (i, j).$$

This suggests the following class of structures:

*Definition 5.* A *partially ordered (p.o.) semigroup with generators* is a quadruple  $(S, G, *, \leq)$  where  $(S, G, *)$  is a semigroup with generators and  $\leq$  is a partial ordering of  $S$ .

This is a somewhat more general characterization than usually adopted by algebraists, where monotonicity of  $*$  with respect to  $\leq$  is assumed (thus  $a \leq b \Rightarrow c*a \leq c*b$  and  $a*c \leq b*c$  for all  $a, b, c \in S$  [see Fuchs 1963; Vinogradov 1969]).

It is obvious that  $(S, G, *, \leq)$  contains new information that cannot be obtained from  $(S, G, *)$ . In the cases we consider, it is not usually possible to place additional a priori restrictions on the ordering, such as requiring it to be a lattice or an upper semilattice (Friedell 1967). Notice, however, that a  $U$  pattern will always be a maximum element and a  $Z$  pattern a minimum element (cf. fig. 11c, where both elements appear). Because they tend to be more densely filled as matrices, longer words usually occupy higher positions in a semigroup p.o.; generators are often (though not necessarily) minimal elements.

### Homomorphisms

Starting from Definition 2, one may define a homomorphism as follows:

*Definition 6.* If  $(S_1, G_1, *)$ ,  $(S_2, G_2, \circ)$  are semigroups with specified generator sets  $G_1$  and  $G_2$ , a homomorphism  $\phi: S_1 \rightarrow S_2$  is a mapping which: (i) preserves semigroup operations:  $\phi(a*b) = \phi(a) \circ \phi(b)$ ; (ii) maps generators to generators:  $\phi(g_1) = g_2 \in G_2$  for all  $g_1 \in G_1$ .

each entry. The results indicated that semigroups would be quite small for  $p$  under about .08 and above about .25. In the .08–.25 range, sizes were larger and it was difficult to compute the mean size since limited storage capacity compelled truncation of semigroup sizes at  $N = 65$  elements. (Interestingly, many sociometric relations have densities in exactly the difficult .08–.25 range.) It may be possible to obtain at least asymptotic estimates and bounds on semigroup sizes by adapting the probabilistic methods of the Hungarian school of combinatorics. We are indebted to Paul Erdős for discussion of technical aspects of this problem.

Our attention is confined to homomorphisms having two additional properties as follows:

*Definition 7.* A homomorphism  $\phi$  is a *homomorphic reduction* of  $(S_1, G_1, *)$  if  $\phi$  maps  $S_1$  onto  $S_2$ , that is, for any  $c \in S_2$  one has  $\phi(a) = c$  for some  $a \in S_1$ .

As already explained, a homomorphic reduction is an algebraic concept of aggregation, and  $(S_2, G_2, \circ)$  is a coarser version of the structure  $(S_1, G_1, *)$ .

*Definition 8.* A homomorphism  $\phi$  *preserves generators* if  $\phi$  maps  $G_1$  onto  $G_2$  in a 1-1 way, that is,  $\phi$  sets up a 1-1 correspondence between generators.

Definition 8 clearly implies that  $(S_2, G_2, \circ)$  is also a homomorphic reduction of  $(S_1, G_1, *)$ , since every word in  $S_2$  can be expressed as a product of generators in  $G_2$ . In earlier discussion of homomorphisms, we assumed Definition 8 implicitly, because the identification of a generator from one empirical case with one of the generators from another case is an important substantive issue.

### The Joint Reduction of Two Semigroups

Given any semigroup with generators, we have already noted that it is possible to interpret it as a partition of  $FS(G)$ . If two semigroups  $(S_1, G, *)$  and  $(S_2, G, \circ)$  have the same generator set (or identified generator sets), each may therefore be interpreted as a partition of the same free semigroup. Since partitions form a natural lattice (Birkhoff 1967), this suggests that the set of all semigroups generated by  $G$  may also be endowed with a lattice ordering. This is in fact correct and is the formal basis of our approach to comparing role structures.

Without loss of generality, we will now assume that all semigroups to be compared are formed from a common generator set  $G$ . In this case, by a *generator-preserving* (GP) homomorphism, we will mean a homomorphism which is the identity mapping on generators, that is,  $\phi(g) = g$  for all  $g \in G$ .

In order to define the semigroup which we call the *joint reduction* of  $(S_1, G, *)$  and  $(S_2, G, \circ)$ , it is necessary to characterize the natural semigroup lattice in more exact terms.

*Definition 9.* Consider the family of all finite semigroups  $(S, G, *)$  on the fixed generator set  $G$ , and define the relation  $\subseteq$  where

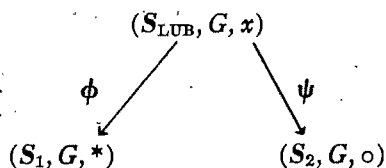
$$(S_1, G, *) \subseteq (S_2, G, \circ) \iff (S_1, G, *) \text{ is a GP homomorphic reduction of } (S_2, G, \circ).$$

It is immediately obvious that  $\subseteq$  is a partial ordering. The statement that  $\subseteq$  also forms a lattice is embodied in the following proposition:

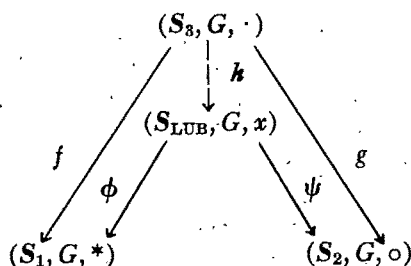
*Proposition:*  $\subseteq$  is a lattice ordering, that is, for any two semigroups



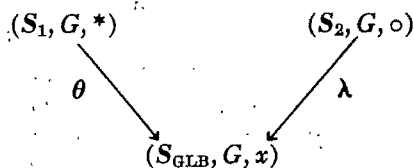
$(S_1, G, *)$  and  $(S_2, G, \circ)$ , (i) there is a semigroup  $(S_{LUB}, G, x)$  which is a least upper bound for  $(S_1, G, *)$  and  $(S_2, G, \circ)$  in the ordering, that is, for which (1) there are GP homomorphic reductions  $\phi$  and  $\psi$



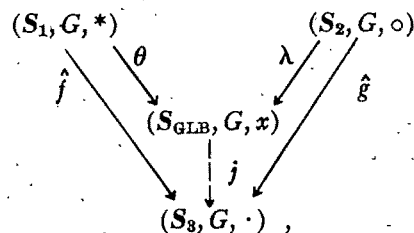
and (2) if one is given GP reductions  $f$  and  $g$  from some third semigroup down onto  $(S_1, G, *)$  and  $(S_2, G, \circ)$ ,



it is possible to construct the indicated GP reduction  $h$ . (ii) There is a semigroup  $(S_{GLB}, G, x)$  which is a greatest lower bound for  $(S_1, G, *)$  and  $(S_2, G, \circ)$ : that is, for which (1) there are GP reductions  $\theta$  and  $\lambda$



and (2) if  $\hat{f}, \hat{g}$  are also GP reductions



the indicated GP reduction  $j$  exists.

*Proof:* See Birkhoff (1967), where the same statement is established for an arbitrary abstract algebra, of which semigroups are one special case.

Given this proposition, the definition of the joint reduction immediately follows:

*Definition 10.* The *joint reduction* of two semigroups  $(S_1, G, *)$  and  $(S_2, G, \circ)$  is the semigroup  $(S_{\text{GLB}}, G, x)$  whose existence and uniqueness follows immediately from the above proposition.

In looser but more intuitive terms, it is possible to characterize the joint reduction as the result of imposing the union of all equations implied by each of the multiplication tables of  $(S_1, G, *)$  and  $(S_2, G, \circ)$ . Viewed as a partition of  $\text{FS}(G)$ , the GLB semigroup is hence the *union* of the partitions corresponding to  $(S_1, G, *)$  and  $(S_2, G, \circ)$  (Birkhoff 1967). This makes clear that the joint reduction is generally a coarser structure than either of the original semigroups, since in it more words are forced to be equal. Obviously, the size of the joint reduction is bounded above by the sizes of  $S_1$  and  $S_2$ . Note that a fixed 1-1 mapping of generators in  $S_1$  to those in  $S_2$  is implied throughout the construction: *if a different matching is used, an entirely different joint reduction will probably result.*

A special case exists when one of  $(S_1, G, *)$  and  $(S_2, G, \circ)$  is already a GP reduction of the other. Without loss of generality, assume that  $(S_1, G, *)$  is a GP reduction of  $(S_2, G, \circ)$ . Then the joint reduction is itself  $(S_1, G, *)$ , as may be verified formally from the above proposition. This makes sense: if two semigroups are already homomorphically comparable, the result of imposing the equations of both should lead identically to the coarser structure.

Observe that if  $(S_1, G, *)$  and  $(S_2, G, \circ)$  are each Boolean matrix semigroups, there is no implication that the joint reduction will also possess a naturally induced matrix representation. The joint reduction is therefore a purely algebraic measure of the consistency of two role structures and has no necessary correlates at the matrix level.

Actual computation of a joint reduction may be complicated. It is effected by a computer program, *JNTHOM*, which contains a separate subprogram for effecting arbitrary homomorphisms.<sup>23</sup> The resulting intersection table may itself be large and complex, and it may appear much closer to one than to the other of the original tables. For both reasons an objective measure of distance between the original tables is desirable.

*Distance measure.*—Using the joint reduction, we seek a numerical measure of distance between two role structures. We require the respective generator sets to be in one-to-one correspondence. From data already given, it is apparent that the raw size of a role structure is not a stable measure across closely related blockmodels. Accordingly, we are led to

<sup>23</sup> The *JNTHOM* program is available upon request; it is written by the second author in the APL language. We are indebted to Dr. François Lorrain for supplying an alternative program, *MASTERGLB*, based on an independent approach, together with an elegant proof of its validity.

reject the naive alternative of defining similarity (and thus distance) through the size of the joint reduction.

Instead, return to figure 8 and consider the *distributions* of words in  $S_1$  and  $S_2$  which are implied by the joint reduction. Specifically, consider the partitions of  $S_1$  and  $S_2$ , respectively, that are obtained from the *inverses* of the reductions  $\phi$  and  $\psi$ :

$$\begin{aligned} a \cong b \text{ (in } S_1) &\iff \phi(a) = \phi(b) \\ c \cong d \text{ (in } S_2) &\iff \psi(c) = \psi(d). \end{aligned}$$

We are accordingly led to equate words in  $S_1$  (respectively, in  $S_2$ ) which map similarly under  $\phi$  (respectively,  $\psi$ ). This gives rise to a partition of  $S_1$  and a partition of  $S_2$ ; it makes sense to treat the coarseness of these partitions as a measure of the extent of aggregation in passing from  $S_1$  or  $S_2$  to the joint reduction, in other words, as a measure of distance to the joint reduction (Boorman 1970; Boorman and Arabie 1972).

The coarseness of a partition is a standard concept familiar from the entropy measure of information theory. For present purposes, we adopt a somewhat different definition, which has been shown to have more desirable numerical properties than an entropy measure when used in conjunction with multidimensional scaling (Arabie and Boorman 1973). This measure is:

$$h(P) = \frac{\sum_{c \in P} \binom{|c|}{2}}{\binom{N}{2}}$$

where

$P = (c_1, c_2, \dots, c_m)$  is a partition of a finite set  $S$  into nonempty and disjoint subsets  $c_i$ ;

$|c_i|$  = size of  $c_i$ ;

$N$  = size of  $S$ .

The measure  $h(P)$  assumes a maximum value of 1 if  $P$  is the "lumper" partition in which  $S$  is not divided at all;  $h(P)$  assumes a minimum value of 0 if  $P$  is the "splitter" partition which places each element of  $S$  in a separate cell. This last possibility will occur when one of  $S_1, S_2$  is itself the joint reduction, so that one of the mappings  $\phi, \psi$  is the identity mapping.

Applying this measure to the present problem, define  $P_\phi$  and  $P_\psi$  to be the partitions of  $S_1$  and  $S_2$ , respectively, which are induced by the joint reduction (see fig. 8 for an example). Define  $h(P_\phi)$  (respectively,  $h(P_\psi)$ ) to be the distance between  $(S_1, G, *)$  (respectively,  $[S_2, G, o]$ ) and the common joint reduction. Finally, therefore, one has:

*Definition 11.* The distance between two semigroups  $(S_1, G, *)$  and  $(S_2, G, \circ)$  is  $\delta([S_1, G, *], [S_2, G, \circ]) = h(P_\phi) + h(P_\psi)$ , where  $P_\phi$  and  $P_\psi$  are determined by the joint reduction.

The measure  $\delta$  will be our basic tool of numerical comparison. Its range is from 0 to 2. The maximum value is attained when all entries in the joint reduction table are the same; that is, maximum degeneracy. For the case shown in figure 8,  $h(P_\phi) = 0.200$  and  $h(P_\psi) = 0.167$ , whence  $\delta = 0.367$ . This indicates that the semigroups are quite close, even though the multiplication tables are superficially quite different.

From a formal standpoint, the measure  $\delta$  has two relevant formal properties:

1. It makes full use of the structure of both  $(S_1, G, *)$  and  $(S_2, G, \circ)$ , since it depends on  $P_\phi$  and  $P_\psi$  and these partitions depend on the reductions  $\phi$  and  $\psi$ , respectively.

2. It is a semimetric, that is, it has the following two properties:

i)  $\delta([S_1, G, *], [S_2, G, \circ]) \geq 0$ , and  $= 0$  if and only if

$[S_1, G, *], [S_2, G, \circ]$  coincide (i.e., are isomorphic).

ii)  $\delta([S_1, G, *], [S_2, G, \circ]) = \delta([S_2, G, \circ], [S_1, G, *])$ .

The measure  $\delta$  is *not* a metric, that is, it is possible to construct examples violating the triangle inequality so that

$$\begin{aligned} \delta([S_1, G, *], [S_2, G, \circ]) + \delta([S_2, G, \circ], [S_3, G, x]) \\ > \delta([S_1, G, *], [S_3, G, x]). \end{aligned}$$

It would be possible to eliminate this apparent difficulty directly by an appropriate transformation of  $\delta$ . Since, however, we are going to use  $\delta$  exclusively in conjunction with scaling, we will use the uncorrected measure as input and allow the scaling itself to determine an appropriate transformation through the Shepard diagram.

**Multidimensional scaling.**—When a large number of blockmodel role structures are to be compared with one another, it would be hopeless to try to discuss explicitly all the possible joint reductions. Even the numerical distances among all possible pairs may be quite difficult to assess as a set (see fig. 12 below for an example). In such situations, a natural strategy is to scale the half-matrix of distances using some form of multidimensional scaling (see Shepard 1962a, 1962b; Kruskal 1964; Shepard 1974).

In the present paper, all scaling applications use the MDSAL-5 algorithm with the Euclidian metric ( $r = 2$  in the scaling solution), stress formula 1 (Kruskal and Carroll 1969) and the primary approach to ties (Kruskal 1964). Scalings were obtained from a substantial number of alternative

random initial configurations, and the solution giving the best (i.e., lowest) stress was chosen for presentation. It was found that many of the starting configurations tried gave rise to clearly nonoptimal local minima, thus supporting the earlier findings of Arabie (1973) on other data sets.

## RESULTS

The results to be considered fall naturally into two parts. First, surveys of distances within large sets of role tables, including variants of given cases, are used to assess the robustness of our approach and to locate different case studies in relation to one another. Second, role structures for particular cases are examined in detail and compared via their joint reductions. As a preliminary, we lay out a consistent notation for identifying role table generators for the numerous cases, over time and with variant codings.

### Nomenclature

All of the case studies in Part I involve multiple networks; some (Sampson, Newcomb) involve data over time; each can be described by a number of blockmodels, some involving different numbers of blocks. In addition, while Part I does not formalize it, there is also a concept of *cutoff density* for coding zeroblocks: in many cases (as exemplified by the Similar Policy relation in the Firth-Sterling data) a given block may contain only one or two ties, so that measurement error in the data suggests coding as a zeroblock rather than as a bond. All of these factors contribute to a proliferation of blockmodels, each possessing an associated semigroup. A system of notation is developed in table 1.

We draw a strict distinction between the determination of a blockmodel partition on the one hand and the coding of bonds on the other. *All blockmodel partitions used in this paper are shown in panel IV of table 1; how they are determined is considered to be a task for the methods of Part I and will not be further discussed here* (see also Breiger, Boorman, and Arabie 1975; Schwartz 1977). Accordingly, the notation merely reminds the reader of the number of blocks and is directed toward the coding of bonds (see panel II of table 1 for an example).

For given density matrices one may define a one-parameter (piecewise constant) family of semigroups  $S(\alpha)$ ,  $\alpha > 0$ , where  $\alpha$  is the density cutoff. If  $\alpha = 0$  a strict zeroblock criterion is being employed. As indicated by the basic blockmodel phenomenology of Part I, we are interested only in  $\alpha$  values that are small compared to the average density. (As  $\alpha$  increases further, more and more blocks are coded as zeroblocks until

TABLE 1  
NOTATION FOR GENERATORS

I. The general format is:

(case study initials)(time period)-(number of blocks)- $X(i)Y(j) > \alpha$  where cutoff density  $\alpha$  is being imposed on both generators  $X$  and  $Y$ , taking top  $i$  and top  $j$  choices, respectively, *weighted* linearly (see example). The first letter in the name of that type of tie is  $X$  or  $Y$ . The specific partition of persons into blocks for each case study is reported in IV below.

*Conventions:*

1. If a strict zeroblock criterion is being employed,  $\alpha = 0$  and the final inequality is suppressed. Thus  $SM(4)-5-I(2)N(2) > 0$  becomes shortened to  $SM(4)-5-I(2)N(2)$ .
2. When the data are observer reported (e.g., in the Bank Wiring group), there is no ordering of choices and ties are not weighted in computing the density. Then  $i$  and  $j$  are not required: thus  $BW-3-LA$  identifies Like and Antagonism in the Bank Wiring group, using a three-block model.
3. If the data are not longitudinal, time period is suppressed.
4. Each density is computed relative to the number of possible entries in a block, excluding the diagonal if the block is a diagonal block. Thus the density in an  $r \times r$  diagonal block is

$$\sum w(i,j)/r(r-1)$$

entries where  $w(i,j)$  is the weight of the choice in the  $(i,j)$ th entry within the block. The density of an  $r \times c$  off-diagonal block is

$$\sum w(i,j)/r \cdot c.$$

II. Example:  $SM(4)-5-E(3)D(3) > 1$

This identifies the matrix semigroup formed from the Sampson data, time 4, five-block model (see IV below), using cutoff density  $\alpha = .1$  on the weighted top three choices for the Esteem and Disesteem relations (weight 3 for top choice, etc.). The weighted densities are (to three figures):

$$\begin{bmatrix} 0.167 & 0.833 & 0.111 & 0 & 0 \\ 0.667 & 1.17 & 0 & 0 & 0 \\ 0.222 & 0.417 & 1.67 & 0.083 & 0 \\ 0 & 0 & 1.5 & 0.917 & 0 \\ 0.25 & 0.063 & 0.5 & 0.125 & 1.17 \end{bmatrix} \text{ (Esteem)}$$

$$\begin{bmatrix} 0 & 0 & 0 & 0 & 1 \\ 0 & 0.083 & 0.75 & 0.375 & 0.688 \\ 0 & 0.25 & 0.333 & 0.25 & 0.333 \\ 0 & 1.12 & 0 & 0 & 1.06 \\ 0.083 & 1.31 & 0.25 & 0 & 0.083 \end{bmatrix} \text{ (Disesteem).}$$

Cutoff  $\alpha = .1$  leads to:

$$K = \begin{bmatrix} 1 & 1 & 1 & 0 & 0 \\ 1 & 1 & 0 & 0 & 0 \\ 1 & 1 & 1 & 0 & 0 \\ 0 & 0 & 1 & 1 & 0 \\ 1 & 0 & 1 & 1 & 1 \end{bmatrix} \quad D = \begin{bmatrix} 0 & 0 & 0 & 0 & 1 \\ 0 & 0 & 1 & 1 & 1 \\ 0 & 1 & 1 & 1 & 1 \\ 0 & 1 & 0 & 0 & 1 \\ 0 & 1 & 1 & 0 & 0 \end{bmatrix}$$

The semigroup is of size 10.

III. Case study names

- BW: Bank Wiring Room  
 SM: Sampson monastery ("Praise" in fig. 4 of Part I has been changed to "Kudos" to avoid "P" as the initial letter).  
 NF1: Newcomb fraternity, year 1.  
 NF2: Newcomb fraternity, year 2  
 FS: Firth-Sterling Corporation, management  
 GS: Griffith scientists (here  $C$  stands for mutual contact ties, and  $A$  for asymmetric unawareness. See Part I).

TABLE 1 (Continued)

## IV. Standard blockmodel partitions

BW-2:	(W1 W3 W4 S1 I1 W2 W5 I3) (W7 W8 W9 S4 W6 S2)
	<i>Numbering:</i> Follows Homans (1950); see also Part I.
BW-3:	(W3 W4 S1 W8 W9) (W1 I1 W7 S4) (W2 W5 S2 W6 I3)
BW-6:	(W4 S1 W3) (W1 I1) (W2 W5 I3) (W8 W9) (W7 S4) (W6 S2)
SM-3:	(10 5 9 6 4 11 8) (12 1 2 14 15 7 16) (13 3 17 18)
	<i>Numbering:</i> Follows Sampson (1969); see also Part I.
SM-5:	(10 5 9) (6 4 11 8) (12 1 2) (14 15 7 16) (13 3 17 18)
NF1-4:	(1 3 4 6 12) (2 11 13) (5 9 10 14 15) (7 8 16 17)
	<i>Numbering:</i> Follows Nordlie (1958) and Newcomb (1961).
NF2-3:	(13 9 17 1 8 6 4) (7 11 12 2) (14 3 10 16 5 15)
	<i>Numbering:</i> Follows Nordlie (1958) and Newcomb (1961); see also Part I.
FS:	(11 9 13) (2 4 6 7 8 1 15) (5 10 12 14 3 16)
	<i>Numbering:</i> Follows White (1961); see also Part I.
GS:	(9 26 23 4 1) (12 7 6 2 24 19) (14 28 11 10 18 22 15) (16 20 17 5 8 13 21 27 25 3)
	<i>Numbering:</i> Follows Part I; see also Breiger 1976.

eventually, when  $\alpha$  exceeds the maximum density, all generators become trivial  $Z$  patterns.) For example, the average density is .375 in the three-block model for the final week (week 15) of the Newcomb year 2 data, with top three choices weighted. The density matrices for the generators are<sup>24</sup>

	0.950	0.071	0.048		0	0	1
L:	0.357	1.08	0.042	A:	0.071	0	0.917
	0.571	0.417	0.067		0.095	0.083	1

Here there is a clear dichotomy between low density ( $<0.1$ ) and high density ( $>0.35$ ) blocks, indicating a preferred cutoff which separates these two ranges. More typical, however, is the Sampson *ED* case illustrated in table 1. Here the density sequence begins (0, .063, .083, .111, .125, .167, .222, .250, . . .) and there is no clearly preferred cutoff. In such cases, we have accordingly explored a variety of alternative cutoffs, with standard  $\alpha$  values being .1, .15, and .2.

## A Global Geometry for Types of Role Structures

Results for two sets of role structures lead to an interpretation of the relations among the main case studies.

Table 2 identifies the first set of role structures, using the nomenclature of table 1. This first sample is intended as a potpourri from all our main

<sup>24</sup> Compare with the bottom panel of fig. 7 in Part I, where unweighted densities of choices are used taking only the top two and bottom three choices for each man. Generally, a cutoff of  $\alpha=0.1$  with weighted top three choices yields the same or nearly the same generators as for the strict zeroblock cutoff on the top two choices which was emphasized in Part I.

TABLE 2

IDENTIFICATIONS OF ROLE TABLES IN FIGURES 12 AND 13

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1. BW-2-LA	10. SM(4)-5-L(2)A(2)
2. BW-3-LA	11. SM(4)-5-E(2)D(2)
3. BW-6-LA	12. SM(4)-5-I(2)N(2)
4. NF1(13)-4-L(2)A(2)	13. SM(4)-5-K(2)B(2)
5. NF1(13)-4-L(3)A(3) > .1	14. SM(4)-5-E(2)I(2)
6. NF1(9)-4-L(3)A(3)	15. SM(4)-5-E(3)D(3) > .1
7. NF2(15)-3-L(2)A(3)	16. SM(3)-5-E(3)D(3) > .1
8. NF2(15)-3-L(3)A(3) > .1	17. SM(4)-3-E(2)D(2)
9. NF2(10)-3-L(3)A(3) > .1	18. FS-3-FU
	19. GS-4-CA

---

cases. All these role structures are based on two generators, which with two exceptions have the quality of a positive versus negative affect pair, evidenced both by face definitions and by their inclusion orders. The test-case exceptions are #14, SM(4)-5-E(2)I(2), with Esteem and Influence as the types of ties, and #19, GS-4-CA, based on Griffith's scientists (see table 1, III).

Figure 12 now shows the pairwise distances. Even without scaling, it is already apparent that Sampson and the two Newcomb years are quite close to one another, while the Bank Wiring Room cases and the Firth-Sterling case are outliers. Note also that the values of  $\delta$  are quite unevenly spread over the  $[0, 2]$  range: there are 35 entries where  $\delta = 2$ , and very few with  $\delta < 0.1$ .

Using the figure 12 matrix as input, one obtains the two-dimensional nonmetric scaling shown in figure 13. The stress value  $S = 12.8\%$  is well within the acceptable range for two dimensions and 19 points (Arabie and Boorman 1973). More important, it is clear that the scaling is coherently discriminating the main data cases, which are shown delineated by clusters superimposed on the scaling solution. The clusters shown may be further supported by an independent hierarchical clustering of the figure 12 lower halfmatrix. Using the HICLUS algorithms (diameter methods and connectedness method) described by Johnson (1967), one obtains clusters which discriminate Newcomb year 2, Firth-Sterling, the Bank Wiring Room, and (#4, #6) of Newcomb year 1. This is in perfect agreement with the clustering by data cases, with the main exception that #5 (a Newcomb year 2 case) is located with the Sampson cluster.

Note that the Sampson LA5 case (#10 in fig. 13) is an outlier to the main Sampson cluster, as is to be expected when one compares the LA blockmodel images with other Sampson images. It is possible that this outlying position may be a reflection of the unwillingness of Sampson's monks to reveal their preferences on social relationships as blatant as Liking Most and Liking Least. Note that the EI5 case (#14 in fig. 13)



	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
1	X																		
2	.02	X																	
3	.05	.03	X																
4	1.4	.98	.75	X															
5	1.5	1.2	.69	.39	X														
6	1.2	.68	.76	.30	.48	X													
7	2.0	2.0	2.0	2.0	.76	2.0	X												
8	2.0	2.0	2.0	2.0	.76	2.0	.67	X											
9	2.0	2.0	2.0	2.0	.93	2.0	.17	.17	X										
10	1.6	1.3	.68	.33	.48	.62	2.0	2.0	2.0	X									
11	1.6	1.3	.93	.61	.25	.65	.35	.27	.20	.49	X								
12	1.7	1.4	.78	.42	.29	.68	.38	1.0	.55	.54	.10	X							
13	1.5	1.2	.94	.78	.48	.48	.24	.24	.14	.75	.09	.29	X						
14	1.6	1.3	1.1	.88	.42	.58	.83	.83	1.0	.88	.66	.85	.59	X					
15	1.6	1.3	.88	.33	.20	.62	.98	.29	.46	.44	.06	.11	.18	.77	X				
16	1.6	1.3	.83	.28	.21	.58	.28	.83	.44	.39	.27	.28	.28	.56	.33	X			
17	1.4	1.4	1.5	1.2	.29	.93	.20	.93	.37	1.4	.18	.20	.29	.66	.37	.29	X		
18	2.0	2.0	2.0	2.0	2.0	2.0	2.0	2.0	2.0	2.0	2.0	2.0	2.0	1.3	2.0	2.0	2.0	X	
19	1.5	1.2	.94	.78	.95	.48	2.0	.76	.76	.75	.33	.83	.19	1.1	.30	1.1	1.3	.96	X

FIG. 12.—Lower halfmatrix of distances between role tables identified in table 2

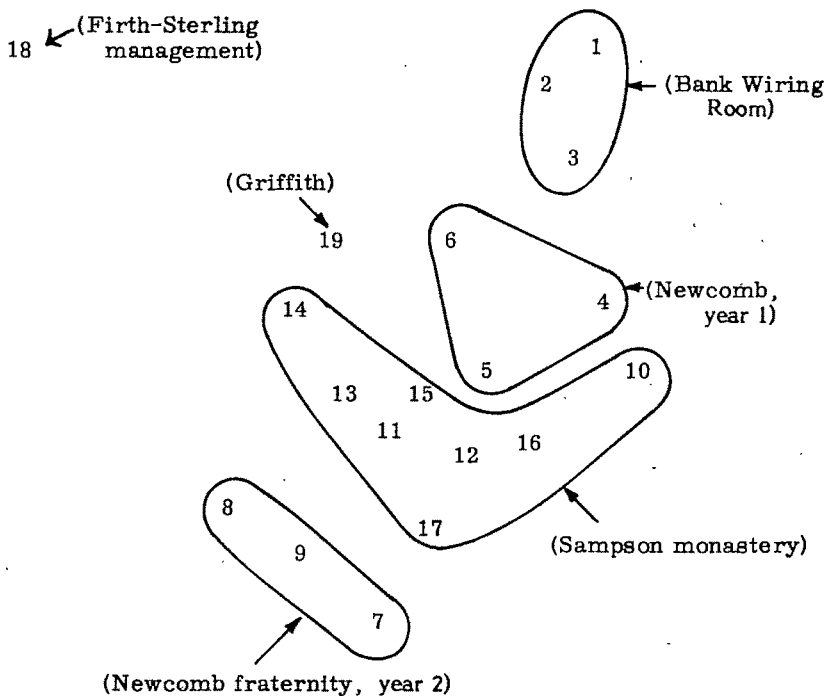


FIG. 13.—Multidimensional scaling of 19 role tables from different data sets, identified in table 2. MDSCAL-5 algorithm applied to lower halfmatrix of dissimilarities (fig. 12) based on JNTHOM algorithm. Two dimensions, Euclidean metric, stress (formula 1) = 12.8% (best of 20 random initial configurations; for theoretical background, see Arabie [1973]).

is the other Sampson outlier, as is consistent with the fact that *E* and *I* are both naturally interpreted as coding positive sentiment. Note that *EI* is situated on a ray leading from the center of the plot and directed toward the Firth-Sterling case. The three-dimensional solution (not shown) places *EI* somewhat farther from the main cluster, and correspondingly closer to Firth-Sterling.

Finally, note that the Griffith Most Contact and Asymmetric Unawareness semigroup is also placed in the same quadrant as *EI*5 and Firth-Sterling. This is internal grounds for arguing that this Griffith case is not picking up any true negative sentiment relation.

Figure 14 shows a second population of 20 role structures identified in table 3, now consisting entirely of positive versus negative generator pairs. The scaling shown in figure 14 is obtained by applying the MDSCAL-5 algorithm in the same manner as figure 13. This second population is constituted to focus more closely on alternate codings of the Sampson monastery and the two Newcomb years. In particular, the *ED*5 images

TABLE 3  
IDENTIFICATIONS OF ROLE TABLES IN FIGURE 14

1. SM(4)-5-E(2)D(2)	10. SM(4)-3-E(3)D(3) > .1
2. SM(4)-5-E(3)D(3)	11. SM(4)-3-K(2)B(2)
3. SM(4)-5-E(3)D(3) > .1	12. NF1(13)-4-L(2)A(2)
4. SM(4)-5-E(3)D(3) > .2	13. NF1(13)-4-L(2)A(2) > .1
5. SM(4)-5-E(3)D(3) > .5	14. NF1(13)-4-L(3)A(3) > .1
6. SM(4)-5-E(3)D(3) > 1.0	15. NF1(13)-4-L(3)A(3) > .2
7. SM(3)-5-E(3)D(3) > .1	16. NF1(9)-4-L(3)A(3) > .1
8. SM(4)-5-I(3)N(3) > .1	17. NF2(15)-3-L(2)A(2)
9. *SM(4)-5-SUMPOS(3)SUMNEG(3) > .4	18. NF2(15)-3-L(3)A(3) > .1
	19. NF2(10)-3-L(3)A(3) > .1
	20. BW-2-LA

\* Computed by adding the weighted block densities of the four positive and four negative images, and using a cutoff density corresponding to .1 on individual generator pairs.

are now introduced with *five* distinct cutoffs ( $\alpha = 0, .1, .2, .5, 1$ ); see the example in table 1 for the density matrices, from which the generator images for these cutoffs may be coded. Remarkably, the ED5 semigroups remain tightly clustered in the center of figure 14 until  $\alpha = 1$ , when the corresponding point (#6) moves out to the northeast. By the present measure of distance, it therefore appears that the structure of the semigroups is quite insensitive to varying cutoffs; the insensitivity is certainly not apparent from simply considering the images.

Because output of the MDSAL algorithm has no preferred orientation, the scaling solution in figure 14 has been rotated to approximately the same orientation as figure 13 (using the CONGRU algorithm of D. C. Olivier). The agreement between the two figures is close, with each of the four principal case studies occupying similar relative positions in each solution. The clusters shown in figure 14 also can be largely replicated by a HICLUS analysis applied to the lower halfmatrix input (not shown). Once again, the chief difficulty consists in discriminating some of the Newcomb year 1 cases from the Sampson cases, as is also apparent from the scaling. Note also that cases #11 and #17 give identical blockmodels, though based on different case studies. This is of course reflected in the scaling, where #11 and #17 correspond to the same point in the solution.

*Outcasts and leading crowds.*—Return to the figure 13 scaling. There is a clear linear arrangement of the four main case studies: Bank Wiring Room :: Newcomb year 1 :: Sampson :: Newcomb year 2, and this ordering is replicated consistently in figure 14 as well. It is in fact almost possible to separate each of these four principal cases in the linear Minkowski sense, that is, by means of a separating hyperplane (a line in two dimensions); this is not possible only because of certain extreme cases, such as #6 in figure 14 (ED5 with the aberrant cutoff value  $\alpha = 1$ ).

It is particularly interesting that the Sampson cluster clearly separates

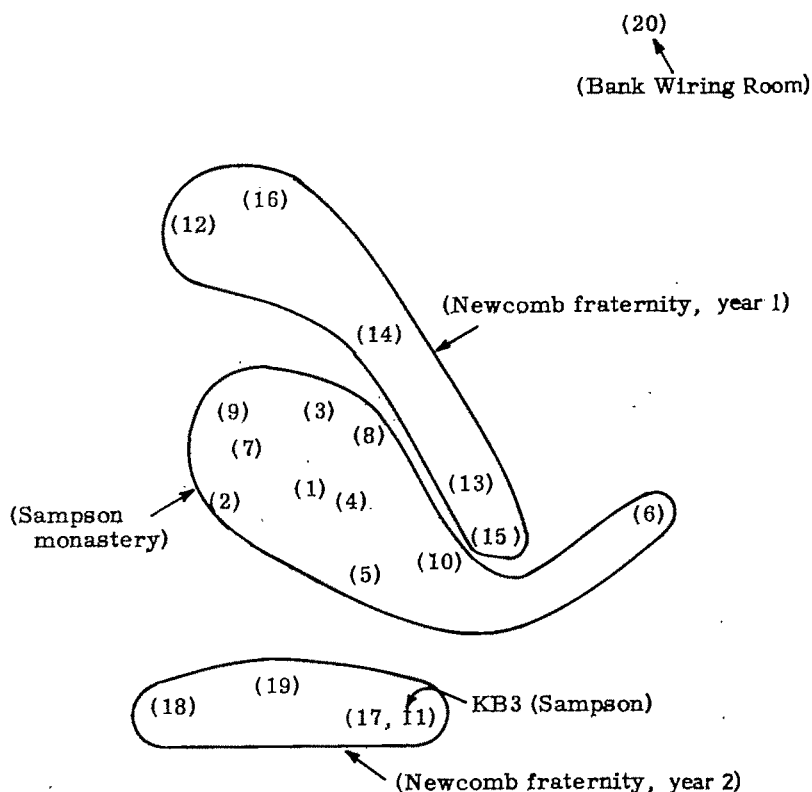


FIG. 14.—Closer view of selected Sampson and Newcomb cases, identified in table 3. Cases 11 and 17 generate the same role table. Case 6 was generated using cutoff = 1, far higher than the values used elsewhere, and one would accordingly expect aberrant results. Multidimensional scaling in two dimensions, Euclidean metric, stress (formula 1) = 13.7%; rotated to approximately the same orientation as fig. 13.

the two Newcomb years. Since Part I does not describe the Newcomb data for his first experimental fraternity (year 1), it is worth briefly introducing a blockmodel. As in year 2, the system reaches approximate equilibrium after the first few weeks (see also figs. 16 and 17 below). A basic blockmodel description can therefore be based on any of the later weeks with little change in result; we chose week 13. With the four blocks produced by CONCOR, the densities are (weighted top three choices):

	0.70	0.87	0.08	0.05		0.30	0.13	0.48	0.50
$L:$	0.33	1.3	0.07	0	$A:$	0.53	0	0.27	0.50
	0.04	0.20	0.96	0.10		0.84	0	0.08	0.35
	0.10	0.08	0.50	0.69		1.0	0.25	0	0.06

This structure can be interpreted as two cliques ( $I + II$  versus  $III + IV$ ), each divided into a core group of leaders ( $II$  or  $III$ ) and a hangers-on

group (*I* or *IV*). Moreover, as in the Bank Wiring present internal evidence for concluding that one or the other is in fact the dominant one. Observe, for instance, that the subordinate clique (*IV*) direct sharp antagonism on of the top dogs of the dominant clique (*I*); while the dominant clique (*I*) direct relatively far less attack on of the subordinate clique (*III, IV*) (cf. Breiger [1970: 104]; Stinchcombe [1968] discussing the structure).

This overall blockmodel structure is generally quite different from the Bank Wiring blockmodel on the same number of blocks, because the data are sociometric and not observer rated. The antagonism is not quite so clear cut (e.g., in the Bank Wiring case the antagonism is either within or between core cliques; in the Newcomb case, for example, there is some antagonism directed from the dominant to the subordinate core, although the density is low). On the other hand, the Newcomb year 1 pattern does have noticeable respects from the basic three-block model of year 2. Thus the year 1 hangers-on scapegoat the dominant clique does the bottom block in the year 2 model, as evident in both Like and Antagonism images.

Looking directly at the blockmodels, it makes sense to consider role structures to be intermediate between the Newcomb and Sampson patterns, identifiable in all generator pairs, the main cliques (the Young Turks and the Loyal Opposites), and the familiar internal organization of leaders and hangers-on. Also a peripheral group, the Outcasts, whose lack of positive sentiment from the top blocks is analogous to that of the Newcomb year 2, but whose position in other respects is closer to the hangers-on in Newcomb year 1.

At the same time, certain regularities in the figures remain anomalous when evaluated solely at the blockmodel level. For example, does the scaling place Sampson so clearly in the Newcomb cases when a strong argument can also be made that the outcasts are not analogous to any Newcomb position? The positive sentiment directed outside the block, though the Sampson outcasts is far lower than for the Newcomb outcasts, and also for the Newcomb year 1 hangers-on. The scaling locate all three Bank Wiring cases in figures 1 and 2 in the same relation to Newcomb year 1, even though they represent quite different aspects of the group's structure than the Newcomb cliques (namely, a three-tiered stratification system operating across cliques)?

These anomalies bear on one main point: there

purely algebraic structure on which figures 13 and 14 are based, and this coherence is reflected only to a limited extent in similarities at the block-model level. Even the most elementary comparisons at the blockmodel level run into difficulties when alternative cases are most naturally described by different numbers of blocks (e.g., four for Newcomb year 1 but three for Newcomb year 2). One may hope that it may become possible to speak with confidence of "the" role structure found in a particular case study, even though thus far we have established no formal criteria for obtaining such a unique description. To carry the analysis one step further, it is time to look inside role tables on a purely algebraic level and to seek a nucleus of common role structure through selected homomorphic images.

### Role Interlock

Our results will be based on role tables constructed from pairs of generators. For pairs interpretable as describing positive-negative affect, figures 13 and 14 have suggested that three of the case studies—the Firth-Sterling management, the Bank Wiring Room, and Griffith's scientists—are quite unlike the others; this divergence is reinforced by the inclusion orders (thus see fig. 11). These three populations are much more constrained than the others by formal organization and outside pressures (see Part I; and see Breiger [1976] for a further report on the Griffith scientists case). In such a population, we suspect that any role interlock which includes negative-affect generators will be idiosyncratic, shaped by particular constraints not accounted for in the formalism. Among the other three populations, however, it is possible to identify role interlocks which have common features.

Where neither generator is negative, we will identify role interlock features common to all the populations with such data (only Newcomb's fraternities are excluded, since here there is only a single generator connoting positive affect). Apparently, it is chiefly role interlock involving *negative* affect which is sensitive to bureaucratic and other outside pressures.

*Positive-negative pairs of generators.*—Sampson's case is central in figures 13 and 14, and we build from it. We begin with the strict zeroblock cutoff version on three blocks which was emphasized in Part I. The earlier analysis of figure 10, as well as the clustering in figures 13 and 14, suggests seeing whether his four different pairs of generators exhibit similar role interblock.

The Influence pair and the Esteem pair, *IN3* and *ED3*, have the same role table, which is shown in figure 6*b*. Note again that these two pairs of generators (also shown in fig. 6*b*) are distinct as blockmodels (de-

pending on whether it is the Opposition or the Turks who are placed on top of the hierarchy); this difference was important in our concrete account of the monastery in Part I, yet we now find it compatible with identical role interlock. For the Kudos pair *KB3* (#5 and #6 in fig. 2) the role table proves to be just the target table  $T_1$ , discussed in detail at the end of the section on theory.

The joint reduction of *IN3-ED3-KB3*, the coarser form of interlock common to them, is itself precisely  $T_1$ ; *thus  $T_1$  is a homomorphism of the IN3-ED3 table, as well as being the KB3 table.* This finding of underlying  $T_1$  structure is now confirmed and extended by three series of tests. First, the joint reduction of the role table for each pair of generators is computed for each of the seven other target tables. Figure 15 reports whether or not these others are also homomorphisms, and in addition gives the distances through the joint reduction.<sup>25</sup> Only the *LA* case gives problems, and this must happen since the original role structure on *L* and *A* is itself degenerate.

Second, the same procedure is carried out for the refined models with the three blocks split into five. The results, also given in figure 15, again show that  $T_1$  is the form of role interlock common to these generator pairs. Again, only *LA* is the outlier. Running reductions of the five-block models in this way is not redundant with the three-block reductions just reported, since a refinement of a blockmodel need not yield a semigroup which maps homomorphically onto the three-block blockmodel's role structure.

Third, we test reliability by seeing whether  $T_1$  remains the common form of role interlock for an alternate coding of the generators. Apply a zeroblock cutoff of 0.1 to the weighted top three choices. The resulting generators for *KB3*, shown in figure 1, again identically yield  $T_1$  as their role table. For *IN3* and *ED3* the generators, and thus the role table, are the same as with the strict zeroblock cutoff, hence must also reduce to  $T_1$  *a fortiori*. (As for the refined models on five blocks, figure 7 has already shown that the role table for the Influence pair reduces to  $T_1$ ; the same is true for *KB5* though not for *ED5*.) Reliability was tested further, using still higher values of 0.15 and 0.2 for the cutoff density, with much the same results for these three generator pairs; and, for the first time, the Like-Antagonism pair also has a role table reducing to  $T_1$ .

Because of relative position in the scalings of figures 13 and 14, Newcomb's two fraternities should contrast in opposite ways with the monastery. The  $T_1$  role interlock common to all of Sampson's pairs of generators can be compared with that for the one generator pair we have

<sup>25</sup> Where  $T_i$  is not a homomorphic reduction of the original semigroup, the joint reduction in each case is one of the trivial  $2 \times 2$  tables shown in the discussion of target tables at the end of the section on theory.

	$T_1$	$T_2$	$T_3$	$T_4$	$T_5$	$T_6$	$T_7$	$T_8$
	1 2 3	1 3 3	1 2 3	1 2 3	1 3 3	1 1 1	1 3 3	1 3 3
	3 3 3	2 3 3	2 3 3	3 2 3	2 2 2	3 3 3	1 3 3	3 2 3
	3 3 3	3 3 3	3 3 3	3 2 3	3 3 3	3 3 3	1 3 3	3 3 3
Size								
Role Table								
SM(4)-3-E(2)D(2)	5	Y(.35)	Y(.20)	N(.93)	N(.93)	N(.93)	N(.93)	N(.96)
SM(4)-5-E(2)D(2)	11	Y(.20)	Y(.29)	N(.90)	Y(.27)	N(.90)	Y(.44)	N(.90)
SM(4)-3-I(2)N(2)	5	Y(.38)	Y(.20)	N(.93)	N(.93)	N(.93)	N(.93)	N(.93)
SM(4)-5-I(2)N(2)	12	Y(.20)	Y(.38)	N(1.0)	N(1.0)	N(1.2)	N(1.2)	N(1.0)
SM(4)-3-K(2)B(2)	3	ISO(0)	N(.67)	N(.67)	N(.66)	N(2.0)	N(2.0)	N(.67)
SM(4)-5-K(2)B(2)	7	Y(.24)	N(.86)	N(.86)	Y(.24)	N(1.1)	Y(.33)	N(.86)
SM(4)-3-L(2)A(2)	3	----- (degenerate) -----						
SM(4)-5-L(2)A(2)	10	N(2.0)	N(2.0)	N(2.0)	N(2.0)	N(1.1)	N(1.1)	N(2.0)

FIG. 15.—Alternative homomorphisms of role tables from the Sampson data. Distance  $\delta$  to the target tables are shown parenthetically.  $Y$  = yes (is a homomorphic reduction),  $N$  = no (is not),  $ISO$  = isomorphic.



identified in Newcomb's data, Like and Antagonism, using a cutoff density of 0.2. In addition, longitudinal stability of the results for each Newcomb fraternity can be shown. (This is not really possible for the monastery: Newcomb collected data week by week, whereas Sampson has only recall data for earlier periods; see Part I.) The results are shown in figures 16 (year 2) and 17 (year 1) in a format parallel to that of figure 15.

From an early week, the role structure for the second fraternity either is or reduces to  $T_4$ , but not to  $T_1$  or the other six. The distinctive equation in  $T_1$ ,  $LA = A$ , is also present in  $T_4$ , but the role interlock is different because in addition the second generator is transitive,  $A^2 = A$ . This latter equation reflects the scapegoating of a bottom block found here but not in the monastery (e.g., contrast the Antagonism matrix in the text of the Nomenclature section with the Disesteem matrix in table 1).

From an early week, the role structure for the *first* fraternity reduces to  $T_1$  and to  $T_2$ , but never to  $T_3$ . A  $T_4$  reduction is not obtained until very late in the sequence, and it is not possible to know whether its presence is robust. Recall that  $T_3$  is the table for Davis's weaker form of balance theory, in which the commutativity of  $A$  with  $L$  is retained. Thus, the two key equations of  $T_3$  cannot be obtained together, but can be obtained separately in reductions of the role table to  $T_1$  and  $T_2$ . Unlike the second fraternity, the first has role interlock akin to balance theory, but a form of balance even weaker than Davis's. This conclusion is consistent with the earlier interpretation in the section on outcasts and leading crowds. The above conclusions for each fraternity are essentially unchanged if a zeroblock cutoff density of 0.1 is used instead of 0.2. The main effect of thus lowering  $\alpha$  is that *none* of  $T_1$ - $T_8$  becomes a homomorphic image until the late weeks—role interlock is not characterized as rapidly.

*Other pairs of generators.*—The first lines in figure 18 already suggest the main conclusion to be drawn: for a range of populations, including those with formal organizations, role interlock between a generator for Similar Policy (or some similar type of tie) and a generator for Liking reduces to the predominance of one's direct ties, whose quality subsequently carries over to indirect ties as well. This is the First Letter interlock discussed earlier under theory, and there exemplified by target tables  $T_5$  and  $T_8$  (see figure 18).

In addition to Friendship and Similar Policy for the Firth-Sterling management, we also report results for the *LH* pair of generators in the Bank Wiring Room and for the *LK* pair of generators in the monastery (*H* signifies "Helping" in the Bank Wiring data and is the only major tie in these data which is not symmetric; see Part I). Figure 19 shows the joint reductions of pairs of these role tables. To show reliability, three

Week	Size	T <sub>1</sub>	T <sub>2</sub>	T <sub>3</sub>	T <sub>4</sub>	T <sub>5</sub>	T <sub>6</sub>	T <sub>7</sub>	T <sub>8</sub>
0	3	1 2 3 3 3 3 3 3 3	1 3 3 2 3 3 3 3 3	1 2 3 2 3 3 3 3 3	1 2 3 3 2 3 3 2 3	1 3 3 2 2 2 3 3 3	1 1 1 3 3 3 3 3 3	1 3 3 1 3 3 1 3 3	1 3 3 3 2 3 3 3 3
1	3	N	N	N	N	N	N	N	N
2	4	N	N	N	N	N	N	N	N
3	5	Y	N	N	Y	N	N	Y	N
4	5	Y	N	N	Y	N	N	Y	N
5	6	Y	N	N	Y	N	N	Y	N
6	3	N	N	N	Y	N	N	N	N
7	6	Y	N	N	Y	N	N	Y	N
8	3	N	N	N	Y	N	N	N	N
10	3	N	N	N	Y	N	N	N	N
11	3	N	N	N	Y	N	N	N	N
12	5	N	N	N	Y	N	N	Y	N
13	5	N	N	N	Y	N	N	Y	N
14	3	N	N	N	Y	N	N	N	N
15	3	N	N	N	Y	N	N	N	N

FIG. 16.—Alternative homomorphisms across Newcomb weeks, year 2. Data are  $NF2(i)-3-L(3)A(3) > 2$  for  $i=0, 1, \dots, 15$  (week 9 missing); Y = yes, N = no.

<u>Week</u>	<u>Size</u>	<u>T<sub>1</sub></u>	<u>T<sub>2</sub></u>	<u>T<sub>3</sub></u>	<u>T<sub>4</sub></u>	<u>T<sub>5</sub></u>	<u>T<sub>6</sub></u>	<u>T<sub>7</sub></u>	<u>T<sub>8</sub></u>
1	5	1 2 3 3 3 3 3 3 3	1 3 3 2 3 3 3 3 3	1 2 3 2 3 3 3 3 3	1 2 3 3 2 3 3 2 3	1 3 3 2 2 2 3 3 3	1 1 1 3 3 3 3 3 3	1 3 3 1 3 3 1 3 3	1 3 3 3 2 3 3 3 3
2	5	N	N	N	N	N	N	N	N
5	7	N	N	N	N	N	N	Y	N
6	7	N	N	N	N	N	N	Y	N
7	7	Y	Y	N	N	N	N	N	N
8	9	Y	Y	N	N	N	N	N	N
9	6	Y	Y	N	N	N	N	N	N
11	9	Y	Y	N	N	N	N	N	N
12	5	Y	Y	N	N	N	N	N	N
13	6	Y	Y	N	N	N	N	N	N
14	10	Y	Y	N	Y	N	N	Y	N
15	6	Y	Y	N	Y	N	N	Y	N

FIG. 17.—Alternative homomorphisms across Newcomb weeks, year 1. Data are  $NF_1(t) - L(3)A(3) > .2$  for indicated weeks

Role Table	Size	T <sub>1</sub>	T <sub>2</sub>	T <sub>3</sub>	T <sub>4</sub>	T <sub>5</sub>	T <sub>6</sub>	T <sub>7</sub>	T <sub>8</sub>
FS-3-FS	6	1 2 3 3 3 3 3 3 3	1 3 3 2 3 3 3 3 3	1 2 3 2 3 3 3 3 3	1 2 3 3 2 3 3 2 3	1 3 3 2 2 2 3 3 3	1 1 1 3 3 3 3 3 3	1 3 3 1 3 3 1 3 3	1 3 3 3 2 3 3 3 3
FS-3-SF	6	N	N	N	N	N	Y	N	N
		N	N	N	N	Y	Y	N	N
SM(4)-5-E(3)I(3) > .1	9	N	Y	N	N	N	N	N	Y
SM(4)-5-I(3)E(3) > .1	9	N	Y	N	N	N	N	N	Y
SM(4)-5-E(3)I(3) > .2	9	N	Y	Y	N	N	N	N	Y

FIG. 18.—Alternative homomorphisms for role tables with neither generator negative; Y = yes, N = no

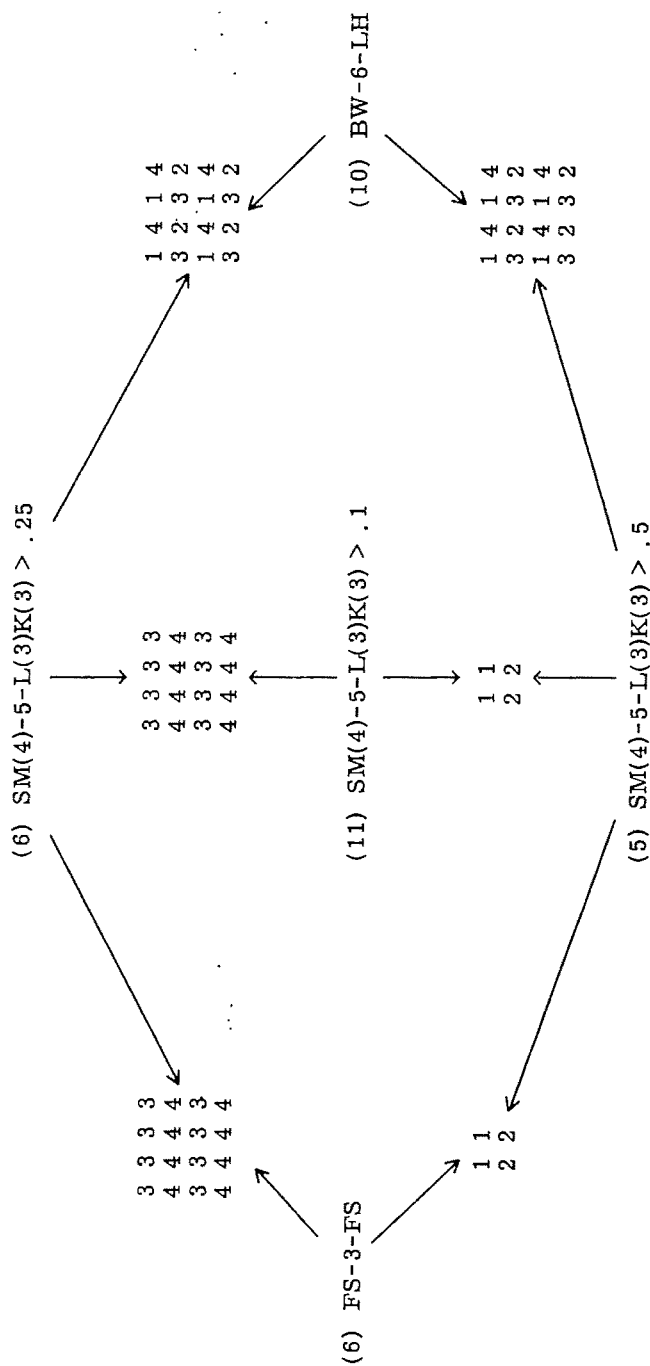


FIG. 19.—Joint reductions of role tables from pairs of positive generators. The size of each role table is shown in parentheses before its identification. Lines lead from each member of a pair to their joint reduction table.

different codings for the *LK* pair are used. It is clear that the First Letter table is the predominant feature of role interlock in the joint reductions.

Return to figure 18. The first line shows that the FS pair for Firth-Sterling reduces *only* to a First Letter interlock, to  $T_6$ . In the second line, order of generators is reversed: the FS pair is reversed to become the SF pair, which has a reduction to  $T_8$  as well as to  $T_6$ , both First Letter interlocks. The role tables for SF and for FS are therefore *not* the same (see the discussion of Definition 10 in the methods section) and need not have the same homomorphisms. (With positive-negative pairs of generators, generator reversal customarily leads to entirely different homomorphisms: indeed, the distance  $\delta$  between such a pair and its reverse through their joint reduction often assumes the maximum value of 2.0.)

First Letter interlock need not occur for a pair of generators even though neither represents negative affect. The remaining lines of figure 18 analyze an example. The Esteem versus Influence pair is interesting in its own right: recall the earlier discussion of *ED* versus *IN* role tables above. We see that regardless of whether  $\alpha = .1$  or  $.2$  the *EI* role table reduces to the  $T_8$  form of transitive generators with no interlock between them.<sup>26</sup> And the reversed pair *IE* yields a similar pattern of reductions. But all these cases reduce also to  $T_2$ , which is the natural dual to  $T_1$ .

*Further work.*—As more case studies become available, it should be possible to correlate the main features of role interlock with further properties of concrete populations. We have made initial analyses of two further sets of data of high quality.<sup>27</sup> Given such correlations, it will be worthwhile to try to verify more detailed descriptions of role interlock for specific cases. And with more cases it should be possible to find comparable sets of three and more generators: the existing analytic techniques and computer programs already described are directly applicable, but further development of target tables will be necessary.

## CONCLUSION

What makes a society human? Speaking as a sociologist, one is tempted to seek the answer in the existence of roles. The problem is not so simple: whatever the distinctive features of the invertebrate societies (Haskins 1939; Grassé 1959; Wilson 1971), it is clear that at least the higher primates have well-developed complexes of stable social relationships which seem to behave much like human roles, at least to primatologists (Kummer 1967; Blaffer Hrdy 1976). A somewhat more sophisticated hypothesis is that the characteristic features of human society lie in the

<sup>26</sup> The full role table for the coarser three-block version is  $T_8$ .

<sup>27</sup> Kindly supplied by A. P. M. Coxon and by F. Lorrain.

peculiarly intricate complexes of interlocking roles which only men can sustain. The present work tries to take seriously what Durkheim saw but most of his followers did not: that the organic solidarity of a social system rests not on the cognition of men but rather on the interlock and interaction of objectively definable social relationships.

There is a moral cast to the study of roles (Emmet 1966). We see support even in the present limited study for a stance we think important: Humans can and do build complex and subtle social structure without the need for a directing hand or an acknowledged plan (Geertz 1965). Official structures can impede solutions to structural problems (Burns and Stalker 1955), not least by the drain of energy involved in reconciling reality with facade. One thinks of law, and also of formal organizations (Boorman 1975*b*).

We see at present no intelligent way to develop role interlock for open networks extending through large populations, even though this topic is much closer to the heart of sociology than is small-group structure (Milgram 1967; White 1970; Granovetter 1976). From an analytic standpoint, the present machinery is suggestive of social castes and classes and their interrelations on a macroscopic level of large-scale social structure (Mayer 1960; Boyd 1969*a*). Various classical hypotheses lend themselves to possible blockmodel and algebraic reformulations: can the classical Aristotelian theory of revolution (as the result of separation between political and economic elites) be made operational through tracing the emergence of certain kinds of zeroblocks? The next analytic task is to provide ways to probe how role structures of the kind we have identified actually come into being, through the continuing accommodations and manipulations of all individuals acting simultaneously (Leach 1954). Some first steps have recently been taken in this direction (Lorrain 1971; White 1973; Spence 1974; Boorman 1975*a*).

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# Sex Differences and Psychiatric Disorders<sup>1</sup>

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Since the turn of the century, over 80 studies designed to count both untreated and treated cases of psychiatric disorder have been carried out in different parts of the world; in the majority of these, data were reported for males and females separately. The procedures and results of these epidemiological studies of "true" prevalence are analyzed with respect to several questions: Are total rates over all types of functional disorder higher among males or among females? Which types of disorder are more prevalent among males and which among females? How do sex differences in overall rates and in different types of disorder vary over time? What are the implications of the findings for theory about sex roles and psychiatric disorder?

In their 1973 study of sex roles and mental illness, Gove and Tudor discovered much in the writings of contemporary social scientists to suggest that "women find their position in society to be more frustrating and less rewarding than do men and that this may be a relatively recent development" (p. 816). Accordingly, they "postulate that, because of the difficulties associated with the feminine role in modern Western societies, more women than men become mentally ill" (p. 816).

To investigate this hypothesis, Gove and Tudor focused on research conducted since World War II, on the grounds that the war marked a high point of change in the role of women, with significant portions of married women entering the work force for the first time. They also restricted their coverage to North America and Europe where, they assumed, economic and technological conditions related to industrialization have promoted the posited change in the role of women. They concluded that the epidemiological evidence demonstrates that women do in fact have higher rates of "mental illness" than do men and that this is, indeed, a relatively recent development.

<sup>1</sup> This is a revised and shortened version of a paper presented at the 8th World Congress of Sociology, Toronto, Ontario, August 19-24, 1974. The analysis was supported in part by research grant MH 10328 and by Research Scientist Award K5 MH 14663 from the National Institute of Mental Health, U.S. Public Health Service. We would like to express appreciation to Frederick S. Mendelsohn for his very useful criticism and to Catherine K. Riessman for her help with the results for manic-depressive psychosis.

However, most of the evidence on which they base their conclusion is inadequate for the problem: It does not deal with trends over time; it omits data on the personality disorders; it includes treatment statistics that are limited and can often be misleading (e.g., Dohrenwend and Dohrenwend 1969); and, where it consists of a presentation of results from some community studies that are not restricted to treated rates (Gove and Tudor 1973, p. 819), the findings are refracted through Gove and Tudor's highly idiosyncratic definition of "mental illness" (p. 812). In this definition, such disparate types of diagnosed psychiatric disorder as psychoses and neuroses are lumped together and tallied along with objective measures of milder signs and symptoms of personal distress whose relation to clinical psychiatric disorder is far from evident, especially when, as in some of the studies considered by Gove and Tudor, the test items are not calibrated against psychiatric evaluations or patient criterion groups (e.g., Dohrenwend 1973).

The most important data that Gove and Tudor bring to bear on the issue, and the only data on trends over time, consist of overall rates of functional disorder (not excluding the personality disorders) that we summarized in our 1969 review of epidemiological studies of "true" prevalence conducted since the turn of the century (Dohrenwend and Dohrenwend 1969). All of these studies identify cases of psychiatric disorder in general populations by one of the following procedures or some combination of them: evaluations by psychiatrists of data from key informants and agency records; evaluations by psychiatrists of data from direct interviews with community residents; and, especially in more recent studies, scores on screening test questions that have been calibrated against psychiatric evaluations or against criterion groups of psychiatric patients (Dohrenwend and Dohrenwend 1969, 1974b).

Perceptively, Gove and Tudor divided the North American and European studies that we included in our 1969 review into, roughly, pre-World War II and post-World War II groups (because of the gap in publication between 1943 and 1950, the division is actually on the basis of whether they were published before 1950 or in 1950 or later). They found that overall rates were consistently higher for men in the earlier studies and for women in the later ones (Gove and Tudor 1973, p. 828). Other things being equal, it would seem that these results provide the strongest support that Gove and Tudor have for their hypothesis. Other things, however, are not equal.

#### ANALYSIS OF EPIDEMIOLOGICAL STUDIES OF "TRUE" PREVALENCE CONDUCTED IN THE UNITED STATES AND EUROPE

Since our 1969 review, we have updated our coverage of the epidemiological literature. Altogether, we have found over 80 studies of the "true"

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prevalence of psychiatric disorder in community populations. The majority of them provide at least some relevant data on sex differences, and 33 of those that do were conducted in North America and Europe.<sup>2</sup>

Table 1 shows that, as Gove and Tudor found on the basis of data

TABLE 1  
NUMBER OF EUROPEAN AND NORTH AMERICAN STUDIES REPORTING HIGHER RATES OF  
PSYCHIATRIC DISORDER FOR MEN OR FOR WOMEN ACCORDING TO PUBLICATION  
PRIOR TO 1950 OR IN 1950 OR LATER

DATE OF PUBLICATION AND TYPE OF PSYCHOPATHOLOGY	STUDIES IN WHICH RATE IS HIGHER FOR (N)	
	Males	Females
Before 1950:		
All types .....	7	2
Psychosis .....	3	3
Neurosis .....	1	2
Personality disorder .....	3	0
1950 or later:		
All types .....	2	22
Psychosis .....	5	10
Neurosis .....	0	15
Personality disorder .....	11	3

from our 1969 review, overall rates in the North American and European studies are consistently higher for men in the pre-1950 investigations and for women in those published in 1950 or later. Accepted at face value, this apparent change provides striking support for Gove and Tudor's hypothesis. Note, however, that by contrast with the overall rates trends on sex differences for the major subclassifications of functional psychiatric disorder do not show such reversals between the earlier and the later studies. Rates of personality disorder in particular are consistently higher for men in both the earlier and the later studies, and there is no consistent evidence of higher male rates of either neurosis or psychosis in the pre-1950 studies. These results for the various subtypes of disorder are difficult to explain in terms of Gove and Tudor's theory of changing sex roles.

Table 2 shows that something else has happened to reported rates of

<sup>2</sup> However, some of these studies present findings based on ratings or screening scores that do not distinguish among diagnostic types and can be tallied with other studies only for total or overall rates of functional psychiatric disorder. Moreover, not all of the studies that give diagnostic breakdowns do so for the same subtypes of disorder; hence, the more detail we seek beyond overall rates, the more studies we are forced to omit from our analysis. Because of space limitations, we present summary tables only. Interested readers can obtain more detailed tables and bibliography by writing to us at the Social Psychiatry Research Unit, Columbia University, 100 Haven Avenue, New York, New York 10032.

TABLE 2

MEDIANS AND RANGES OF PERCENTAGES OF PSYCHIATRIC DISORDER REPORTED FOR EUROPEAN AND NORTH AMERICAN STUDIES BEFORE 1950 AND IN 1950 OR LATER

	BEFORE 1950		1950 OR LATER	
	Males	Females	Males	Females
Overall:				
Median .....	1.89	1.91	15.4	23.5
Range .....	1.1-9.0	1.0-6.0	2.0-51.8	2.7-75.2
Studies ( <i>N</i> ) .....		9		22*
Psychosis:				
Median .....	0.445	0.43	.120	1.69
Range .....	0.23-0.90	0.247-0.91	0.006-4.6	0.009-56.0
Studies ( <i>N</i> ) .....		6		14†
Neurosis:				
Median .....	0.20	0.26	3.56	8.0
Range .....	0.14-0.22	0.11-0.42	0.30-44.0	0.31-64.0
Studies ( <i>N</i> ) .....		3		15
Personality disorder:				
Median .....	0.63	0.12	5.85	2.94
Range .....	0.11-1.08	0.01-0.18	0.046-18.0	0.047-11.0
Studies ( <i>N</i> ) .....		3		13†

\* Two studies reported only the relative ranking of men and women.

† One study reported only the relative ranking of men and women.

psychiatric disorders since 1950. Overall rates and rates for the major subclassifications of psychosis, neurosis, and personality disorder have all increased dramatically from the earlier to the later studies—for men and for women. Accepted at face value, these results not only would be consistent with the hypothesis that the role of women has become more stressful, but also would imply more stress in the male role since World War II. Moreover, the increase in the median of the overall rates from under 2% to over 15% for both sexes appears to be of epidemic proportions.

Amidst a plethora of possible speculations about the reasons for such an extraordinary increase, there is one firm fact. One thing that we know has increased is the breadth of our definitions of what constitutes a psychiatric case. A great impetus to this expansion was provided by the experience of psychiatrists in screening and treatment during World War II. As Raines wrote in his foreword to the 1952 *Diagnostic and Statistical Manual* of the American Psychiatric Association, which reflected that experience: "Only about 10% of the total cases fell into any of the categories ordinarily seen in public mental hospitals. Military psychiatrists, induction station psychiatrists, and Veterans Administration psychiatrists, found themselves operating within the limits of a nomenclature specifically not designed for 90% of the cases handled" (p. vi).

The expansion has been a continuing process. One can see it in the development of such concepts as "pseudoneurotic schizophrenia" (Hoch and

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Polatin 1949) and, reflecting the efforts of the mental health education movement, in a dramatic increase between the 1950s and the 1960s in the willingness of the public to broaden its own definition of what it identifies, at least attitudinally, as "mental illness" (e.g., Dohrenwend and Chin-Shong 1967). Consistent with these changes in conceptions of what constitutes a case, there has been an increase, as table 3 shows, in rates of

TABLE 3

MEDIAN AND RANGES OF OVERALL PERCENTAGES OF PSYCHIATRIC DISORDER REPORTED FOR EUROPEAN AND NORTH AMERICAN STUDIES, BY DATE OF PUBLICATION

	BEFORE 1950		1950-59		1960 OR LATER	
	Males	Females	Males	Females	Males	Females
Median .....	1.89	1.91	12.1	13.6	18.43	26.75
Range .....	1.1-9.0	1.0-6.0	2.0-22.1	2.7-26.0	4.8-51.8	2.4-75.2
Studies (N) ..	9		6		16	

psychiatric disorder reported in the true prevalence studies not only between the pre- and post-World War II periods but also during the latter period.

While the expanding concepts of what constitutes a case appear to provide an economical explanation of the general tendency for reported rates of all types of functional psychiatric disorders to increase over time for both men and women, they do not explain why the rate of increase should be so much greater for women than for men (table 2) that sex differences in overall rates are actually reversed between the earlier and the later studies (table 1). We think that the most plausible and parsimonious explanation is again methodological rather than substantive.

In the studies published prior to 1950, there was a far greater tendency than in later ones for the investigators to rely on key informants and official records to identify potential cases (Dohrenwend and Dohrenwend 1974b, p. 425). Such procedures are well suited to identifying the types of personality disorder characterized by chronic antisocial behavior and addiction to alcohol or drugs: persons, disproportionately male, who show such problems tend to leave records with police and other agents of social control or, at the least, to develop unfavorable reputations in their communities (cf. Cawte 1972; Mazer 1974).<sup>3</sup> Correspondingly, such procedures

<sup>3</sup> It can be argued, in fact, that none of the "true" prevalence studies, cross-sectional in nature, use the appropriate methods for identifying the mobile and hazard-prone individuals who show the antisocial types of personality disorder. Far better for this purpose would be prospective studies of cohorts identified from birth records or early childhood records and followed to their destinations, whether residence in the community of birth, another community, a jail or other institution, or an early grave (cf. Robins 1966). Such studies, unfortunately, are extremely rare in psychiatric epidemiology.



are less likely to identify persons, disproportionately female, whose neurotic problems are more private (cf. Cawte 1972).

By contrast with the earlier epidemiological studies of true prevalence, the investigations published in 1950 or later tended to rely for the most part on direct interviews with all respondents for data collection. Moreover, beginning in the 1960s when, as can be seen in table 3, the strong trend for women to show higher overall rates than men emerged, a growing number of the investigators have adopted screening inventories such as the ones developed by Langner (1962), Macmillan (1957), and several investigators who have used variations on the Cornell Medical Index (e.g., Rawnsley 1966) to identify cases. Typically, these inventories focus on symptoms of anxiety, depression, and physiological disturbances that are more indicative of neurosis and some aspects of manic-depressive psychosis than of personality disorders or schizophrenia. Given their content, it is not surprising that scores on such measures are generally higher for women than for men (cf. Cawte 1972; Mazer, 1974).

These contrasts in method are confounded with date of publication in the studies whose results are summarized in table 2, so that a direct test of their impact is difficult to make. There are, however, four studies concerning the United States and Europe published after 1950 that relied on key informants and records instead of direct interviews with all subjects to identify potential cases. The median of the ratio of female to male overall rates for these studies is 1.18. By contrast, the corresponding median for the seven post-1950 studies in Western communities that relied on the Langner screening items or a similar measure is 1.92. Moreover, one study reports for the same subjects both sex differences based on key informant reports and sex differences based on self reports to questions adapted from the Cornell Medical Index (CMI) (Cawte 1972). The men are found by the informants to be more disturbed (p. 59); the women report more symptoms on the CMI (pp. 74-75).

It is possible, then, to speculate that the results in tables 2 and 3 indicate that there have been increases in rates of the functional psychiatric disorders in modern Western societies since World War II and that, particularly since 1960, these increases have been greater for women than for men. It seems more plausible, however, to interpret these results as being a function of changes in concepts and methods for defining what constitutes a psychiatric case.

#### WHERE TRUTH LIES IN THE EPIDEMIOLOGICAL STUDIES OF TRUE PREVALENCE

For us, the truth of these epidemiological studies does not lie in the accuracy of their estimates of rates for this or that type of psychiatric dis-

order, much less for overall rates. Such estimates, as we have shown, vary with contrasts in the concepts and methods used at different times and in different places by the various investigators. Instead, their truth lies in the consistent relationships they report between various social variables and various types of disorder across—even in spite of—such methodological differences. We have found such relationships with social class (Dohrenwend and Dohrenwend 1969, 1974*b*) and with rural versus urban location (Dohrenwend and Dohrenwend 1974*a*).

More recently we have been able to reach certain conclusions about sex differences, which we summarize here. With time since the turn of the century specified as pre- and post-World War II, with place specified as rural and urban settings in selected United States and European communities in contrast with selected communities in the rest of the world, and with the functional psychiatric disorders defined as operationalized by the various epidemiological investigators who have worked in these communities: (1) There are no consistent sex differences in rates of functional psychoses in general (34 studies) or one of the two major subtypes, schizophrenia (26 studies), in particular; rates of the other subtype, manic-depressive psychosis, are generally higher among women (18 out of 24 studies). (2) Rates of neurosis are consistently higher for women regardless of time and place (28 out of 32 studies). (3) By contrast, rates of personality disorder are consistently higher for men regardless of time and place (22 out of 26 studies).

These results cannot easily be explained by role theories arguing that at some time and place one or the other sex is under greater stress and, hence, more prone to psychiatric disorder in general. Instead, the findings suggest that we should discard undifferentiated, unidimensional concepts of psychiatric disorder and with them false questions about whether women or men are more prone to "mental illness." In their place we would substitute an issue posed by the relatively high female rates of neurosis and manic-depressive psychosis, with their possible common denominator of depressive symptomatology, and the relatively high male rates of personality disorders with their possible common denominator of irresponsible and antisocial behavior. The important question then becomes, What is there in the endowments and experiences of men and women that pushes them in these different deviant directions?

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## Commentary and Debate

### COMMENT ON CLANCY AND GOVE

In a recent paper, "Sex Differences in Mental Illness: An Analysis of Response Bias in Self-Reports" (*AJS* 80 [July 1974]: 205-16), Clancy and Gove criticized the work of Phillips and Segal (1969) and Cooperstock (1971) for arguing "that the consistent finding that women have higher rates of mental illness than men is a product of response bias and does not reflect actual differences in rates of mental illness" (p. 205). The authors then examine three forms of response bias to determine their effect on the respondents' reports of psychiatric symptoms (utilizing the 22-item Langner scale). They find that when the three forms are controlled, the differences between the sexes do not diminish but actually increase, supporting their position that the higher rates of mental illness among women are not artifactual but are real differences.

At the risk of being redundant (see the excellent work by Dohrenwend and Dohrenwend 1974), we feel it necessary to point out the tautological basis of the authors' argument. They have chosen to examine sex differences in mental illness (and hence to argue against the theoretical position of Phillips and Segal and Cooperstock, as well as others, e.g., Mazer 1974) on the basis of their highly personal definition of mental illness. Their view of mental illness is never fully defined in the article under consideration. The only reference to their definition of it is in a footnote in which the authors state: "Gove and Tudor (1973) use a fairly stringent definition of mental illness, limiting it to functional disorders involving personal distress (e.g., neurosis) and/or mental disorganization (e.g., psychosis)" (p. 205). The article mentioned pertains only to the functional psychoses, not to "the two other major diagnostic psychiatric categories, the personality disorders and the acute and chronic brain disorders, [which] do not conform to our conception of mental illness" (Gove and Tudor 1973, p. 813). Gove and Tudor make an interesting and provocative distinction between the functional disorders, which involve personal discomfort, and those other disorders in which individuals "do not experience personal discomfort, being neither anxious nor distressed, . . . These persons are characterized by aggressive, impulsive, goal-directed behavior which is either antisocial or asocial in nature. . . . The brain disorders (the acute and chronic brain syndromes) are caused by a physical condition, either brain damage or toxins, and are not a functional disorder. Since personality and brain disorders do not conform to our conception of mental illness, . . . neither of these disorders will be treated as mental illness" (*ibid.*). The authors have, of course, effec-

tively eliminated alcoholic psychosis (presumably because of its toxic origin), as well as all of the other categories of psychosis listed by the International Classification of Disease (ICD) in which the rates for males exceed those for females, for example, personality disorders. By redefining mental illness in such a way as to exclude the psychoses in which males predominate, they are able to produce excessive rates of psychosis in women as an artifact of their method. A crude analogy might occur if one were to undertake a study of genito-urinary diseases as defined by the ICD and omitted the diseases of the male genital organs (presumably because the symptoms are different), thus assuring the reader that these diseases are all women's diseases!

Having redefined mental illness as that which produces some level of personal discomfort, they then go on to claim that all studies of sex differences in psychiatric disorders show that females exceed males. By the application of their highly personal criteria this would be so, but they cannot ascribe the same idiosyncratic definition to other researchers. The authors appear to have redefined more than mental illness: they have also redefined the content of Cooperstock's paper. They claim ". . . that Phillips and Segal and Cooperstock are correct in believing that, in Western nations, women do appear to have higher rates of mental illness" (p. 206). Cooperstock's article actually said on this point that ". . . rates of admission to psychiatric hospitals for those labelled as psychotic are about the same for men and women. . . . Numerous articles seem to indicate that women suffer more frequently from neurotic illness than do men" (1971, p. 241). This is only one example of selective misreading or misinterpretation on the part of the authors.

The authors, referring back to the earlier paper by Gove and Tudor, note that "men probably had higher rates of mental illness prior to World War II" (p. 206). In that paper the change in rates after World War II is attributed to changes in aspects of sex roles. Dohrenwend and Dohrenwend (1974) have already dealt with this issue at length in their critical analysis of Gove and Tudor's earlier work, pointing out that the methods of assessment of psychiatric morbidity changed at this time, leading to vast differences in the sex rates of psychopathology. The use of psychiatric screening devices which utilize symptom checklists relying on self-reports came about after the war (in fact as a result of the war-time efforts to screen young men for military service). In addition to changes in sex roles, we may have witnessed a more considerable change in measuring instruments to ones which, instead of emphasizing the community's awareness of anti- and asocial behavior among men, record the more negative and "expressive" self perceptions of women.<sup>1</sup>

<sup>1</sup> Phillips and Segal (1969) expressed a similar concern about the use of the Langner Scale when they posed the following question: "Do items of the kind on the 22-item

This brings us, then, to the use of symptom checklist instruments as legitimate measures of mental illness. By now the published papers questioning the meaning of the Langner Scale and its use as a measure of mental illness are legion (e.g., Crandell and Dohrenwend 1967; Dohrenwend and Crandell 1970; Meile 1972; Dohrenwend 1973; Seiler 1973). There seems agreement that the scale measures physiological disturbances as well as psychological stress.<sup>2</sup> The use of this scale as a test of sex differences in mental illness becomes a dubious procedure at best, particularly in view of the authors' highly personal definition of mental illness. Because we reject their basic premise regarding sex and mental illness, it seems hardly appropriate to comment on Clancy and Gove's measures of response bias, although it is of some interest that they admit that their findings "could lead us to conclude that the Langner instrument is a poor scale" (p. 214).

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MH index measure psychological disturbance among men as well as they do among women?" (p. 70). Blumenthal (1967) demonstrated that for men highly significant correlations were found between the MH index and other indices of mental health. That this finding did not hold for women suggests that similar responses by men on a symptom check list are indicative of more serious problems.

<sup>2</sup> A cutting point of four or more items has frequently been applied as a measure of "impairment." In his first study of response bias, in which he utilized the same instrument, Clancy (Phillips and Clancy 1970) used a cutting point of four. Clancy and Gove use a cutting point of three items "because the respondents in this study reported fewer symptoms than is typical . . ." (Clancy and Gove 1974, p. 210).

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## SEX DIFFERENCES IN MENTAL ILLNESS: COMMENT ON CLANCY AND GOVE'S INTERPRETATIONS

Although Clancy and Gove entitle their article "Sex Differences in Mental Illness: An Analysis of Response Bias in Self-Reports," it is unlikely that their data indicate any substantial response bias or pertain, except tangentially, to mental illness. Their procedure is to correlate scores on the 22-item scale introduced by Langner (1962), which *purportedly* indicates mental illness, with three potential sources of response bias: the "trait desirability" of scores on 9 of the 22 items, a short version of the Crowne-Marlowe scale indicating "social approval," and a naysaying-yeasaying measure. From their analysis Clancy and Gove conclude "it is clear that the respondent's perception of the undesirability of the Langner Scale items, his need for social approval, and particularly his tendency to nay-say are strongly related to the number of symptoms he admits. . . . It appears that these forms of response bias affect a very wide range of items and that response bias is a very serious if generally ignored problem" (p. 214).

These conclusions appear largely unwarranted for perceived undesirability and need for social approval because they correlate only weakly ( $\gamma = .2$  and  $.15$ , respectively) with symptoms on the Langner Scale. Gamma cannot be interpreted as a percentage of explained variation; nevertheless, the low magnitude of gamma in this instance suggests that, even were the coefficients additive (Clancy and Gove, p. 211), little variation in scores on the 22-item scale could be explained by variation in perceived undesirability and need for social approval. Furthermore, using Goodman and Kruskal's (1963) procedures it is found that when considering need for social approval, gamma is not significantly different from zero at the .05 level using a 2-tailed test, even with their sample size of 402.

Although "naysaying" is moderately correlated ( $\gamma = .47$ ) with symptom scores on the Langner Scale, this coefficient appears to be arti-

factual. For some reason the large majority of Clancy and Gove's respondents have the tendency to naysay or yeasay: 73% naysay and some proportion of the remaining 27% yeasay (the percentage of yeasayers is not indicated). The bias is heavily in the naysaying direction. What could induce such a large proportion of their respondents to show this form of response bias, and most of them in only one direction? A review of the items used to measure naysaying suggests that item wording is the problem. For example, naysayers must respond negatively to both forms of the couplet "at times I have really insisted on having things my own way" (true/false) and "I never insist on having things my own way" (yes/no). However, in the second, reversed, format the response "no" produces a confusing double negative: what is the meaning of "I never insist on having things my own way—No"?

One of the items in the 22-item scale also contains the problem of a response choice producing a double negative. On the numerous occasions when I administered the 22-item scale orally to my research methods class (this format is similar to Clancy and Gove's telephone interview) the double-negative item invariably caused confusion among some students. Five of the six naysaying couplets contain a double negative item. These items might well have confused some respondents, causing them to become inadvertent naysayers.

But why should naysaying be correlated with scores on the 22-item scale? One *possible* explanation is as follows. Previous literature suggests that scores on the 22-item scale are inversely correlated with years of education (e.g., Dohrenwend 1966; Seiler and Summers 1974). Furthermore, the least educated are most likely to be confused by complicated grammatical constructions such as double-negative items. Therefore, it is feasible that naysaying and symptom level will be positively correlated because of their common relationship with education.

Additionally, Clancy and Gove note: "A naysaying score was calculated for each respondent by subtracting the number of positive responses from the number of negative responses, producing a scale with a range from +6 (consistent naysaying) to -6 (consistent yeasaying)" (p. 210). The problem is that this procedure, for example, allows a respondent to yeasay to the first three couplets, naysay to the last three, and still obtain a naysaying score of zero. Thus, the scoring of the naysaying scale is questionable.

They continue: "Unlike most studies, which have used four or more symptoms as the cutting point, we have used three or more symptoms because the respondents in this study reported fewer symptoms than is typical . . . We would also note that the differences [*sic*] found between the sexes is somewhat smaller than that found in most community surveys. . . ." In effect, the reader is left to question whether any of



the results in this study are replicable. Given the above, there appears to be little support for Clancy and Gove's conclusions concerning response bias associated with the 22-item scale, because (1) the overall results do not replicate previous studies, (2) the strongest evidence of response bias was discovered through the use of a questionable naysaying measure, and (3) the remaining measures of response bias correlate only weakly with the 22-item scale.

Clancy and Gove's inability to replicate previous findings also casts into doubt their assertion that "males and females do not differ in the perceived desirability of the items in the Langner Scale. This is fairly strong evidence against the argument proposed by Phillips and Segal (1969) and Cooperstock (1971), which hinges on women perceiving the manifestation of such symptoms as less undesirable than do males" (p. 211). It is possible that a long and confusing telephone interview induced respondents to modify their answers, thereby lowering the validity of these data.

#### The 22-Item Scale as a Measure of Mental Illness

Finally, I believe Clancy and Gove have only a vivid imagination to support their claim that the 22-item scale is a measure of mental illness, given the current state of knowledge. It is a historical fact that the 22-item scale has previously been considered a measure of mental illness (see Manis et al. 1964; Phillips 1966; Phillips and Clancy 1970). It has also been considered a measure of "psychiatric symptoms or disorder" (Langner 1962; Crandell and Dohrenwend 1967; Phillips and Segal 1969; Dohrenwend and Crandell 1970; Summers, Seiler, and Hough 1971); "psychological symptoms, disturbance, or disorder" (Dohrenwend 1966, 1967; Dohrenwend and Dohrenwend 1966, 1967; Haese and Meile 1967; Phillips 1968); "psychophysiological symptoms, disturbance, or disorder" (Langner 1965; Haberman 1965a, 1965b; Meile 1972); "emotional adjustment" (Segal, Phillips, and Feldmesser 1967); "emotional disturbance" (Segal and Phillips 1967); "mental health" (Manis et al. 1963; Muller 1966; Martin et al. 1968); and "symptoms of stress" (Meile and Haese 1969).

Regardless of claims concerning what the 22-item scale measures, what do previous studies suggest? Langner himself notes that his scale is *at best* an incomplete measure of mental illness because it fails to detect organic brain damage, mental retardation, sociopathic behavior, or the dimensions of anger, depression, anxiety, acting out, suspicion, hallucination, delusion formation, memory loss, and concentration difficulty (1962, p. 271). Manis et al. (1963) and Dohrenwend and Crandell (1970) find the scale unable to rank community and hospitalized groups "correctly" with respect to the number of symptoms they manifest.

Crandell and Dohrenwend (1967) find the scale to be composed mainly of mild (nonserious) symptoms. Phillips and Segal (1969) suggest that it is constructed of items which do not represent typical masculine symptoms such as aggressiveness, overt hostility, or active antagonism. Dohrenwend suggests that the items "may grossly under-represent psychological symptoms among lower educated Negroes and Puerto Rican respondents" (1966, p. 29).

In reviewing empirical validations of the 22-item scale, I previously noted:

Most troubling of all, however, is the heavy reliance upon "known-group" techniques for the validation of the 22-item scale. It appears that use of this technique has resulted from not fully differentiating two theoretical questions. The first question, the one upon which the 22-item scale's utility rests, is: Are respondents with some high number of symptoms, as represented in the 22-item scale, mentally ill? The second, which is tangential to the first is: Do the mentally ill evidence some unusually high number of the 22-item-scale symptoms? If the answer to the second question is affirmative, that is, that mental patients evidence a statistically abnormal number of such symptoms, this does not imply that the answer to the first question is also affirmative, that those residing in the community who evidence a statistically abnormal number of symptoms are also mentally ill. This obviously is the fallacy of affirming the consequent. Using "known group" techniques to validate the 22-item instrument answers the second question: Do those in mental hospitals, on the average, have more symptoms than community residents? However, this technique and this logic do not imply that community residents with many symptoms are mentally ill. Thus, these validation studies, which are the best currently available, present weak, if any, substantiation for the assertion that the 22-item scale "detects" mental illness in community respondents. [Seiler 1973, p. 255]

Just as troubling as the above is the fact that users of the 22-item scale have not come to grips with the reality that any presumed symptom may or may not be indicative of an underlying disorder, depending on the context in which it is expressed. A recently escaped convict is not paranoid in thinking that virtually everyone is out to get him.

In case the above comments do not persuade the reader that the 22-item scale does not only measure mental illness, a demonstration is in order. Four items from the instrument are given below. All of them are among the items most frequently endorsed as indicating mental illness by the Midtown Manhattan respondents (Langner 1962). They occupy, respectively, the first, fourth, eighth, and tenth places among items so endorsed. Using Langner's original cutting point, positive responses to any four questions indicate that a person is mentally ill. If you respond "yes" to these four items you would be considered mentally ill: "Are you the worrying type?" "I have periods of such great restlessness that I cannot

sit long in a chair." "Every so often I feel hot all over." "There seems to be a fullness in my head or nose much of the time." Time's up. How did you do?

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## RESPONSE BIAS, SEX DIFFERENCES, AND MENTAL ILLNESS: A REPLY

It doesn't take a careful reading of either Cooperstock and Parnell or Seiler to realize that they don't like our data and don't like our conclusions. As we attempt to show in the following paragraphs, although the two comments touch occasionally on important questions, their overall effect is to confuse the issue, for they are replete with inaccurate and irrelevant statements and almost totally ignore what our study actually showed.

First, let us comment on the definition of mental illness used in Gove and Tudor (1973), which so upsets Cooperstock and Parnell and which bears on some of Seiler's questions regarding the utility of the Langner Scale. The history of science has shown that an essential ingredient in the development of science is the grouping of the phenomena under investigation into homogeneous categories. As Scheff (1966) has so cogently noted, mental illness has often been treated as a residual category into which very disparate phenomena have been grouped. To treat as the same disorder all phenomena that at some time receive a psychiatric label

makes about as much sense as for a doctor to treat all persons admitted to a general hospital (including those admitted to the psychiatric ward) as suffering from the same disease.

Gove and Tudor utilized a definition of mental illness which they felt captured the historic and cross-cultural meaning of the term, had theoretical utility (e.g., Gove 1968, 1970), and delineated a set of phenomena that reacted to therapeutic intervention in a relatively similar manner (i.e., it is reactive to drug therapy and, to some extent, psychotherapy). In short, they defined mental illness as a functional (i.e., nonorganic) disorder involving personal distress and/or mental disorganization. Thus they treated organically caused disorders (e.g., brain damage and toxins) as a separate phenomenon. Although some (perhaps many) persons would want to call both phenomena mental illness, we would expect anyone with even a rudimentary understanding of the phenomena in question to think this distinction highly relevant. Their definition also treated the personality disorders as a separate phenomenon. Let us look at the reasons for this. First we note that Cooperstock and Parnell's statement that the personality disorders are a form of psychosis is simply incorrect. Klein and Davis indicate that the "outstanding characteristic of the personality disorders is developmental defects or pathological trends in the personality structure, with minimal subjective anxiety, and little or no sense of distress. In most instances, the disorder is manifested by a life-long pattern of behavior rather than mental or emotional symptoms" (1969, p. 336). Persons with a personality disorder are characterized by impulsive asocial acts that are seriously disruptive to others. It is our belief that such persons tend to be treated as mentally ill as a means of social control for behavior that is felt to be intolerable. Further, it is very clear that counseling and drug treatment, the forms of therapy which are effective with the disorders covered by Gove and Tudor's definition, have virtually no effect on most persons diagnosed as having a personality disorder. Again, although some (perhaps most) persons would want to label the personality disorders a form of mental illness, virtually everyone with any understanding of the disorders in question would want to distinguish between the personality disorders and what Gove and Tudor label mental illness.

In short, there are historical, theoretical, therapeutic, and behavioral reasons for the distinction made by Gove and Tudor. Whether one follows these authors and uses the label of mental illness for the functional disorders associated with distress and/or disorganization or wishes to talk about mental illnesses of type A, type B, and so forth, is not important; what is important is that we treat the phenomenon Gove and Tudor label mental illness as a separate phenomenon. In the paragraphs that follow we retain their use of the term "mental illness," but the reader should be aware that we are using it in the specialized sense discussed above.

The use by Gove and Tudor of their definition of mental illness led them to an important discovery, namely, that women in modern Western industrial societies have higher rates of mental illness than men. This finding was uniformly supported by all the evidence on mental illness derived from mental hospitals, psychiatric treatment in general hospitals, outpatient clinics, psychiatric treatment by general physicians, and community surveys. There are three conceptually plausible explanations for this phenomenon. First, women in our society experience more stress than men, in short, a role explanation. Second, women are inherently more prone to mental illness, in short, a genetic or biological explanation. Third, the higher rate of mental illness apparent among women is not real but an artifact of cultural norms. This is the position of Phillips and Segal (1969) and Cooperstock (1971), who argue that the apparent higher rate for women is a consequence of its being more appropriate culturally for them to verbalize their emotional difficulties. That is, men and women have comparable rates of mental illness, but women appear to have higher rates because they are more expressive about their mental state. It is this proposition that we were concerned with in our paper. We note that in her reply Cooperstock has completely reversed her position. Instead of adhering to her initial cultural explanation, she and Parnell now take the position that women are inherently more likely than men to become mentally ill. In short, they argue that the findings reported in Gove and Tudor are "tautological," for they are comparable to a report that women are more likely than men to have disorders of the female genito-urinary organs, when men don't have such organs. They cite no source in support of their position, and for good reason; there simply is no evidence that the experience of distress and/or a functional psychosis is inherently a female experience. In fact, not only is there no evidence to support their position but their position runs directly counter to the evidence reported elsewhere (Gove and Tudor 1973; Gove and Lester 1974; Gove 1972*a*, 1972*b*, 1973) which strongly suggests a stress or role explanation of the higher rates of women.

As noted, we investigated whether or not the apparent higher rates of mental illness among women could be due to a cultural norm making women more willing to verbalize their symptoms of mental illness. As our measure of symptoms we used the Langner Scale. This has been the scale most commonly used to measure symptoms of mental illness in community surveys. Furthermore, it has been used in earlier studies of response bias (Dohrenwend 1966; Phillips and Clancy 1970). Both replies attack our use of the Langner Scale, and some comments are in order.

Seiler notes that the scale does not measure organic brain damage, mental retardation, or sociopathic behavior, and we agree. Another way of saying this is that the Langner Scale does not measure those behaviors

and conditions which we do not wish to measure. What it does measure is symptoms of distress (including anxiety and depression)—exactly what we would like it to measure. It is true that the scale does not measure psychotic disorganization and that it does not distinguish between the moderately mentally ill and the severely mentally ill. As the literatures of both physiological indicators and verbal reports of distress suggest, this is apparently because a well-developed delusional system helps minimize the extent to which the mentally ill experience extremely high levels of distress (Gove and Howell 1974, p. 94). This, however, does not pose a serious problem for us because (a) we would expect only four or five psychotic persons in our sample, and (b) the literature (e.g., Dohrenwend and Crandell 1970) strongly indicates that these psychotic persons will score well above our cutoff point because they will be at least moderately distressed. Furthermore, there is an overriding reason why the use of the Langner Scale is appropriate. The major article (Phillips and Segal 1969) to argue that cultural norms make women appear to have higher rates of mental illness than men is based on an analysis of the replies of men and women to the Langner Scale. It seems to us eminently reasonable that if we are going to evaluate Phillips and Segal we should look at the same type of data they did. However, Seiler and Cooperstock and Parnell, who are impressed with Phillips and Segal's work, indicate that the scale Phillips and Segal used is inappropriate for us. They seem to be saying that if responses to the Langner Scale suggest that women have high scores because they are more expressive, that is strong evidence, but if, instead, an analysis of the responses to the Langner Scale contradicts the expressiveness argument, it is no evidence at all. At this point we might mention that neither Seiler nor Cooperstock and Parnell are particularly judicious in their review of the literature on the Langner Scale. For example, Cooperstock and Parnell cite Meile (1972) as attacking the scale, whereas Meile, in fact, mounts a strong defense of it. (Somehow Seiler, too, in his article [1973] reviewing the Langner Scale, manages to ignore the data reported in Meile.) Similarly, Seiler says that Dohrenwend and Crandell found that the Langner Scale did not "rank community and hospitalized groups 'correctly' with respect to the number of symptoms they manifest." This is very misleading. Dohrenwend and Crandell found that the scale did not differentiate between outpatients and inpatients, but that it differentiated sharply between patients and nonpatients.

We might note that our defense of the Langner Scale does not mean that we think it is ideal—we don't. The scoring system is weak and, as Seiler notes, some of the items are poor. However, the scale does about as good a job as most scales in the social sciences and, to paraphrase

Seiler, given the literature, it takes a vivid imagination to assume that the Langner Scale does not do a reasonably good job of distinguishing between those who experience a large number of psychiatric symptoms and those who experience a small number.

As noted, Phillips and Segal argued that the reason women scored higher on the Langner Scale and similar instruments is that the manifestation of (this type of) psychiatric symptoms is culturally more appropriate for women than for men. In short, men see the symptoms as more undesirable than women. Cooperstock makes a similar argument to explain why females have a higher rate of psychotherapeutic drugs. However, we found *absolutely* no difference between men and women in the extent to which they see the psychiatric symptoms used in the Langner Scale as undesirable. Similarly, we found no difference between men and women in the extent to which responses were affected by the respondents' need for social approval. However, women were more likely than men to naysay, and this lowered the number of symptoms they reported. When we controlled for the three types of response bias, we found that the difference between men and women did not diminish, but instead increased. Thus our results are directly counter to the argument posed by Phillips and Segal and by Cooperstock. In short, our data indicate that the higher rate of reported psychiatric symptoms among women is not an artifact of response set (bias) but reflects real differences in the experience of such symptoms. Seiler, however, questions our results because he feels that (a) they are not replicable, and (b) our measures of response bias are either weak or questionable. Let us now turn to these two issues.

Seiler notes that our "overall results do not replicate previous studies." We are not quite sure what he means by this. It is true that our respondents reported fewer symptoms than is typical and that the difference between the sexes is somewhat smaller than that found in most community surveys. It is also true that in our presentation we used three or more symptoms instead of the traditional four or more. Let us comment on these points. First, we used three or more symptoms because doing so gave us large marginal values; if the marginal values are small, partial gammas, which we used extensively, are unstable. Prior to writing the paper we had also run the analysis using a cutting point of four or more symptoms, and the results were virtually identical with those reported. Second, although the reported number of symptoms was relatively low (18.5% of our respondents reported four or more), it was higher than that reported by some investigators (e.g., Haberman 1965; Summers, Seiler, and Hough 1971). Third, although the difference between the sexes is smaller than that reported in the majority of studies, it is larger than reported in some (Gove and Tudor 1973). In short, although our



results differ somewhat from the median values of other studies, they are within the range reported by other studies and clearly can be considered as a replication of their results.

Seiler also "questions whether any of the results in [our] study are replicable." The way to find out whether results can be replicated is to try to replicate them. Gove, McCorkel, and Fain (1975) have, with two notable modifications, already attempted a replication. One modification was the use of a face-to-face interview instead of a telephone interview; this should meet another of Seiler's criticisms, namely, that our results might be due to the use of a telephone interview. The second significant modification was the use of three different scales of psychiatric impairment, the Langner (1962) Scale, the Zung (1965) depression scale, and the Dupry (1971) scale of general well being.<sup>1</sup> In the replication six demographic variables were looked at: income, education, sex, marital status, race, and age. At the zero-order level sex was the most powerful predictor of psychiatric symptoms, with women reporting more symptoms than men. The three different types of response bias used in Clancy and Gove were then controlled for. With these controls sex was still the single most powerful demographic predictor of psychiatric symptoms.<sup>2</sup> In short, the replication supports the conclusion reached in Clancy and Gove that the higher rates of symptoms reported by women are real and are not an artifact of response bias. Furthermore, the replication demonstrates that the results reported in Clancy and Gove are not specific to the Langner Scale but are generalizable to other measures of mental illness.

Seiler is concerned that our naysaying scale produces more naysayers than yeasayers and suggests that item wording is the problem. He argues that the double negatives in the scale confuse respondents, particularly those with little formal education. Thus, he hypothesizes that confused, less educated respondents are our naysayers. Since the literature suggests a negative relationship between education and scores on the Langner inventory, he then goes out on a limb to posit that the reason naysaying and symptom scores are related is their common correlation with education. Unfortunately, we must saw down the tree.

In contrast with Seiler, we are not at all surprised at the degree of naysaying in our study. Indeed, we would be surprised if the findings turned out any other way. As mentioned in Phillips and Clancy (1970) and developed in Clancy (1971), naysaying is a very common response

<sup>1</sup> This is the mental health scale used in the current National Health Examination Survey.

<sup>2</sup> There were some differences in the results, the most noticeable being that in the replication the difference between the sexes did not increase when response bias was controlled for.

pattern when people are being questioned regarding their physical or mental health. Since we know that "healthy," "symptom-free" responses are often rewarded (with a clean bill of health or a job) and "unhealthy" ones punished (by more time-consuming tests or a higher insurance premium), one might say that healthy responses are positively reinforced. It so happens that the "no" or denial response category is the response considered healthy in most medical inventories. Therefore, many people, including Clancy and Gove's and Seiler's respondents, can be expected to adopt an indiscriminate naysaying response pattern, saying no and never to almost anything, when we question them concerning their physical and mental health. They have been trained to do so.

Phillips and Clancy (1970), for example, using a dramatically different naysaying instrument from that to which Seiler objects (one without any double negatives), also found a strong skew in the direction of naysaying. In that study some respondents (the naysayers) denied symptoms which they almost certainly experienced, such as "sometimes feeling sleepy," "having had a cold during the past 10 years," or "ever feeling grouchy or irritable." We suspect that if we embedded the item "are you still breathing?" in a standard medical inventory, some people would say "no." While we agree with Seiler, then, that the double negatives in our scale can be confusing, such confusion has little, if anything, to do with the preponderance of naysayers in our sample.

More important is Seiler's contention that the *positive* correlation between naysaying and Langner Scores is spurious, a result of their common negative correlation with education. This argument is simply inconsistent with earlier hypotheses and data. Building on the work of Dohrenwend, Colombotos, and Dohrenwend (1968), Phillips and Clancy (1970) and Clancy (1971) hypothesized a *positive* relationship between naysaying and social-class measures generally, education in particular. Using different measures of naysaying, insignificant positive correlations were identified. Of critical importance is the fact that, as Clancy and Phillips (1972) have shown elsewhere, for the data under consideration our measure of naysaying has a weak positive correlation with social class. Furthermore, the replication by Gove et al. (1975) found that an almost identical measure of naysaying did not have a significant correlation with either education or income. Therefore, Seiler's speculations regarding spurious correlations are clearly without foundation. In short, the strong negative correlation between our naysaying scale and the Langner inventory ( $\gamma = .47$ ) does appear to be a consequence of response bias. At the same time, Seiler's useful comments on the double negative problem indicate that more work can and should be done to improve our ability to measure and correct for this form of bias in ongoing research.

Seiler also argues that "it is unlikely that [our] data indicate any substantial response bias," and points to what he feels to be especially weak relationships of the perceived undesirability of symptoms and the need for approval with scores on the Langner Scale. He begins this part of his discussion by stating that "gamma cannot be interpreted as a percentage of explained variation," but, as Costner (1965) has shown, gamma is a PRE measure and should be interpreted as a percentage of explained variation. We would point out that perceived undesirability is as strongly related to scores on the Langner Scale as is sex and that need for approval is almost as strongly related. Similarly, Clancy and Phillips (1972) have shown that for these data perceived undesirability is as strongly related to scores on the Langner Scale as is social class, and need for approval is almost as strongly related as is social class. Naysaying, of course, is more strongly related than either sex or class. The data presented in Clancy and Phillips demonstrate "that the combined effects [*sic*] of the three response determinants on the Langner scale is approximately three times greater than that of socioeconomic position" (1972, p. 22). Because socioeconomic position is traditionally held to be the most powerful predictor of scores on such scales as the Langner, it seems unlikely that our response bias variables are dealing with an inconsequential amount of explained variance. Furthermore, in the replication by Gove et al. (1975), the predictive power of each of two variables, the perceived undesirability of symptoms and the need for approval, on scores on the three psychiatric scales used is greater than that of income, education, marital status, race, or age; and the total predictive power of the three response bias variables is as strong as the combined effect of these five demographic variables plus sex of respondent.

In summary, there is nothing in the comments of either Seiler or Cooperstock and Parnell that would lead to a modification of our conclusion that the consistent finding in community surveys that women have higher rates of mental illness than men appears to reflect real differences. Furthermore, if we can assume that a respondent's willingness to admit symptoms in an interview situation reflects his or her willingness to admit them in other contexts, it would appear that the higher rates of mental illness<sup>3</sup> are not a product of women's being more willing to discuss their emotional problems.

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<sup>3</sup> Again, as defined by Gove and Tudor (1973).

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## COMMENT ON MC KENNA'S "MARRIAGE AND FERTILITY IN POSTFAMINE IRELAND"

Edward E. McKenna's article (*AJS* 80 [November 1974]: 688-705) represents a welcome application of regression and path analysis to historical demographic data. Unfortunately, the final part of the paper (pp. 701-03), in which the general fertility ratio ( $X_{12}$ ) is regressed on the average mortality rate ( $X_8$ ), the percentage of females married ( $X_9$ ), and the marital fertility ratio ( $X_{11}$ ), is flawed by an inadequate familiarity with some technical details of either demography or regression.

It is well known to demographers that overall fertility is the product of marital fertility and the proportion married. That is, redefining  $X_9$  as a proportion instead of a percentage, in order to eliminate a constant factor of 100, we have  $X_{12} = X_9 X_{11}$ . It is truly curious that McKenna defines his measures to have this property but never utilizes it. He stops short with this statement: "The number of females marrying, in combination with their fertility after marriage, should account for virtually all of the variation in general fertility levels" (p. 701). He then regresses  $X_{12}$  on  $X_8$ ,  $X_9$ , and  $X_{11}$ .

When mathematical relationships exist between variables, it is obvious that they should be exploited. We have, through definitions, a multiplicative relationship among the three variables  $X_{12}$ ,  $X_9$ , and  $X_{11}$ . Why, then, should we attempt to impose a linear model, including, moreover, a fourth variable ( $X_8$ ) which is not part of the mathematical relationship?

In this comment I suggest three possible ways of evaluating the relative importance of  $X_9$  and  $X_{11}$  as determinants of  $X_{12}$ . Each of these three ways recognizes that, together,  $X_9$  and  $X_{11}$  *completely determine*  $X_{12}$ . Two approaches use standardized variables; the third uses unstandardized variables. But all three indicate essential agreement on the relative importance of the two determinants of the general fertility ratio. In contrast, McKenna's multiple-regression approach offered qualitatively different results, depending on whether standardized or unstandardized variables were used.

At the outset I shall discard the measure of infant mortality,  $X_8$ . McKenna has shown it to have a major impact on marital fertility,  $X_{11}$ , and there is no theoretical reason to believe it has a separate effect on  $X_{12}$ . Moreover, as is mentioned above,  $X_{12}$  is completely determined by  $X_9$  and  $X_{11}$  alone.

First, one can put the multiplicative relationship into linear form by defining  $Y$ ,  $U$ , and  $V$ , respectively, to be  $\log X_{12}$ ,  $\log X_9$ , and  $\log X_{11}$ , so that  $Y = U + V$ .

Following Duncan (1966), I standardize  $Y$ ,  $U$ , and  $V$ , representing the standardized variables by  $y$ ,  $u$ , and  $v$ , respectively. Then

$$y = \left( \frac{s_U}{s_Y} \right) u + \left( \frac{s_V}{s_Y} \right) v,$$

and  $s_U/s_Y$  and  $s_V/s_Y$  are identified as the desired path coefficients. Extending Duncan's discussion of this way of handling multiplicative relationships, I next show that  $s_Y$ ,  $s_U$ , and  $s_V$ , the standard deviations of  $Y$ ,  $U$ , and  $V$ , respectively, can be approximated in terms of the basic characteristics of  $X_{12}$ ,  $X_9$ , and  $X_{11}$  themselves.

Let us refer to the mean and standard deviations of these original variables as  $\bar{X}_{12}$ ,  $\bar{X}_9$ ,  $\bar{X}_{11}$  and  $s_{12}$ ,  $s_9$ , and  $s_{11}$ . A statistical procedure based on a Taylor series expansion (see, for example, Brownlee 1965, p. 144), leads to the approximation  $s_Y \doteq s_{12}/\bar{X}_{12}$ , and similarly for  $s_U$  and  $s_V$ .

The coefficients for the proportion married and for marital fertility are then closely approximated by  $(\bar{X}_{12}s_9)/(\bar{X}_9s_{12})$  and  $(\bar{X}_{12}s_{11})/(\bar{X}_{11}s_{12})$ , respectively. Note that these have the ratio  $CV_9/CV_{11}$ , where  $CV_9$  and  $CV_{11}$  are the coefficients of variation of the original  $X_9$  and  $X_{11}$ . The estimates of these coefficients are presented in the first panel of table 1.

There are good arguments, however, for not taking logarithms but retaining the original variables (with or without standardization) as McKenna did. My second approach uses the decomposition which occurs if one standardizes the original variables by working with  $x_i = (X_i - \bar{X}_i)/s_i$  for  $i$  equal to 12, 9, and 11. Then, equivalently,  $X_i = \bar{X}_i + s_i x_i$ . Substituting into  $X_{12} = X_9 X_{11}$  one obtains  $(\bar{X}_{12} + s_{12}x_{12}) = (\bar{X}_9 + s_9x_9)(\bar{X}_{11} + s_{11}x_{11})$ , which simplifies to

$$x_{12} = \left( \frac{s_9 s_{11}}{s_{12}} \right) x_9 x_{11} + \left( \frac{s_9 \bar{X}_{11}}{s_{12}} \right) x_9 + \left( \frac{s_{11} \bar{X}_9}{s_{12}} \right) x_{11} - \left( \frac{s_9 s_{11}}{s_{12}} \right) r_{9,11},$$

where all quantities are as defined earlier and  $r_{9,11}$  is the correlation between  $X_9$  and  $X_{11}$ . We thus see that a multiplicative relationship in the

TABLE 1

COEFFICIENTS ASSOCIATED WITH EFFECT OF PROPORTION MARRIED ( $X_9$ ) AND MARITAL FERTILITY RATIO ( $X_{11}$ ) ON GENERAL FERTILITY RATIO ( $X_{12}$ )

APPROACH	YEAR			
	1851	1871	1891	1911
1. Standardized coefficient of log $X$ :				
$X_9$ .....	0.78	0.83	0.73	0.94
$X_{11}$ .....	0.80	0.33	0.60	1.24
2. Standardized coefficient of $X$ :				
$X_9$ .....	0.89	0.83	0.72	0.94
$X_{11}$ .....	0.91	0.33	0.60	1.24

NOTE.—I am grateful to Professor McKenna for kindly furnishing the data required for tables 1 and 2.

unstandardized variables can be restated as an additive, second-order linear relationship in the standardized variables.<sup>1</sup> In this form, the effect due to the proportion married is  $s_9\bar{X}_{11}/s_{12}$ , and the effect due to marital fertility is  $s_{11}\bar{X}_9/s_{12}$ . The relative importance of these two effects is indicated by their ratio, which is  $CV_9/CV_{11}$ . The estimates of these coefficients are presented in the second panel of table 1.

A third approach to the problem uses the original unstandardized variables. It takes a rather different form, however, because I know of no statistical procedure which will yield coefficients of the variety seen above. Instead, I use a procedure for partitioning the variance of  $X_{12}$ .

Approximate formulas for the variance of a product are easily obtained from a Taylor series expansion (as used above), and an exact formula has been developed by Goodman (1960). These formulas, not reproduced here, partition the variance of  $Y = UV$  (say) into three parts: (a) the variance of  $U$  times the square of the mean of  $V$ , plus (b) the variance of  $V$  times the square of the mean of  $U$ , plus (c) a measure of the co-variation between  $U$  and  $V$ . We are thus able to interpret  $a$  as the portion of variance in  $Y$  which is due to variation in  $U$ . That is, if  $V$  had not varied at all,  $a$  would have been identical with the variance of  $Y$ . Similarly,  $b$  is the portion of variance in  $Y$  which is due to variation in  $V$ , and  $c$  is the portion due to interaction between  $U$  and  $V$ .

These three parts of the total sum of squares for McKenna's data are given in table 2. The quantity representing the effect of the proportion married ( $X_9$ ) is  $\bar{X}_{11}^2 s_9^2$ . The quantity representing the effect of the marital fertility ratio ( $X_{11}$ ) is  $\bar{X}_9^2 s_{11}^2$ . The relative importance of these two effects

<sup>1</sup> Because the product of  $x_9$  and  $x_{11}$  is not in standard form, the coefficient of this variable must be multiplied by the standard deviation of  $x_9 x_{11}$  if it is to be interpreted as a path coefficient of the usual sort.

TABLE 2  
PARTITIONING OF VARIANCE OF  $X_{12}$  INTO COMPONENTS FOR  $X_9$ ,  $X_{11}$ , AND INTERACTION

INTERACTION	YEAR			
	1851	1871	1891	1911
Due to $X_9$ alone .....	8.60	35.03	7.94	9.27
Due to $X_{11}$ alone .....	8.92	5.67	5.48	16.14
Due to interaction .....	-6.70	10.56	1.79	-14.85
Total variance of $X_{12}$ .....	10.82	51.27	15.21	10.56

on the variance of the general fertility rate is indicated by their ratio, which is  $(CV_9/CV_{11})^2$ .

Table 2 manifests two aspects of McKenna's data that did not reveal themselves in his paper: an unusually high variance in the general fertility ratio in 1871 and a negative correlation between the proportion married and the marital fertility ratio in 1851 and in 1911.

Table 2 and the two panels of table 1 are in close agreement regarding the relative impact of the proportion married ( $X_9$ ) and the marital fertility ratio ( $X_{11}$ ). (As already noted, the ratio of the effects is the same for the two procedures shown in table 1, and the ratio of effects in table 2 is the square of the ratio in table 1.) We see that (a) in 1851,  $X_9$  and  $X_{11}$  were almost exactly equal in importance, but  $X_9$  was *slightly* less important than  $X_{11}$ ; (b) in 1871,  $X_9$  was a much more important determinant of general fertility than was  $X_{11}$ ; (c) in 1891,  $X_9$  was somewhat more important than  $X_{11}$ , but (d) by 1911 there was another reversal, with  $X_9$  somewhat *less* important than  $X_{11}$ .

These conclusions are in approximate agreement with McKenna's standardized coefficients for 1871, 1891, and 1911. However, I cannot support any of his interpretation based on unstandardized regression coefficients, even though my third approach used unstandardized variables. The consistency of my own conclusions, reached by three different routes, also calls into question McKenna's efforts to reconcile his standardized and unstandardized coefficients with each other.

Finally, I wish to suggest how known multiplicative relationships can be incorporated into a path-analysis format for examples in which all variables are standardized. First consider that the multiplicative relationship is to be represented as an additive relationship in the logarithms, as in the first approach above. It may well be that the *other* relationships involving these variables are not conveniently expressed in terms of logarithms. In fact, in some instances one might demonstrably violate other known mathematical equalities by replacing a variable with its logarithm. My suggestion is that a variable be permitted to appear in



two (or more) mathematically related forms in the same diagram, with a connecting path whose coefficient is unity. Alternatively, one may be able to restate a multiplicative relationship as an additive relationship in other functions of the standardized variables, as in the second approach. Again, mathematical links would be represented by paths having a coefficient of unity. I am well aware of the ensuing complications in the algebra and in the basic equations of path diagrams, but some comprehensive procedure is required to permit a variable to appear in more than one form in the same model.

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#### COMMENT ON MC KENNA

Edward McKenna's recent multivariate analysis of postfamine marriage and fertility patterns in Ireland is a welcome addition to a literature that is overly restricted to the British Isles. I would like to add several points which might clarify and partially resolve some of the problems in the conclusion to his paper. These concern primarily his theoretical model and not the fine mathematical analysis.

First, Connell's thesis concerning the role of early marriage in marital fertility and population growth in the years 1750-1845 was, as McKenna notes, the basis for suspicions that trends reversed after the famine. Recently, Connell's thesis has been subjected to critical support and tempered doubt (Lee 1968; Drake 1969). Both supporters and doubters agree that the rural economy was beginning to change long before the famine and perhaps as early as 1825. Thus it is limiting to begin an analysis of social change as late as 1851.

Second and more important, it should be stated that all of the social scientists working on this period have stressed *exclusively* the agricultural variables affecting marriage and fertility. This is an error founded partly on Connell's pioneering work and partly on 19th-century interpretations.

Rural problems were equated with land-tenure problems. However, there was another, but little examined, side to the rural economy.

At the turn of the 19th century, the linen trade penetrated virtually all of northern and northwestern Ireland. This industry was not confined to the cities and towns but reached households in the most remote "wild districts." Women and children in the countryside, primarily the very poor ones, spun flax raised by the men, and the money from this work went to pay rising land rents.

The linen yarn industry of the remote countryside began to collapse in the third decade of the 19th century, primarily because the women working at their wheels could not compete with the increasing number of power-driven spinning mills. Tens of thousands of women lost their means of contributing to household economies. The importance of the rural linen industry for population growth, marriage patterns, and migration is not yet well understood, but it is presumed that spinning skills substituted for accumulated capital dowries, adding to early marriage in the first decades of the 19th century and perhaps much earlier.

Thus, the female side of the rural economy (textiles) may have been just as important qualitatively to marriage decisions and thus to demographic trends as the male side (agriculture). New work on the pre- and postfamine periods must consider the total rural economy and not merely agriculture. The female side has not yet entered the models, McKenna's included.

I am currently conducting research on female domestic industries in Ireland from 1750 to the present.

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#### FERTILITY AND MARRIAGE IN IRELAND: A REPLY TO PULLUM AND ALMQUIST

Professor Pullum comments that the final part of my paper (pp. 701-3) betrays "an inadequate familiarity with some technical details of either demography or regression." I dare say the section in question betrays lack of familiarity with both. My failure to see fully the demo-

graphic relationship between the proportion of women married, marital fertility, and general fertility is the more embarrassing problem. After Pullum points it out, it seems less a "technical detail" than a fairly obvious demographic fact, one which should not have required much expertise to recognize. I have less difficulty deferring to Pullum's statistical expertise. The techniques he demonstrates for examining and exploiting these relationships, while they do not negate my own, are more exhaustive and instructive. I thank him for pointing them out.

Mr. Almquist's comments are also well taken. First, I agree with his observation that the analysis should perhaps have begun at an earlier date. The famine of the late 1840s was the culmination of crop failures that began in the 1820s and 1830s. A major statistical indication of this trend was a gradually increasing mortality rate during the 1830s and the early 1840s (McKenna 1972, p. 55). Yet the famine of the late 1840s did represent a watershed in the pace of change, not only in the agricultural sector but in virtually all areas of behavior. In addition, the famine has been taken as a dividing point in most of the previous work on Irish historical demography (whether it centers on the prefamine *or* the post-famine period). I simply decided, at the time, to follow the pattern. Nevertheless, I agree that this dividing point is not sacred. Unfortunately, the type of analysis undertaken in my article cannot easily be extended beyond 1841. The 1821 and 1831 censuses are very limited in terms of detail, and their accuracy is suspect. Moreover, the agricultural statistics of even the 1841 census have been questioned (Bourke 1965).

Almquist's second argument is also valid. My study does ignore the female (textile) side of the economy, as does most, if not all, previous work in the area. Whether this question has, indeed, been *totally* ignored appears to be a matter of debate, hinging on what one defines as social scientific study. Freeman (1957, pp. 82-89), for one, presents an extensive discussion of nonagricultural trade and industry, especially textiles, and seems to come to some of the conclusions Almquist suggests are in order for this sector of the Irish economy.

At any rate, there are compelling reasons for the relative lack of empirical concern with this problem. First, as Almquist notes, the cottage industry in textiles began to collapse early in the 19th century, well before the first fairly reliable census of 1841. As a result, the statistical analysis of the problem has had to be confined so far to periods during which we can assume it was relatively less important. Second, the data on female occupations during even these later periods are open to varying interpretations. The 1841 census commissioners' definition of male occupations, for example, seems to rule out a study of male by-employment in cottage industry: ". . . the occupation which we sought was that which

the individual himself considered to be the *chief business* of his life. It is absolutely necessary to keep this fact clearly in view, otherwise the tables might be erroneously considered as showing the total number of persons in the country pursuing particular occupations, whilst its true effect is merely to show the total numbers whose *leading* or acknowledged occupation is of a particular description" (Great Britain 1843, p. xxi). On the other hand, the census commissioners used a somewhat different approach to women's occupations: "It will be observed, that having sought the occupation of every individual, we obtained that of the females as well as that of the male portion of the population, at least so far as such occupation was considered profitable" (p. xxii). So, instead of being limited to *chief* occupations, women's occupational engagement was defined as anything that was considered "profitable," which, presumably, would include by-employment in the home in textiles. Yet the profitability of such employment was to be judged by the respondent, a procedure which may have introduced an unascertainable degree of bias.

The earliest available data (for 1841) do indicate that about 40% of the women in Ireland above age 14 were occupationally engaged, with the highest percentage in Ulster (52%), as Almquist suggests. About 60% of the occupationally engaged women ministered to clothing needs, with the highest percentage again being in Ulster (80%). Finally, in 1841, about three-quarters of the women in the clothing industry were classified as spinners. Between 1841 and 1851 there were only slight declines in the total percentages of women occupationally engaged and of those engaged specifically in clothing trades. But there were major changes within the clothing sector of the female labor force: the percentage engaged in spinning declined to 30 for Ireland as a whole and to 18 in Ulster. This last pattern suggests that the famine disrupted not only the male (agricultural) sector of the economy, but also the female (textile). In particular, by destroying the agricultural base of cottage industry, especially spinning, which could easily be done in the home, the famine seems to have accelerated the shift of textile manufacture from the cottage to the factory.

This interpretation is indirectly supported by the data reported in table 1. These data show the partial regression coefficients between the percentage of women (aged 15-44) married and both the five original independent variables and a sixth independent variable, the percentage of women occupationally engaged (the original analysis is reported in parentheses). The multiple correlation coefficients, adjusted for degrees of freedom (Johnson 1972, pp. 129-30), indicate that the addition of female occupational engagement does increase the explained variance in female marriage appreciably in 1871 and 1891. Moreover, the inclusion of female

TABLE 1

STANDARDIZED PARTIAL-REGRESSION COEFFICIENTS AND MULTIPLE-CORRELATION  
COEFFICIENTS BETWEEN SELECTED VARIABLES AND PERCENTAGE OF FEMALES  
(15-44) MARRIED

YEAR	INDEPENDENT VARIABLES						$\bar{R}^2$
	1	2	3	4	5	6	
1851 ....	0.98 (1.18)	0.60 (0.67)	-0.06 (-0.35)	0.07 (-0.01)	0.22 (0.30)	-0.30	.20 (.19)
1871 ....	0.47 (0.29)	0.51 (0.65)	0.07 (0.01)	-0.03 (-0.16)	-0.17 (0.14)	-0.77	.59 (.17)
1891 ....	-0.23 (-0.62)	-0.15 (-0.16)	0.46 (0.88)	0.97 (0.43)	0.15 (0.35)	-0.82	.29 (.05)
1911 ....	-0.38 (-0.25)	-0.35 (-0.36)	0.55 (0.41)	0.80 (0.92)	0.55 (0.48)	0.18	.46 (.48)

NOTE.—Variables: 0 = percentage of females (15-44) married; 1 = holdings per 100 males in agriculture; 2 = median holding size; 3 = percentage of males in agriculture classified as "farmer" or "grazier"; 4 = percentage of males engaged in nonagricultural occupations; 5 = sex ratio; and 6 = the percentage of females engaged in the labor force. The coefficients in parentheses are the regression weights derived from the original five-predictor regression equation.

occupational engagement considerably alters some of the regression weights of the other independent variables. The only fairly consistent changes, however, are in the lowered impact of agricultural holdings per male in agriculture on marriage. The rest seem random. Still, Almquist's point about the importance of female occupational engagement seems to take on greater weight from these data.

The most important pattern in table 1, however, calls into question the major thrust of Almquist's argument. Instead of acting as a positive influence on marriage, the impact of female occupational engagement was negative until 1911 and quite strongly so in 1871 and 1891. These instances of negative influence suggest that the occupational data reported in the censuses during the postfamine period refer to something quite different from what Almquist has in mind. In particular, they seem to refer to full-time employment, and the indications are that such employment was negatively related to female marriage. Two interpretations of this negative relationship seem plausible. On the one hand, as Kennedy suggests (1973, pp. 170-71), there may have been a tendency for women to choose employment and remain single in order to avoid rural marriage with its lower standard of living. On the other hand, it is possible that marital status here becomes the causal variable instead of the dependent variable. Failure to marry could have forced large numbers of women to seek full-time employment.

Either of these interpretations is supported by the data. Both are quite different from what Almquist suggests. But the data quite probably refer to something with which Almquist is only marginally concerned—full-time employment. These are the only data available at this time, however.

Granting Almqvist his point, then, I can only hope that he is successful in generating more adequate data.

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#### ON DURKHEIM, ANOMIE, AND THE MODERN CRISIS

One cannot help feeling ambivalent about Stephen Marks's essay "Durkheim's Theory of Anomie" (*American Journal of Sociology* 80 [September 1974]: 329-63). On the plus side, he deserves thanks for turning our attention once again to the importance of anomie in all of Durkheim's works. It is good that Marks has attempted to elucidate the complex interrelationships of Durkheim's abiding concern with anomie with the rest of his sociology—political, educational, religious, and so on. A sympathetic reading might even dispose us to grant Marks some of his theses—for example, that he has discovered a potentially troublesome hiatus between Durkheim's micro- and macrosociological approaches to resolving the problem of anomie and the "moral anarchy" of the modern world.

However, on the debit side, Marks's interpretive exegesis is flawed by certain key omissions. For example, simply in terms of anomie theory, the lack of explicit and systematic definition of anomie, omission of Durkheim's crucial doctrine of the dualism of human nature, failure to treat Durkheim's entire suicide typology in evolutionary perspective—these and other fatal omissions seriously impair the value of Marks's contribution. However, perhaps the most basic flaw in his interpretation, as in so many others, is a fundamental lack of clear recognition of what might be called the "paradigmatic" or "nuclear structure" of Durkheim's lifework. By their very nature, partial readings or understandings are bound to mislead. What we need are stratigraphies, not more topographies.

Marks considers Durkheim's classic *Suicide* "hopelessly confused." Could it be that the confusion lies in the eye of the beholder? In short, if Durkheim and anomie are to be claimed as paradigmatic for sociology, then how is it possible that we still struggle so to bring his basic paradigms into clear focus?

Let us briefly review the essential structure of Marks's discussion. Then I shall show how his argument is irretrievably altered by recognition of the paradigmatic facts. One might abstract Marks's argument as follows:

A hiatus between Durkheim's micro- and macrosociological analyses of anomie has been discovered. Focusing on Durkheim's program to "engineer the crisis of anomie out of existence" (p. 330), this paper shows that Durkheim attempted successively four different solutions. The key to each solution concerns what structural base generates what kind and level of normative consensus. On the microlevel, Durkheim first tried the occupational solution. The problem of anomie was then shifted to the macrolevel, where the "bold problem that Durkheim . . . now set for himself" (p. 338) was how to generate a "macro-nomos," "a societal self-consciousness." On the macrosocietal level, Durkheim explored two indirect "gatekeeper" solutions in terms of normative mediation by political and educational elites. Finally, Durkheim suggests the direct macrosocietal solution of "charismatic" mass movements. Ultimately, however, all these theoretical solutions to the problem of anomie failed on their own terms. It is concluded that Durkheim's attempts were doomed to failure, in any case, since anomie inevitably characterizes any large group.

Marks's analysis exhibits many problems. In terms of his apparent inability to recognize the significance of Durkheim's paradigmatic or nuclear structure in relation to his theory of anomie, the key flaws include: (a) slighting of Durkheim's crucial doctrine of the presocial individual as inherently egoistic and insatiable; (b) slighting of Durkheim's central explanatory model linking superstructural (cultural) collectively representational processes to substructural social morphological processes; (c) slighting of Durkheim's genetic-evolutionary perspective; (d) slighting of Durkheim's developing concern with civilizational (or intersocietal) processes; (e) slighting of Durkheim's central concern, on the world-historical level, with universalizing, autonomizing, and rationalizing intercultural processes. If one attempts to address any part of Durkheim's sociology, especially his notion of anomie, without reference to these and other crucial anchors of his system, one runs the risk of seriously distorting the true meaning and significance of anomie. This is precisely the limitation of all topographic exegeses (see McCloskey 1974).

I cannot claim sufficient space for a detailed page-by-page disputation with Marks. Contrasting a systematic stratigraphy of Durkheim's nuclear structure with Marks's misrendering of anomie, I simply lodge the following objections. First, Marks's title is misleading: the real focus of his paper is Durkheim's changing program to "engineer the crisis of anomie

out of existence" (p. 330), as Marks puts it. Apparently, Marks's underlying purpose is a critique of attempts at planned social change, evaluated from a micro or social psychological perspective. Yet, if the specific problem he addresses is the modern crisis of anomie, what is the nature of that crisis? What does anomie mean, precisely? The reader searches Marks's essay in vain for explicit and systematic definition of this key word. The closest he comes are such tautological references as "anomie is the situation in which . . . normative boundaries are thrown awry" (p. 333). Is anomie synonymous with "normlessness?" And what does "normlessness" mean, anyway? Is anomie equivalent to anarchy, to the so-called Hobbesian dilemma? Does anomie refer to a psychological, a structural, or a cultural condition? Or to all three? What relations do such notions have with Durkheim's original meanings and intentions? Marks is sadly mistaken if he neglected these crucial definitional problems because he presumed consensus concerning anomie. The simple truth of the matter is that Durkheim's original meanings have been routinely distorted as social scientists have alternately lambasted him and scrambled to lay claim to his still vibrant "charisma-on-deposit" (see McCloskey 1974). Perhaps from his own point of view, Marks was wise to ignore these problems, for a close examination of anomie theory and literature might well open up a "can of worms." But I repeat: If Marks wishes to discuss this topic, he should first define what he means by anomie; second, he should show what he thinks Durkheim meant; third, he must legitimize his claims concerning Durkheim's charisma.

Second, Marks attempts to address Durkheim's theory of anomie in isolation from the nuclear structure of the latter's work. For example, he completely ignores Durkheim's doctrine of the dualism of human nature ("homo duplex," see Wolff 1964). Yet without this absolutely crucial distinction between ego and person and the characterization of the pre-socialized ego as inherently egotistic, passionate, and even insatiable, Durkheim's theory of anomie and the need for constant moral discipline makes little sense. The image of "homo duplex" lies at the very foundation of Durkheim's sociology of morality, knowledge, and religion. Indeed, Marks generally slights Durkheim's overriding concern with anomie—namely, with insatiability, with "the infinity of dreams and desires" that underlies modern "moral anarchy." Whether these destructive drives derive from the historical release of the ego from traditional controls or whether they are anchored in modern cultural sanctions of absolute individualism and drives for unending "progress and perfection," the fact remains that Durkheim's empirical point of departure was the modern "infinity sickness."

Third, Marks does what almost everyone else has presumed to do: he isolates Durkheim's anomie (and egoism, to a certain extent) from the



rest of the typology of suicide. I must insist that Durkheim chose his terms carefully; they meant, at least initially, what they clearly connote. These terms can only be understood relationally; thus, altruism and egoism are clearly defined as opposites, as are anomie and fatalism. Seen in historical perspective, as crosscutting sets, altruism and fatalism, anomie and egoism, represent key differences in the relations between the individual and culture in primitive and modern societies. Indeed, Durkheim's perspective was always genetic and evolutionary—there is simply no way of adequately understanding what anomie and egoism meant initially except in terms of the breakdown of traditional moral controls over the ego. In short, no insatiability and no transforming historical process, no anomie and no egoism!

Along with many others, Marks generally slights the crucial evolutionary framework of Durkheim's thought. For example, looking only at the end of *The Division of Labor*, which is concerned with anomie, Marks concludes that Durkheim's basic "focus is on microsocial processes, on deficiencies of social interaction" (p. 331). But it is Marks, not Durkheim, who insists on the exclusive virtue of social interactional paradigms. With regard to a sociological classic based on the macroevolutionary passage from "mechanical" to "organic solidarity," can Marks seriously ask us to believe that Durkheim's perspective was basically ahistorical and microsocial psychological? Or again, at the end of his essay, Marks contends that "in *Elementary Forms*, the problem of social solidarity is now the same in both simple and complex societies" (p. 354). But this proposition is simply not true! Apparently, Marks has succumbed to the functionalists' persistent misrendering of Durkheim's genetic-evolutionary theories as solely abstract, ahistorical, generic propositions. Contrary to the implications of Marks's statement, Durkheim never urged a return to the repressiveness of the primitive, fused, "sacral-magical collective conscience." Contrary to popular misperception, Durkheim's central problem was not the so-called Hobbesian dilemma—he cannot legitimately be portrayed as an abstracted, formal theorist searching for the generic bases of social order and control. As Robin Horton (1973), Benjamin Nelson (1973), Anthony Giddens (1971a, 1971b, 1972a, 1972b), and Lawrence Krader (1968), among others have recognized, as even Parsons (1973) and Bellah (1959, 1973) have come to recognize, Durkheim's key theses in *Primitive Classification* and *The Elementary Forms* concern the genesis (out of the primitive sacral complex, the womb of society and culture) and evolution of increasingly universal and rational cultural forms and the autonomizing of the person.

Fourth, Marks's dating of Durkheim's breakthrough to a macrosocietal focus on anomie is similarly misconceived. Durkheim intertwined concerns with micro- and macrosociological processes throughout his sociology,

from *The Division of Labor* to *The Elementary Forms*. Contrary to prejudicial portrayals of him as a platonizing or hypostatizing social realist, the central rule of the Durkheimian school was always to anchor analysis of social phenomena in a "geographically determinable substratum." That is, collectively representational (cultural) processes were always linked with substructural social morphological processes. Seen in genetic-evolutionary perspective, this meant that as the underlying social morphological substratum differentiated, so too did collective representations become—in more or less direct correspondence—increasingly abstract, universal, and rational. Indeed, as their seminal "Note on the Notion of Civilization" ([1913] 1971) reveals, Durkheim and Mauss struggled with the implicit societal limitations of their foundation theorem. They realized they must devise terms and strategies for analyzing the intersocietal, intercultural horizons of complex sociocultural life.

However, Marks' tacit commitment to a social psychological or micro-interactional framework apparently barred recognition of these deeper dimensions of Durkheimian sociology. Marks's micro commitment permeates his paper and is summed up in the following underlying premise: "Nomos . . . must always have some microsocial nexus" (p. 336). In regard to Durkheim's "program" to "engineer . . . anomie out of existence," the operationalized corollary of Marks's general principle is: "The elimination of anomie requires direct interpersonal relationships" (p. 353). Now, although the terms "nomos" and "nomic" are strewn like code words throughout Marks's essay, nowhere does he define them or their importance. Are "normative," "nomic," and "cultural" synonymous? Does "nomos" refer to norms, to values, or perhaps to world images, or to all three? Given this nomic focus, how is it possible that Marks slights so consistently the central theoretical perspective of culture? Must all normative processes have their origins and continuing structural bases in direct, face-to-face social interaction? Armed only with such a small-meshed net, how can we hope to achieve systematic insight into macro-level social phenomena such as cultural processes on the societal level, cultural traditions on the intersocietal level, even civilizations—in short, all those crucial intersocietal, intertemporal, interspatial matrices of complex sociocultural life? Surely we cannot hope to understand the contemporary era, much less the meaning of anomie and the modern crisis of "moral anarchy," if we continue to allow such intellectual or cultural prejudices to restrict fatally our commitment to understanding sociocultural reality on all levels of complexity. Had Marks read Durkheim's works more carefully, especially the neglected "Note on the Notion of Civilization," written with Mauss, he might have relinquished his position, realizing that Durkheim and Mauss struggled with and overcame the limitations of precisely that position. In truth, Marks's tacit nominalis-

tic commitments, which at one point he implies allow "American sociologists to better retain their ideological commitment to individualism" (p. 353 n.), not only distorts Durkheim but also arbitrarily cut off from view some of the most crucial objectives of the sociological task. Indeed, was not the raising of our sociological sights to wider, more profound horizons one of the most profound contributions of both Weber and Durkheim?

Similar problems are revealed in Marks's invidious comparison of Durkheim with G. H. Mead. Siding with the latter's social interactional focuses, Marks argues that "only when the prospect of a societal consciousness is abandoned in favor of a more individual-focused 'social process'" (p. 353) will it be possible for theorists to deal adequately with such "indirect" interactional paradigms as reference group theory. But, in relation to Durkheim, the truth of the matter is that, from *The Division of Labor* to *The Elementary Forms*, Durkheim was fundamentally concerned with the relations between micro- and macrosociological processes involved in the progressive passage to ever-wider "universes of discourse." Nowhere is this seen more clearly than in Durkheim's seminal thesis, developed later by Mauss in his profound lecture "A Category of the Human Spirit: The Person" ([1938] 1968), that the evolving autonomy of the person and the progressive universalization and rationalization of the "universe of moral discourse" proceed together on the world-historical level (see Leites 1974; Krader 1968; Nelson 1973). Apparent ignorance of Durkheim's model intertwining structural, cultural, and psychological processes in evolutionary perspective, his intimate intertwining of micro- and macrosociocultural processes, his growing concern with civilizational, universalizing, and rationalizing processes, his seminal thesis of the evolutionary emergence of the person, and so on, lead Marks and others fundamentally astray.

Finally, quite apart from the limitations of his discussion of Durkheim's theory of anomie, Marks's account is seriously deficient in that it so persistently ignores the relevant secondary literature. The existence of well-known discussions of Durkheim's anomie and schematic interpretations of his typology of suicide in Dohrenwend (1959), B. Johnson (1965), Giddens (1966), Mawson (1970), and Cresswell (1972), to mention only a few, is not even acknowledged. At least Marks might have indicated his debt to or disagreement with these discussions. Further, the vast literature on anomie is simply ignored. No mention is made of the best review of Durkheim's educational sociology (Ottaway 1955) nor of the best review of Durkheim's political sociology (Giddens, 1971c). In terms of serious discussions of Durkheim's basic commitments, Marks neither cites nor even refers to the pioneering analyses of Parsons (1937), Alpert (1939), and Foskett (1939) of the previous generation. Nor does he acknowledge

the wealth of major Durkheim scholarship published since 1971, including Giddens (1971a), LaCapra (1972), Wallwork (1972), Poggi (1972), Lukes (1973), and Bellah (1973). Even though he cites Giddens's (1972a) excellent introduction to Durkheim, he seems to have little fathomed its messages, which are very close to those I have presented here. What justifies Marks's splendid isolation from the sociological literature? How are we to explain this apparent lack of compelling scholarly obligation?

In sum, I invite Marks to reexamine his theses in the light of both the relevant sociological literature and Durkheim's paradigmatic or nuclear structure. May I suggest, once again, that we need no more summary recapitulations of the main theses of Durkheim's major works, no more topographies. We urgently need systematic stratigraphic inquiries into Durkheim's core commitments. Perhaps if he invests as much energy in these tasks as he has more or less profitably expended in elucidating what he takes to be Durkheim's changing theory of anomie, Marks may yet help lead us in new and valuable directions.

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## DURKHEIM'S THEORIES OF ANOMIE RECONSIDERED—A REPLY TO McCLOSKEY

I welcome David McCloskey's provocative critique, and while I must honor the tradition of finding grievous fault with one's critics, I am nevertheless grateful for having been given the opportunity to rethink a number of important issues. Much of McCloskey's critique is devoted to knocking down assorted straw men, and thus some brief disclaimers are in order:

1. Nothing I said even faintly implies "a critique of attempts at planned social change." Durkheim's ultimate remedy for anomie was to change everyone's consciousness to a livelier humanism. He reasoned that, although this humanistic individualism itself could not be engineered, it was possible to engineer various structural changes that would conduce to its spontaneous emergence. And my statement (p. 357) that Durkheim's later notion of "periods of creation or renewal" failed to address the prospect of deliberately implementing social change did not imply any general critique of planned social change on either my part or Durkheim's.

2. Nowhere did I insist "on the exclusive virtue of social interactional paradigms," nor did I ever imply that this was Durkheim's exclusive perspective. I would argue, however, that one cannot come to grips adequately with Durkheim's theories of social pathology without bringing in some sort of social interaction paradigm at some point in the analysis. I made no pretense of writing an exhaustive analysis of all of Durkheim's ideas, many of which do not entail any interaction paradigm. McCloskey often uses my paper as a springboard to launch into his concern with some macrocultural phenomena, which, despite their importance, have nothing to do with Durkheim's theory of anomie.

3. Similarly, it is not reasonable to extract a fragment in which I sought to show Durkheim's concern at the end of *The Division of Labor* with certain social interactional deficiencies in modern industry and then hold this up as evidence that I "ask us to believe that Durkheim's *perspective* [in that work] was basically ahistorical and microsocial psychological" (my emphasis).

4. When I said that "in *Elementary Forms*, the problem of social solidarity is now the same in both simple and complex societies" (p. 354), I immediately clarified this statement as meaning that all societies need ways of actively reaffirming their beliefs and values. To say that Durkheim held this conviction is not to claim his "theories as solely abstract, ahistorical, generic propositions." Durkheim's theories were not "solely" anything, but they were often generic propositions, such as the three "elements" of morality in *Moral Education*. I fail to see how, in singling out the generic proposition about the need for reaffirmations of belief, I implied that Durkheim "urged a return to the repressiveness of the primitive, fused, 'sacral-magical collective conscience'" (McCloskey's statement here is doubly misleading in that Durkheim did urge a return of modernity to a kind of sacral collective conscience, but to one that would be coupled with modern rationality and industrialism, not to the "primitive" and "repressive" one portrayed in *The Division of Labor*; on this point, see LaCapra 1972, pp. 90-91, 242).

5. I did not make any invidious comparison of Durkheim and Mead. My point was that one who is fundamentally committed to societies prefers Durkheim, while one who is more committed to social processes prefers Mead. I hasten to add that neither of these theorists was exclusively committed to either approach.

Another line of criticism concerns my lack of precise definition of the concepts of "anomie" and "nomos" (which I treat as opposites of each other). But as Jack Douglas points out (1967, p. 71), Durkheim did not provide clear and distinct definitions of many of his theoretical concepts, and egoism and anomie are among them. My strategy was to show the implicit connotations of these terms in the larger contexts of the works

in which they appear or are implied, particularly because the sense kept shifting with Durkheim's own concerns. By the same token, it is not a sensitive criticism to claim that my paper was not about anomie but about Durkheim's changing program to eliminate it, because Durkheim continued to alter and clarify his "diagnosis" in the process of setting forth his various therapies. For the record, Durkheim's final view was that anomie refers (1) to the failure of a properly societal morality to appear in the form of a set of integrative, humanistic "collective representations" and simultaneously (2) to a paucity of societally focused social interaction that is sustained enough to generate these societal representations (in my view the earlier notion of "egoism" is incorporated into this second element).

Note my claim that the above is Durkheim's "final" view of anomie. I should stress that while this societal view of anomie is the most important one in the light of Durkheim's works after *Suicide* (and the one that has been almost completely ignored), it is not his only theory of anomie. McCloskey argues that what egoism and anomie meant "at least initially" is perfectly clear to him and Durkheim but peculiarly unclear to me. But Durkheim has rarely been applauded for his consistency, and what things meant to him "initially" could scarcely have met his own criteria for "social facts"! Accordingly, my article would have been more appropriately entitled "Durkheim's Theories of Anomie." These different theories are not mutually exclusive but are tied to a complex analysis of the concrete features of social life at all the important levels of social organization in the modern world. McCloskey fails to recognize this. He thinks that Durkheim's notions of egoism and anomie can be explained simply in terms of "the breakdown of traditional moral controls over the ego." This position is misleading because it is faithful to only one of Durkheim's theories of anomie, the one based on the notion of "homo duplex," and as Lukes points out (1972, p. 23), Durkheim did not and could not consistently maintain this notion (see also LaCapra 1972, p. 231). Moreover, McCloskey's position here is far more abstract than Durkheim's; it makes anomie into an either/or state of social life and thereby keeps us from specifying which of Durkheim's theories is being considered. Durkheim almost always treated anomie as a pathology of social relations within particular social organizations. He wrote not of anomie in the abstract but, for example, of an anomic division of labor, and he further specified this particular theory of anomie as applying only to certain occupational social organizations and not to others. He worked out specific theories of egoism/anomie for marital, familial, religious, and occupational social organizations, and when his thinking later converged on a macrolevel notion of societal anomie, he was conceiving of society not as an abstract, over-generalized level of social order but as a very determinate, specific,

organized human grouping having its own peculiar qualities of social relations.

Durkheim's basic recognition, then, was that individuals are simultaneously members of different social organizations. It follows that "anomia," as a characteristic of individuals, would not have been seen as something that individuals always carry around with them; instead, it must refer to the nature of the individual's relations in some specifiable social unit, for example, a family, an occupation, or a society. An individual may be anomic in his family or society but eunomic in his religion, and to recognize this (as Durkheim often did) is to abandon the oversimplified distinction between a "presocialized," anomic "ego" and a socialized, eunomic "person." We can therefore speak of "an anomic individual" in abstraction from this or that social context only if we have already determined that his relations are anomic in *all* of the social organizations to which he belongs.<sup>1</sup>

Finally, McCloskey would like me to relate my analysis to other interpretations of Durkheim. Although it would require a whole book to satisfy this demand adequately, I will make a few regrettably sketchy remarks about some of the more recent literature. They can best be prefaced by saying that the key to my own interpretation is my insistence that when we follow the theory of egoism/anomie from *Suicide* to *Elementary Forms*, we find that it involves both a social interactional and a cultural moral pathology. Durkheim's basic analytical distinction between an egoistic lack of attachment to groups and an anomic lack of normative regulation cannot be found empirically, at least not beyond the very short run. Stated positively, sustained patterns of social interaction in any social unit cannot fail to generate an appropriate cultural-moral order for that social unit. Stated negatively, without regular social interaction, no cultural moral system can long prevail: egoism necessarily gives rise to anomie.<sup>2</sup> In this respect there are two interpretations with which I feel completely comfortable—Barclay Johnson's (1965) and Alvin Gouldner's (1958, pp. 25–27); while I never consciously tried to follow Gouldner's

<sup>1</sup> Durkheim would have rejected Leo Srole's (1956) anomia scale because it treats individual attitudes in abstraction from the multiple social contexts within which they are formed and maintained. To ask people whether they agree that "life is meaningless" is not reasonable, regardless of their willingness to answer the question, whereas it is acceptable to ask them whether their family life or occupational life is meaningless. Robert Merton, whose earlier work on anomie tended also to see it in abstraction from specific social contexts, later brought his ideas much more in line with Durkheim's (see Merton 1964, pp. 228–29).

<sup>2</sup> I am aware of Durkheim's claim that collective representations, once formed, become "partially autonomous realities with their own way of life" (1953, p. 31; see also 1965, p. 493). I would simply argue that this claim is part of Durkheim's cultural evolutionism and is always bracketed whenever he shifts his focus to social pathology.



program for analyzing Durkheim, in retrospect I find that my entire analysis is incisively foreshadowed in these three programmatic pages.

Many of my reactions to other interpretations follow from the above remarks. While I admire Jack Douglas's (1967) analysis of *Suicide* for its sheer analytical brilliance, I cannot accept his attempt to pin Durkheim down to a fundamentally cultural explanation of suicide. For Douglas (pp. 41-45), suicide must be caused either by meanings or by social behavior, so he logically sees "a high degree of vacillation [in *Suicide*] over just what the fundamental theory is" (p. 45). For me, Durkheim is not vacillating at all but simply shifting his focus back and forth between different analytical elements of a single nomic process.<sup>3</sup>

Similarly, I cannot accept A. R. Mawson's attempt (1970) to distinguish between egoism and anomie. Mawson sees anomie as "weakened legal, economic [or] moral norms" (p. 310) and egoism as "psychological isolation" from close personal relationships, resulting in deficient self-concepts and meanings (pp. 306-7). For Mawson, then, anomic individuals who are "frustrated in their desires, are not [necessarily] isolated psychologically from each other" (p. 306). But this statement can be made only by abstracting the theory of egoism/anomie from its linkage with specific levels of social organization. For Durkheim, individuals suffering from economic anomie necessarily suffer psychological isolation from enduring economic close relationships (hence the need for corporate groups), although this does not mean they are psychologically isolated within other units of social organization, nor does it mean they have no close personal ties even within the economic unit. A group of anomic workers may be psychologically close to one another and develop some group moral norms and attendant self-concepts, but then their economic anomie must involve a lack of close relationships within the *larger* economic unit (e.g., to management and/or to individuals performing more distant functions in the division of labor). Similarly, sexually anomic individuals could not, for Durkheim, have close marital relationships, though they might have close personal ties within some other unit of social organization. Finally, individuals may have close relationships within marriages, within families, and at all levels of some economic unit, and they may thus be protected from anomie within each of these social units, but they may still suffer

<sup>3</sup> A major difficulty of Douglas's analysis is that he fails to do what he himself claims is essential: to consider "*Suicide* in the context of Durkheim's *total* sociological work" (1967, pp. 33-34; my emphasis). Not only does he fail to look at Durkheim's works after *Suicide*, but also his intensive analysis covers only the middle sections of the book. Thus his statement (p. 62) that Durkheim's macrocultural analysis in *Suicide* "is really a carry-over from the *Division of Labor*," and that *Suicide* "is almost entirely concerned with . . . the micro-cultural level," could not have been made so readily had he given careful consideration to the "Practical Consequences" conclusion of *Suicide* and had he followed that conclusion into Durkheim's later works and lectures.

from political anomie if they have no enduring relationships with people simply as citizens.

Let me return briefly to Durkheim's "final" theory of anomie, the one focusing on the societal level of social organization (see above). The key to my interpretation here, again, is that Durkheim sees both a social interactional and a cultural moral pathology. Other interpretations generally see the cultural moral basis of the societal level already solidly established in the "cult of the individual" (the most explicit recognition of Durkheim's dismay about the failure of humanistic individualism is provided by Johnson 1965, pp. 885-86). LaCapra, who neatly sums up Durkheim's conviction that society was going through "not death throes" "but prolonged and disruptive birth pangs" (1972, p. 24), fully understands that by the time of *Elementary Forms*, Durkheim's appreciation of structural processes was more keen than ever (see 1972, p. 86). And virtually alone among recent interpreters, LaCapra—through harnessing the concepts of "community" and "communitas"—rightly stresses Durkheim's continuing interest in microsocial processes and their pathological absence at the societal level (see pp. 86, 88, 117, 208-11, 217, 226, 242). Yet even though he recognizes this structural pathology and acknowledges the importance of the cult of the individual for ensuring a healthy modernity (pp. 74-75, 232), LaCapra does not make it clear that for Durkheim this cultural moral element was also pathologically absent from the consciousness of average people.

Similarly, Anthony Giddens writes (1972, p. 10) that in Durkheim's eyes organic solidarity "is emergent rather than fully actualized. The modern world is still in a transitional phase." But again, like LaCapra, Giddens focuses only on the structural side of societal anomie (see p. 42) and implies that the cultural moral element of society is, for Durkheim, already in good order (see pp. 21-24; see also Steven Lukes [1972, pp. 166-67, 339-44], who seems to imply the same thing).

Turning to the work of Ernest Wallwork (1972), I find a somewhat more complex analysis. With Gouldner, he recognizes Durkheim's "assumption that moral norms are generated and sustained by stable patterns of interaction" (1972, p. 87). But Wallwork somehow thinks that Durkheim did not follow this principle when it came to the moral norms on the societal level, those having to do with humanistic individualism. Like LaCapra and Giddens, he treats the "cult of man" as having simply come down to us through the ages, being now fully institutionalized and having no need of microsocial reaffirmation. Wallwork does provide an excellent analysis of the "social foundations of morality" within families and occupational groups. But while he promises (pp. 87-88) to do likewise for the state and for educational institutions—for Durkheim, the two main institutions that could mediate "society" to average people—he

considers abstractly only the cultural moral side of "civic morality" and of education (see chap. 5); he says almost nothing about Durkheim's elaborate concern with the dynamic structural bases of these institutions. To be sure, these omissions are faithful to Wallwork's unfortunate conviction that Durkheim shifted his earlier concern with attachment to actual groups to a concern with collectively shared values (see pp. 104, 129). They do not square, however, with Durkheim's belief that at the societal level, such shared values were missing along with the microsocial processes that would generate and sustain them.

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#### COMMENT ON HAWKES'S "NORMS, DEVIANCE, AND SOCIAL CONTROL"

Hawkes's mathematical treatment of norms, deviance, and social control (*AJS* 80 [January 1975]: 886-908) is an important paper. Hawkes seeks to clarify and spell out the implications of a basic idea in sociology, that of the deviance-controlling feedback system. The importance of the article lies in the generality of its possible application. It is not merely a contribution to the areas of deviance and mathematical sociology: all substantive

areas of sociology are concerned with the social control of behavior. However, I come neither to bury Hawkes nor to praise him, but to inquire into some puzzling points in his article. Were the article not of general interest, I would not bother, for I have no special interest in mathematics or deviance.

The points I find puzzling are contained in the section of the paper on the amount of deviance and the extent of regulation. These points stem partly from the assumption that the variables—norms, deviance, behavior, and social control—have means of zero and that there are no constant additive effects. Hawkes assumes that the same units are used in measuring norms and behavior, and he defines the deviance variable as norms minus behavior. This definition seems consistent with standard usage. Yet, in defining the amount of deviance in a group or situation, Hawkes selects a definition which in no way relates deviance to norms, departing from standard usage and his previous definition of the deviance variable.

In looking at the notion of amount of deviance in a group or situation, Hawkes examines various possible ways of defining the idea. First, he rejects the average value of deviance over the set of cases because this definition is "not responsive to the substantive questions posed by the inquiry" (p. 901). A second possible definition, the correlation of norms and behavior, is rejected because "other things being equal, the correlation of norms and behavior is the same whether or not the . . . negative feedback loop exists" (p. 901). Finally, the variance of behavior is taken as a proper definition of deviance—that is, the more behavior varies across time or space or among people, the more deviance there is (p. 902).

Given that Hawkes's treatment of deviance assumes that norms do vary (p. 900), it is curious that he should equate a low variation of behavior with a low amount of deviance. This definition diverges radically from his definition of deviance as a variable and from standard use of the term. Certainly, if norms prescribe different behavior in different situations and if a person's behavior is homogeneous across these situations, he cannot be conforming in all of them. Norms about drinking in middle-class America, for example, direct us to drink at cocktail parties in the evening in the company of others but not to drink alone or in the morning. Both the teetotaler at the cocktail party and the chronic drinker are deviant, although each has a low variance in drinking behavior across situations. The person who drinks only when the situation is appropriate, and therefore has varied behavior, does not deviate from the norms. Since deviance is not referenced to norms in Hawkes's formulation, his notion of the amount of deviance is not in accord with standard usage.

In rummaging through the discarded definitions, I find that I must agree with Hawkes in rejecting the second one, the correlation of norms and behavior, but for different reasons. One assumption underlying

Hawkes's analysis is that the means of all variables are "zero and that there are no additive constant effects" (p. 894). Suppose, however, that in the real world the means are not zero and that there are additive constant effects. More specifically, suppose that in any group or situation there is some difference between norms and average behavior but that the size of the difference is not necessarily constant. In regressing behavior on norms, one would find that the intercept of the regression line was not zero, and the average behavior would not be equal to the norms. On the other hand, one may find that the slope of the regression line is unequal to one. If the slope is less than one, then as norms increase in value, behavior does not increase so much. As norms in different situations prescribe more and more extreme behavior, deviance, or the difference between norms and behavior, increases.

In short, Hawkes properly rejects the random scatter about the regression line of behavior on norms (i.e., the correlation) as the definition of the amount of deviance. Excluded from this definition are the slope and the intercept in the regression of behavior on norms, which together can indicate the extent to which average behavior departs from norms.

It might be useful for some purposes to think of three components of the amount of deviance in terms of the regression model—namely, the intercept, the departure of the slope from a value of one, and the random scatter of behavior left over.<sup>1</sup> This conception does not exclude systematic deviance as long as relationships are linear. In addition, it can be shown to be equivalent to a single concept of the overall amount of deviance for which Hawkes searches.

It seems to me that the elusive definition of the amount of deviance was there all along in the article, briefly mentioned as "some average of  $D$ [eviance] over sets of cases" (p. 901). It is of course easy to define an average amount of deviance as follows:

$$\text{amount of deviance} = \sqrt{\frac{\sum (N_i - B_i)^2}{n}},$$

where  $N_i$  and  $B_i$  denote paired values of norms and behavior, and  $n$  denotes the number of observations. In this definition, the amount of deviance is simply the "standard deviation" of behavior from norms rather than from the mean of behavior. This definition is more in line with ordinary usage than is Hawkes's notion of the variance of behavior.

<sup>1</sup> While this formulation makes intuitive sense when the intercept and the slope are considered separately, its meaning becomes clouded when both are considered simultaneously. This is so particularly if the slope of behavior on norms is less than one and the intercept is positive. Hawkes indicates that, given only the direction (but not the magnitude) of relationships posited in the model, the slope could conceivably be negative. I recognize some of the problems involved, but I use this line of thinking to clarify the ways in which Hawkes's model ignores some deviance from norms.

In addition, this average deviance can be broken down into the tripartite conception of deviance discussed above. For any given value of norms, behavior will be equal to the intercept, plus the product of norms and slope, plus the difference between observed behavior and its estimate. If this clause replaces  $B_i$  in the previous equation, then

$$\text{amount of deviance} = \sqrt{\frac{\sum (N_i - \beta_0 - \beta_1 N_i - \epsilon B_i)^2}{n}},$$

where  $\beta_0$  and  $\beta_1$  denote the intercept and slope of behavior on norms.

With this formulation the amount of deviance is clearly a comparison of measured behavior in different situations with what would have occurred if behavior coincided precisely with norms. In other words, the measure indicates how large a discrepancy there is between what the regression line would be if there were perfect conformity and the actual regression line (plus some dispersion of observations from the line). The regression line for perfect conformity would have an intercept of zero and a slope of one.

Since Hawkes's discussion of the extent of social control rests on his definition of deviance, another problem naturally emerges. After defining deviance as the variance of behavior, he defines the extent of regulation as the ratio of this variance of behavior in the absence of the feedback link, or the square of the reciprocal of the determinant. The reciprocal of the determinant can also be derived from the ratio either of the slopes or of the covariances of norms and behavior with and without the feedback loop (p. 904).

The preceding discussion of the regression of behavior on norms is applicable here also. If the extent of control is measured as the ratio of the slopes or covariances of norms and behavior with and without the feedback loop, the matter of intercepts is ignored, and the extent of departure of behavior from norms under the two sets of conditions is unclear.<sup>2</sup> However, suppose for a moment that the intercepts in both models are zero. It is possible that the slope of behavior and norms without feedback is near one, indicating that the average change in behavior per unit of change in norms is equal. In other words, the average behavior in different situations matches the norms. The only deviance (i.e., norms minus behavior) is the random scatter about the regression line. If, when the feedback loop is included, the slope of the line becomes less than one, the average behavior in different situations is no longer identical to the norms. The regulatory mechanism has caused a difference between norms and average behavior. This difference becomes greater in absolute magnitude as the value of

<sup>2</sup> My training in mathematics and statistics is inadequate for me to determine whether the exclusion of the feedback loop will affect the intercept of norms and behavior.

norms increases or decreases from zero. In other words, under these conditions the regulatory mechanism makes behavior both more homogeneous and more deviant, if we think about deviance in the usual sense of the departure of behavior from norms.

While I think that Hawkes's discussion of the amount of deviance and extent of control is to some extent misleading, it does contain numerous insights. In addition to those pointed out by Hawkes, there are insights into the question of why behavior and group structure seem to vary less than norms. In the area of formal organizations, it is often believed that groups with bureaucratic ideals tend to develop egalitarian informal structures. Groups with democratic ideals; on the other hand, tend to develop oligarchic informal structures. When the variation of ideal and actual, or informal, structures is compared, there is seemingly less variance among actual structures. Perhaps the same is also true for ideal and actual patterns of family power structure, household composition, kin ties, and systems of mate selection. For all of these variables, there is some comparative evidence suggesting that actual patterns vary less than the ideal ones. Part of this discrepancy which seems to appear in various contexts may be due to the negative feedback of behavior on control. As Hawkes points out, such feedback insulates behavior from norms and other extraneous causes, reducing its variance. In structural terms, then, the effect of the regulatory social control mechanism is to make actual patterns somewhat more constant across cases and somewhat less responsive to the ideals. In behavioral terms this implies paradoxically that in some circumstances the normal operation of the regulatory social control mechanism is one of the causes of deviation from norms.

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#### ORDINAL-SCALED VARIABLES AND MULTIVARIATE ANALYSIS: COMMENT ON HAWKES<sup>1</sup>

Given the generally low levels of measurement used in the social sciences, the possession of a powerful set of multivariate methods might seem to

<sup>1</sup> The ideas developed in this comment arose partly out of various problems I encountered while acting as a consultant to the Department of Community Health, Medical School, University of Nottingham, Nottingham, England. That work was supported by Research Contract HSM110-70-393 from the Health Services and Mental Health Administration, Rockville, Maryland.

be somewhat superfluous. What is the sense, the iconoclast and others ask, in using these methods, which presume the highest level of measurement and (usually) multivariate normal distributions, with "soft" data? An appealing answer has been proposed by Hawkes (*AJS* 76 [March 1971]: 908-26), who suggests calculating rank measures of association (Kendall's  $\tau_b$ ) and using these instead of ordinary (Pearson) product-moment correlations in the multivariate methods. Although this method is appealing, it is invalid even if it is assumed that the observed variables are related to unobservable variables of a multivariate normal distribution.

In more detail: given an observed (ordinal?) variable,  $u$ , we can perhaps regard it as some monotonic (order-preserving) function of an underlying variable,  $x$ , where  $x$  is interval scaled and normally distributed. For two such observed variables,  $u_1$  and  $u_2$ , Hawkes calculates  $\tau_b$ , and (because  $u_1$  and  $u_2$  are order-preserving functions of  $x_1$  and  $x_2$ ) the value of this coefficient is the same as the value that would be calculated on the unobservable variables  $x_1$  and  $x_2$ . If we were to calculate a product-moment correlation on the bivariate normal variables  $x_1$  and  $x_2$  (a justifiable procedure), its size would be related to the size of  $\tau_b$  for the same set of values. If  $t$  is our sample value of  $\tau_b$  calculated on  $u_1$  and  $u_2$ , it is the same as that between  $x_1$  and  $x_2$ . Our estimate of the product-moment correlation between  $x_1$  and  $x_2$  ( $r$ ) is given by

$$r = \sin \left[ \frac{\pi}{2} t \right]$$

(Kendall 1970, chap. 9). These  $r$ 's are the values that should be used in the multivariate analysis, not the  $t$ 's.

It might be suggested that, although the preceding statements are true, the use of  $t$ 's will not affect inferences about the nature of a partial relationship. A simple example will suffice to show how inferences can be drastically altered. Suppose that  $r_{12} = r_{23} = .75$  and  $r_{13} = .5625$ , implying that  $r_{13.2} = 0$  (because  $r_{12}r_{23} = r_{13}$ ). Corresponding to these  $r$ 's,  $t_{12} = t_{23} = .56$  and  $t_{13} = .38$ , and obviously  $t_{12}t_{23} \neq 0$  (actually  $t_{13.2} = .141$ ). This characteristic of  $r$ 's and  $t$ 's explains the results of Reynolds's (1973, p. 1514) simulations, in which he consistently finds that the partial  $\tau$ 's corresponding to zero partial (Pearson) correlations are much greater than zero. Hawkes (1973, pp. 1516-17), in his reply to Reynolds, suggests that the latter's results are due to measurement error. Reynolds himself proffers no explanation.

The characteristics of the above approach need study. My own investigations so far, using simulations, have led me to believe that the  $\tau_b$  conversion is fairly accurate for ungrouped and grouped data provided the grouping is not too strange (e.g., over 50% in an end category). Tables



have been prepared to facilitate the conversion of  $t$  into  $r$  and vice versa; they are available from me.<sup>2</sup>

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<sup>2</sup> NOTE ADDED IN PROOF.—Because the reply to this comment was received too late to be printed here, it will appear in a forthcoming issue.—Ed.

#### COMMENT ON "PARACHUTING" BY GIDEON ARAN

Gideon Aran's article, "Parachuting" (*AJS* 80 [July 1974]: 124-52), reveals at least as much about the style and state of contemporary sociology as it does about parachuting. For that reason, the comment which follows is concerned far less with Aran than with the discipline in which he finds himself newly involved.

There is little question that Gideon Aran has experienced a great deal concerning the subject of which he writes. Moreover, he has reflected on that experience and has studied and contemplated the related professional writings of others. Given that, Aran's article offers a good deal of insight into the human activity of parachuting. Thus the problem he encounters is less his than sociology's. For, in essence, a concrete experience and its human significance, whatever that may be, suffers as it is forced into certain analytical categories apparently deemed sociologically meaningful at one of this nation's leading universities.<sup>1</sup> The result is a product which one senses is less than Aran could contribute in fully human terms on the subject of parachuting and, more important, engages in a brand of pseudo-sociology that detracts from a conceptual precision quite essential to the vitality of the sociological discipline.

To specify, Aran begins well. At the outset he indicates that in parachuting (as in human existence generally), psychological components can be fully understood only within a social context. "This context includes

<sup>1</sup> Aran states that his article "is a revised version of part of [his] M.A. thesis submitted to the Department of Sociology, University of Chicago, summer 1972" (p. 124).

elements of the jump situation (the state in the aircraft), the jumper's primary group (the company of paratroopers), the institution to which he belongs (the army), and the broader social and cultural milieus" (p. 125). He then adds that "the introduction of these social dimensions into the study of parachuting is indispensable for understanding it." Absolutely correct—and sociology has long and often been helpful in this regard in righting the sociopsychic balance.

The difficulty, however, comes very soon and is central. As Aran states in his abstract, the "bipolarity of parachuting provides a rare opportunity to study a nearly ideal-typical manifestation of extreme opposite forms contained within an organizational setting" (p. 124). The case for bipolarity is presented substantively as follows: "Within a few moments [as paratroopers jump], the highly integrated collectivity that has dominated its individual members (prejump phase) changes drastically into a tenuous, anomic social situation that gives rise to a very egocentric individuality followed by a return to the former state (postjump phase)" (p. 124). As is later stated more graphically, "parachutists jump into [a state of] anomie" (p. 135).

The fact is that they do no such thing. If one wished to indicate, as Aran does elsewhere with a good deal of sense, that the individual experiences an exhilarating feeling of independence and freedom as he floats to earth, that is one thing. But that this same individual is also simultaneously conscious of himself as paratrooper (status), that he perhaps feels a little more courageous than others, that he may believe that he is a part of an elite military unit, and so forth is also very much to the point. In fact, as a paratrooper suspended in midair he may well have thoughts about his girl friend or potential girl friends, or he may possibly vaguely or quickly contemplate his father's or mother's pride in his accomplishment or have numerous other such personally and socially meaningful thoughts closely related to his status as paratrooper and his own self-conception.

"Jump into [a state of] anomie" (normlessness)? Hardly. Apparently it seems sociologically appropriate to say so and to build a thesis on this point, but, beyond the fact that Durkheim's concept is misemployed in this instance, Charles H. Cooley's point that human beings are very fundamentally human because social values and norms are internalized within them cannot be so easily disregarded.

One wonders whether Aran would care to apply his analysis to the activities of a lone mountain climber, white-water canoeist, or cross-country skier. One would hope not, since it is quite clear that all such pursuits, even though engaged in by lone individuals, have a social content—that social values and norms have been integrated and are ever present within the minds of individual participants. What then makes the

parachutist so different? That his act is more dramatic? Perhaps. But that alone would scarcely seem good reason to suspend all our powers of critical reasoning and analytical judgment concerning the fundamental relationship between individual and society.

Aran is on far sounder ground as he concludes his article. After reiterating his central point in the first sentence of the following, he comes far closer to stating the essential sociopsychic relationship involved: "It is interesting to note that an act which is so much associated with individualistic and even somewhat anarchistic tendencies [the anomic allegation] can be related to the opposite elements. Military parachuting, notably in Israel, is not an act of rebellion but rather of identification with the central symbols of the society, the voluntary attempt to integrate oneself into the established institutions. It is perceived as a sort of service to the society and the state. In a way, parachuting is an act of heroic chauvinism" (p. 151). This concluding statement is far more to the point and very far from a state of anomie.

Clearly Aran knows something about parachuting, as much of what he communicates clearly indicates. Yet, the title of his article aside, that is not the issue here. Sound analysis of the relationship of the individual within society and the social within the individual surely is. Indeed, it would seem the central sociological task. That being so, the utilization of inappropriate sociological terminology leading to faulty analysis not only detracts from a significant human experience (parachuting) but also blurs the conceptual clarity fundamental to an important sociological endeavor.

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## Review Essay

*Production de la société.* By Alain Touraine. Paris: Éditions du Seuil, 1973. Pp. 543.

Randall Collins

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Alain Touraine is noted for a number of works on industrial organization, industrial society, the Third World, and recently on the May uprising of 1968 and on postindustrial society. In *Production de la société*, he has produced his general systematic theory.

This work is, in fact, comparable in many ways to Talcott Parsons's *The Social System*. Touraine has assimilated American sociology as of approximately the late 1950s, and he weaves a critique of Parsons, Rostow, Merton, and others into his sparring against more recent French intellectual factions. Touraine's stance is, on one side, a thoroughgoing conflict position. He defines society as the product of action—of productive work—while attacking the position of Althusser and Bourdieu as representing society merely as the reproduction of a dominant order, without equal emphasis on the conflictually creative production of society by itself. Touraine rejects as well any sociology of values, arguing that these pertain only to individuals, not to a system, and that they are ideologies of the dominant class. But he is equally opposed to individualistic or phenomenological sociologies; these he declares utopian because they ignore the system of domination that determines conflicts, movements, and ideologies. Thus, although he attacks functionalism for its idealizations, its ethnocentrism, and its obliviousness to class conflict, he regards the notion of system as its positive contribution: for society is a system producing the orientations in individuals that make up its functioning.

The basic elements in Touraine's explanatory apparatus are *historicity* (the techniques of production of all aspects of society, including its operations on the material environment), *the system of historical action* (the complex of cultural orientations through which historicity operates), and *class relations* (the conflicts between dominating and dominated groups over control of the system of historical action [S.H.A.]). The crucial determining variations are under the rubric of historicity. Its basic components are a type of knowledge, an associated system of accumulation, and the resulting cultural model of each type of society. The greater the accumulation, the greater the distance between the production and the reproduction of labor (here the influence of Althusser's fashionable reading of Marx becomes apparent) and hence the greater the capacity of the society to act upon itself. Touraine distinguishes accordingly four ideal types of societies: (1) ones in which the units of production coincide with those of consumption, (2) ones with separate means of exchange, (3) ones

organized around industrial capital, and (4) ones organized around the specialized production of scientific knowledge. The first are agrarian societies, in which the cultural system and its conflicts center on religion; the second are merchant societies (like those of the European Renaissance), whose culture and conflicts revolve around the state (and around such *étatiste* doctrines as those of Machiavelli and Bodin); the third are industrial societies, whose culture and conflicts center on economic progress; the fourth are postindustrial or programmed societies, which turn consciously on the production of themselves and hence have a center of conflict which is self-consciously cultural. Touraine recognizes all four as ideal types rather than evolutionary stages and points out that any specific historical society may be a mixture of types.

If Touraine's emphasis on the primacy of this cultural level of analysis bears a resemblance to Parsons's, there is a further resemblance in the way the analysis is elaborated. For Touraine goes on to cross classify a whole series of abstract dichotomies—orientations versus resources, culture versus society, movement versus order, and others—giving names to the boxes of the resulting grids and thereby typologizing every subject as it comes along, whether it be types of sociological analysis, the component elements of the S.H.A., types of societies, class relations, or political situations. Touraine rather pointedly distinguishes himself formally from Parsons by labeling half the slots in his boxes as counterelements to the others, representing the divisions of class conflict. Furthermore, different kinds of social crises are described according to which axes of the basic grid of eight boxes might be ruptured.

Touraine's analysis of class relations is also fundamentally cultural. The dominant class is that which controls the S.H.A., and its ideology is its claim to speak on behalf of it, as the productive and progressive technical agent of that era. The popular class, in turn, has two faces, one negatively defensive against the ideology in the name of which it is dominated, the other positively contesting control of the S.H.A. and thus producing its own ideology and its own utopia. Touraine sees classes not as directly observable entities but as ideal types. He recognizes the existence of conflicting interests among ethnic, local, national, age, family, sex, religious, and other groups; but he recognizes them not in order to incorporate them into an explanatory model but only to set them neatly to the side of his theory of class conflict. Much of this theory turns out to be more cross classification (of such distinctions as dominating class versus directing class, the defensive and contestatory orientations of the popular class, etc.), but occasionally more concrete propositions emerge. For example, there is the hypothesis that the more heterogeneous a society is in its mixture of historical types, the more the upper class dominates instead of directing, the more it uses mechanisms of class reproduction as weapons of control, and the more class conflict is directed toward political or organizational levels instead of ideological confrontations over the core culture itself.

Touraine considers other levels of analysis as well: those of institutions,

organizations, social movements, and social change. The chapter on institutions is devoted primarily to the state, which he is careful to argue has relative autonomy, being neither completely reducible to class relations nor completely independent of them; he goes on to cross classify varying degrees of these with various class situations. In the chapter on organizations, Touraine uses a typology borrowed (but with a new terminology) from Etzioni's coercive, remunerative, and normative control types. Here the levels of abstraction take on a reality of their own, as the author argues that on the organizational level *per se*, roles are based on norms accepted as legitimate because of a strictly technical determination; this is not to say that class conflict is absent from the factory but that it belongs to a more inclusive level of analysis, that of the S.H.A. In the same vein, Touraine asserts that power, which by its very nature divides people into dominators and dominated, is not on the technical level of organizations but part of the more general conflict system.

Social movements, similarly, are distinguished from social change: the former are class conflicts over control of the S.H.A., while the latter is the result of yet another overlay of forces, which may include external forces like war, conquest, and colonization, or mutation in the basic cultural techniques. Touraine is at pains to dissociate himself from evolutionism of both Marxist and non-Marxist varieties. Societies of different types not only exist simultaneously but also, through conquest and colonization, incorporate elements of each other, a process that fosters interdependently different modes of change: not only does underdevelopment depend on advanced development, but also vice versa, and the revolutionary ruptures of a situation of extreme heterogeneity in underdeveloped societies occur in symbiosis with the smoother accretions of technical modification in advanced ones. But if this argument resonates with the dependency theories of the Left, Touraine is nevertheless highly critical of the Marxist tradition that the dominated class of one era is the agent of transformation into the next. Every society has its own classes, its own conflicts, and hence (by Touraine's definition) its own social movements; as part of each system, classes, conflicts, and movements have no resolutions but merely disappear to make for a new system of conflict when change does come about. Touraine wisely remarks that one should not confuse the conflict between dominant and dominated classes with that between the directing elites of old and new technical systems. Nor should one be led astray by ideological claims of particular classes to represent the future: both dominant and dominated classes carry on two-way ideological battles, each claiming to represent the best ideals of the past and the coming possibilities of the future.

Touraine, in fact, has virtually stood the Marxian scheme of class relations on its head. For him, it is not the dominated class of each era, the workers, who make the productive contribution, but the dominant class. In postindustrial society, it is the technicians, the scientific innovators, who control the core of the S.H.A. Here Touraine speaks the language of the Saint-Simonian tradition; he departs from it into an unidealized

realism by recognizing that every form of domination, even a productive one, nevertheless generates conflict with the have-nots. In the post-industrial type, the have-not class is not the workers but those who are excluded entirely: the youthful and the aged, who are outside the realm of work. Economic issues are nowhere part of Touraine's scheme. Like Durkheim, he asserts the absolute autonomy of social explanation from any metasocial forces; what takes their place is a pure cultural conflict, in which the cultural system of every era always offers the possibilities of rival interpretations, and the possession of this cultural instrument of production ensures that there will be forces attached to a positive and a negative side.

Touraine's is indeed a general systematic theory, but I would not call it a very successful one in explanatory terms. He has produced a realistic and nonutopian left-wing functionalism, cleverly playing off against each other the philosophical debates of the French intelligentsia and the concept-classifying techniques of Parsonianism. Neither of these styles has come very near to actual causal explanations; it is perhaps a measure of sociology's progress that Touraine should be able to push through at least occasionally into suggesting some conditions under which empirical differences actually do occur. Much of what he says about political conflicts and social movements in different types of societies moves in a promising direction through the conceptual haze.

But there is also a fundamental weakness in the substantive scheme itself. Touraine relies on an unexamined faith that what are called "techniques of production" are rightly named; from this follows the re-definition of class struggle in terms of the dominating possessors versus the dominated nonpossessors of productive power and the identification of social movements and their debates with classes struggling over the productive techniques. But recent empirical sociology, I would say, is becoming more skeptical about so-called technological skills and more attuned to the actual maneuvers over the control of ideas and emotions that make up so much of the power of professionals, technicians, and other "experts." It is probably because student movements (especially French ones) of the 1960s attacked the domination of technology that Touraine infers that a postindustrial type of domination really exists; one need not take so seriously the rhetoric either of the protesters or of the mouthpieces of authority. In a sense, then, for all his conflict-oriented realism, Touraine falls into yet another variety of the old sociological pitfall—taking ideologies for more than they really are.

## Review Essay

*Conflict Sociology: Toward an Explanatory Science.* By Randall Collins. New York: Academic Press, 1975. Pp. x+584. \$22.50.

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This is an important book that will provide food for discussion in the sociological community for a long time to come. It not only contributes to general sociological theory but also opens new vistas in such diverse areas as the study of organizations, stratification, sex roles, and micro-sociology. It makes a major contribution to the analysis of the interrelation between the political and the economic order. Yet it is also a seriously flawed work: time and again the author's ambitious reach exceeds his grasp. I shall first discuss Collins's overall orientation and then deal with some, though by no means all, of his substantive contributions.

What is attempted here is nothing less than a reconstruction of sociological theory. Confining functionalism to the dustbin of history, Randall Collins wishes to erect the framework of a new "conflict sociology," building on the work of Max Weber in sociology and on the utilitarian philosophy of Thomas Hobbes, with major assistance from as diverse a congeries of scholars as K. Marx, É. Durkheim, H. Garfinkel, E. Goffman, R. Dahrendorf, and S. Freud.

This curiously diversified pedigree already indicates some of the difficulties of the enterprise. How, for example, is one to reconcile the utilitarian premise that "everyone pursues his best line of advantage according to resources available to him and to his competitors" (p. 89) with the stress on the Durkheimian anti-utilitarian notion of ritual as the cement of social solidarity? How is one to adhere to what Collins calls "Weber's nominalistic conflict approach to stratification and organization" (p. 43) and at the same time use major portions of the holistic thinking of scholars working in the Durkheimian tradition? If human beings are essentially self-interested maximizers of their private satisfaction, how can one explain the noncoercive aspects of social order unless, as Collins does largely under the guise of "ritual," he smuggles in the Durkheimian and Parsonian emphasis on common values and norms that he has knocked out in the first round?

The author wishes to put an end to what he considers the undue reifications in most previous sociological theorizing and to focus attention on "real people in real places" instead of "organizations," "classes," "communities," "roles," and "systems" (p. 7). So far so good, but, in fact, major chapters deal with such abstractions as are indicated by the titles "A Conflict Theory of Organizations" (chap. 6), "State, Economy, and



Ideology" (chap. 7), and "Wealth and Social Mobility" (chap. 8); also, subchapters are devoted to "external organizational roles" (chap. 9.1) and "types of societies" (chap. 4.1).

Once he has left the microsociology of self-maximizing and self-enhancing individual actors behind, Collins is forced  *nolens volens*  to deal with social structures, reified as they may be. At one point he argues that if one conceives of stratification as a pyramid, a ladder, or a hierarchy of geological layers, one misses the mark, since "this is not what human society  *looks like* . What it looks like . . . is nothing more than people in houses, buildings, automobiles, streets. . . . No one has ever seen anything human that looks like a ladder or a pyramid" (p. 51). But, of course, no ordinary citizen has ever seen atoms or molecules either, and natural science got off the ground only when it gave up thinking about fire, water, or air and turned instead to a concern with such "unseen" entities as chemical elements in their various combinations. In concrete fact, to be sure, Collins is by no means a scientific Luddite; so he blithely continues talking about unseen entities throughout his work, being unable, just like anybody else, to do without analytical and conceptual entities. He states programmatically that "what old-fashioned sociologists used to call 'norms' are nothing more than whatever points of agreement there exist in and around conversations" (p. 146), yet he can also say with approval that "what Kuhn refers to as a paradigm is not merely an intellectual entity, a set of ideas, but is apparently both based on a social consensus and maintains an organized set of social relationships" (p. 493). Through this curious alchemy, the old-fashioned notion of norms becomes acceptable once it is rebaptized as a newfangled paradigm.

Collins wishes to challenge "the sentimental notion that social behavior is inherently meaningful (i.e., based either on linguistic categories of thought or on an orderly nonverbal social structure beneath them)." Instead, "meaning becomes something that each man tries to impose upon the world" (p. 111). Yet the rest of the book talks of meaning structures such as "deference culture" and "youth culture," and a whole chapter is entitled "The Organization of the Intellectual World." He who wishes to slay a dragon had better do a thorough job of it; the beast has many lives and its teeth may sprout in the very backyard of the professed dragon slayer.

Because I do not wish to harp further on the many conceptual and philosophical ambiguities with which this volume unfortunately abounds, I now turn to its substantive content. Wishing to lay the foundations of an explanatory science, eschewing practical, ideological, and aesthetic concerns, the author offers what he considers an integrated conflict theory of sociology. The theory is premised on the notion that "human beings are sociable but conflict-prone animals" who engage in conflict because "violent coercion is always a potential resource, and it is a zero-sum sort" (p. 59). This organizing idea, with which I have no desire to quarrel, allows the author to advance a series of propositions that provide much-needed correctives to received wisdom in sociological theo-

ricing. For example, he rightly challenges the widely prevalent market model of stratification, according to which people are variously distributed throughout the social structure in terms of their "functional contributions" and marketable skills. Collins show that stratification involves power contentions among individuals or groups variously striving to maximize their life chances and not just individual movements in relatively unchanging structures. People have different power resources, he argues persuasively, depending on their location in the class and occupational order, and they bring these to bear in a social struggle for an optimum place in the social hierarchy that ceases only in death. "Life is basically a struggle for status in which no one can afford to be oblivious to the power of others around him" (p. 60). Human beings attempt perpetually to coerce others so as to maximize their advantages. But they also "strongly resist being coerced" (p. 281). Hence, conflict is central to all human interaction. "Experiences of giving and taking orders are the main determinants of individual outlooks and behaviors" (p. 73).

I am prepared to go a long way with Collins along this road, but I must part with his company when his emphasis on the conflictful elements in human interaction threatens, as in some of the quotations above, to become a pan-conflict imperialism. This threat becomes especially apparent in the chapter on stratification by age and sex. One may be grateful to the author for attacking the textbook versions of marital adjustment and companionate families and yet remain skeptical when he attempts to argue that, in effect, men and women, as well as parents and children, are natural enemies. To be sure, as Collins is not the first to point out, the more intimate the relationships between human beings the higher the chances of conflict between them, but to ignore the patent fact that men and women, as well as parents and their offspring, need each other and that their wants and desires are to some extent complementary results in a portrait that is, in fact, a caricature. Collins postulates, for example, that "human males are usually larger and stronger than females" and derives from this the proposition that "in the absence of other resources, the members of the larger sex are the sexual aggressors and control the scheduling of sexual activities, and members of the smaller sex attempt to avoid sexual access to avoid being coerced" (pp. 281-82). One hardly needs to be a Freudian to consider that in passages such as this—and they abound—Collins is a *terrible simplificateur*.

No doubt, in the chapter on stratification by age and sex the author attempted to present a truthful picture of the human condition, warts and all, but he seems to end up with a portrait that is all warts. Damn it all, human beings also love each other, sacrifice for each other, and defer to one another's feelings and sentiments. In Collins's bleak world nobody would ever dream of donating a kidney to save the life of a loved one—unless, of course, doing so would "enhance his status."

The chapter entitled "A Conflict Theory of Organizations" is not open to similar criticism. It is, in fact, exemplary. Collins here pulls together a variety of materials from different studies of organizations to emerge

with a highly integrated and fully convincing general theory, based largely on Weber but with major assists from such students as A. Etzioni, A. Stinchcombe, P. Selznick, M. Dalton, and J. D. Thompson, that is likely to become must reading for students in that area. Here the emphasis on conflict, balanced as it is by a complementary emphasis on the sources of compliance, bears important analytical fruit.

The succeeding chapter, "State, Economy, and Ideology," is also excellent. The subchapter on military organization and technology especially, though derived largely from Weber and ignoring the important work of S. Andreski, shows a very firm grasp of historical developments and a fine analytical sense of the complex interrelations among the development of the technology of violence, its economic base, and the strategies of its practitioners. The rest of the chapter, on the relation between the economy and the state, based partly on the work of G. Lenski and B. Moore, is equally illuminating though not as elaborate.

My comments on the remaining chapters of the book will be brief. "Microsociology and Stratification" attempts to argue at the hand of certain Darwinian notions and the work of Goffman and the ethnomethodologists that "life can be seen as a series of ongoing negotiations" (p. 114) and focuses attention on conversational interchanges and their effects, as well as on the symbolic counters that are used in such interactions. "Wealth and Social Mobility" argues persuasively against a market model of the economy and the opportunity structure, proposing instead a model that stresses the preemption and monopolization of economic resources in the hands of elite groupings, as well as the "power positions that bound the market" (p. 438) and the way in which "careers take place within an ongoing struggle of cultural groups to control positions by imposing their standards of selection" (p. 452). The last chapter, "The Organization of the Intellectual World," contrary to its title, deals almost exclusively with the sociology of science. It is largely based on the work of J. Ben-David, with whom the author has collaborated in the past. It strikes me as a very competent summary of what has been done in the area. Though it starts from the unorthodox proposition that "ideas about reality are weapons in the struggle for dominance over channels of communications, and can be explained by the interests of individuals with the resources to uphold them" (p. 520), it seems to me in practice not to depart widely from what we have already come to learn in this area.

This book covers most areas of social life in which such resources as power, wealth, and high status are scarce, so that conflict or competition between users or potential users of these resources must stand in the forefront of analytical attention. But it neglects those areas where scarcity is not a factor. In the religious order, or in the universe of nationalism, success of some does not depend on the failure of others. My quest for salvation is not affected in principle by that of my neighbor. Had Collins paid more attention to these noncompetitive spheres, he would have gained access to an understanding of integrative forces in the social order that are

now a closed book for him. One can only wish that he would return to reading his beloved Weber.

The author has decided to put the gist of what he had to say in the form of a series of propositions and to show how his general principles relate logically to the more particular statements. This is commendable indeed. He argues correctly that the absence of such codification among the classical authors is one of the reasons for the lack of cumulative development in sociological theory. Why Collins calls these propositions "causal" I do not know, since, as the few examples below will show, many are not causal at all. More important, these propositions are not of equal worth. Some are valuable in that they direct attention to further research, others are vacuous and tautological, and many, alas, are just wrong. The following examples of each category have been selected at random.

*Valuable leads to research:* "The intensity of commitment to an ideology depends on the social density of interaction and the level of physical threat" (p. 379). "The more people one gives orders to and the more unconditional the obedience demanded, the more dignified and controlled one's verbal behavior" (p. 157).

*Vacuous and tautological statements:* "The closer people live to each other, and the more their lines of movement cross, the more likely they are to talk" (p. 156). "The more an army is rewarded by *assigning conquered land to individuals*, the more likely the territorial regime is to be feudal decentralization" (p. 361). Feudal decentralization is largely defined by assignment of conquered lands to individual warriors.

*Not-so statements:* "The more one receives orders in the name of political, religious, organizational, or moral ideals, the more one is alienated from these ideals" (p. 157). Tell this to old Bolsheviks or members of the Society of Jesus. "Each individual values highest what he is best at" (p. 73). Reference group theory and the abundant research inspired by it should not be ignored by an author who is so much concerned with cumulation in sociology. In religion, for example, laymen invariably value highest what they are not best at, namely, the activities of practitioners of sacred rites and of "religious athletes" of various kinds. And psychologically we are all familiar with people selecting activities that they are not good at.

That this review has been couched largely in a mode of dissent does not mean that the reviewer is ungrateful to the author. This work is without doubt one of the most important books in sociological theory of the past decade. It belongs on the shelf of anybody who is concerned with the development of our discipline. It has, as I have tried to show, a number of serious flaws, but to encounter it among all the dreary and humdrum offerings that come down the pike also gives a sense of relief, for this book prods one forward with the sting of novelty and the shock of recognition—even as it also makes him very angry.

## Book Reviews

*Sociology: A Multiple Paradigm Science.* By George Ritzer. Boston: Allyn & Bacon, 1975. Pp. vi+234. \$7.95.

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In a few bold strokes, George Ritzer offers us a paradigm for sociological paradigms. He suggests that "most sociologists are unaware of the nature of the basic differences in the field" (p. 210) and, building on the work of Kuhn and Masterman, argues that sociology should be seen as a multi-paradigm science. Three basic paradigms or, to oversimplify Ritzer's usage, fundamental images of the subject matter are identified as currently struggling for hegemony in the discipline and its subfields: the social facts, the social definition, and the social behavior paradigms.

The bulk of the book is devoted to delineating these paradigms in terms of their four principal components, that is, (1) fundamental image of the subject matter, (2) exemplars, (3) theories, and (4) methods. Here is a brief summary.

*The social facts paradigm.*—(1) macroscopic social structures and institutions or external social constraints such as norms, values, and social control agencies; (2) the work of Durkheim and of Warriner; (3) structural functionalism, conflict theory, and systems analysis; and (4) interviews and questionnaires.

*The social definition paradigm.*—(1) relatively microlevel intra- and intersubjective phenomena; (2) the aspects of Weber's work that emphasized social action and interpretive understanding; (3) action theory, symbolic interactionism, and phenomenology; and (4) observation, particularly participant observation.

*The social behavior paradigm.*—(1) human behavior or responses as controlled by external stimuli or reinforcement; (2) Skinner's work; (3) behavioral sociology and exchange theory; and (4) field and laboratory experiments.

In effect, Ritzer is suggesting that theoretical systems that have generally been seen as distinct or opposed have fundamentally more in common than has heretofore been appreciated, that those commonalities rest on their fundamental images of the subject matter of sociology, and that the field can be seen as basically divided into three warring camps.

Several of the more interesting implications that Ritzer draws out of this perspective are that (a) behaviorism is actually a major perspective in contemporary sociology, (b) structural functionalism and conflict theory share many more commonalities than differences and do not constitute the basic split in sociology, (c) there are more intimate paradigmatic linkages between theory and method than is commonly appreciated, and (d) there is considerable irrationality in the doing of sociology and coun-

terproductive political conflict in the struggle among paradigms. He feels that, on balance, the negative consequences of this struggle (e.g., the inhibition of doing normal science, identifying anomalies, and making revolutionary breakthroughs) now outweigh the positive, and urges concentration on more rational scientific discourse. Finally, although he suggests that some of the great sociological thinkers (Durkheim, Weber, Marx, and Parsons) should be seen primarily as paradigm bridgers (especially of the facts and definition paradigms) and that adequate explanation of social phenomena requires the use of all sociological paradigms, he remains pessimistic about the prospects of paradigm reconciliation in the near future, especially given the commitment by many sociologists to the victory of their own paradigms.

On balance, I think this is a useful book that should promote the examination of basic assumptions in our discipline. Ritzer's general scheme of paradigms strikes me as not unreasonable (especially since it complements my own). His treatment of the various paradigms, while not particularly sophisticated, is a relatively clear, concise, and fair overview that should be particularly useful for advanced undergraduate and beginning graduate students. Further, his insights about the nature and implications of sociology as a multiple-paradigm science seem sound.

Still, there are a number of unresolved problems in Ritzer's analysis that need more careful attention. The most basic problems concern his claims (a) that the distinguishing feature of a paradigm should be its fundamental image of the subject matter and (b) that the most important aspect of a given theory's fundamental image is captured using the threefold scheme outlined above. (Ritzer does acknowledge the possibility of other paradigms such as biologism and critical theory—thus verging on the ad hoc proliferation of paradigms that he criticizes.)

I do not find either of these crucial claims well justified. Regarding the first claim (a), Kuhn's "Postscript" to his revised edition of *The Structure of Scientific Revolutions* suggests that exemplars constitute the central feature of paradigms. While I am more inclined to share Ritzer's position on this point, clearly it needs more careful consideration than Ritzer gives it. As for his second claim (b), I do not think Ritzer has shown adequately that he has selected the most crucial aspects of the basic image. Theories that Ritzer classifies in different paradigms share certain basic premises, and those classified together do not share certain ones. It is not made clear why some aspects of the theories are not as (or more) important than those on which he focuses. Thus, it seems that the criteria for deciding which are the basic aspects of the images of a given theory, and thus for classifying the theories, are not sufficiently explicated, justified, and operationalized.

Finally, related points needing attention include (i) developing the analysis of paradigmatic exemplars; (ii) probing more deeply into the philosophical roots and metatheoretical aspects of the paradigms, particularly, drawing out their deeper ontological and epistemological premises; (iii) developing an interpretation of why paradigms evolve that avoids

too empiricist an emphasis on anomalies or too structuralist an emphasis on political conflicts; (iv) treating Marx and Marxism more extensively; (v) clarifying the bases of important intraparadigm differences and conflicts and attending to whether there are systematic within-paradigm differences across the paradigms; (vi) grounding the conceptualization of the differences among the paradigms in a way that would be more analytical and comprehensive and which would suggest what other paradigms there are or could be; and (vii) correcting some glaring omissions that lead to dubious judgments of some theories (e.g., the fruitful work of Bellah, Smelser, Mayhew, etc. in action theory, or Althusser, Poulantzas, Freire, and Gramsci in Marxist theory).

I trust that as Ritzer's work evolves he will have further useful insights and positions for us to consider.

*Ideology and Social Knowledge.* By Harold J. Bershady. New York: John Wiley & Sons, 1973. Pp. 178. \$9.25.

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On the basis of a conscientious and detailed review of Talcott Parsons's action theory, Harold Bershady has written an important, precisely argued, and lucid essay examining the epistemological foundations and logical structures of social knowledge. The work is one of the most searching—and sympathetic—examinations to date of Talcott Parsons's theorizing. But it is much more than that. Bershady wisely chose Parsons's work as the most advanced and comprehensive effort at constructing a fundamental frame of reference and body of theory which recent sociology had to offer, in order to investigate the epistemological status and adequacy of explanatory structures in social knowledge generally. This is no textbookish exposition, but a subtle analysis of structures and strategies in Parsonian theorizing which is of major importance in its own right. At the same time, readers will find in it a reliable, albeit selective, guide to Parsons's work, focused upon its strategic core in the action frame of reference and the logical structure of explanations. Many of the more specific contributions of Parsons are thus, quite appropriately for the purpose at hand, mentioned only briefly or excluded.

As the title *Ideology and Social Knowledge* signals (unfortunately, not without some ambiguities), Bershady's intent is to study the foundational issues of social knowledge in order to help establish a firm and nonrelativistic basis upon which to separate social knowledge sharply from ideological relativisms or special claims.

The argument that the basic work on epistemology and explanatory structures for social knowledge still needs to be done is essentially correct. The status of social knowledge is peculiar, at least in the sense that social theory is about theory-containing systems. Social knowledge explains the conduct of actors who themselves theorize (and who, further, can and do

learn the theories about them). This is a point clearly seen by Simmel, who seeks to find "sociological a priori." For Simmel, society is a special object because it consists of conscious and "synthesizing" units. There are, of course, important differences in emphasis between Simmel and Parsons, but "they are nevertheless rather close, epistemologically, and it may be stated once again, that for Parsons as for Simmel, the categories underlying a theoretical system are a priori, and they are in fact held to be necessary to organize our knowledge" (p. 80).

Parsons's solution to the philosophical and scientific problems of theory building in sociology is the construction of a comprehensive, and universal, frame of reference from which all forms of social life can be mapped. ". . . Parsons sees *his* action scheme to be *analogous* to the space-time framework of physics which is 'necessary' for the comprehension of every physical phenomenon—precisely as Kant claimed it to be. . . . And it is Parsons' framework which (in action sociology) makes it possible to talk about social processes" (p. 68). Bershadsky assesses the a-prioristic claims of Parsons with considerable skepticism:

For Parsons wishes to incorporate and transform every narrow view into a single over-arching conception which will allow us to behold, eventually, every variation of society that can exist. There remains a huge gap between his intention and his achievement. And whether his intention can ever be fulfilled is a subject best left to sooth-sayers. But there is no doubt that there is a constancy in his work that does not come solely from his interest in the "problem of social order," or the conditions of societal equilibrium. Parsons has remained true to the epistemological vision expressed in his first major work. [P. 150]

Bershadsky argues that the explication of distinctly social-scientific modes of explanation may have become possible on the basis of Parsonian theoretical arguments: the clear recognition of modes of explanation often follows their *use* rather than precedes it:

When we look beyond his programmatic statements to his substantive work another picture emerges. He is concerned with determinations, but they are no longer "causal." He does not seek the "pre-determinations" of social life. He does not try to tell us what must be but what must have been. We have seen in his pervasive employment of necessary reasoning that Parsons seeks "post-determinations" of social activity, and the quasi-teleological elements in his thinking are thus most prominent. Meanings, normative regulations, conceptions of the desirable are among the chief premises in his attempt to explain social action. . . ." [P. 164]

The logical structure of such nomic rather than nomological emphases is as yet insufficiently clear, but it is here that Bershadsky sees hope for progress. In this sense his work can be considered prolegomenous to a logic of sociological explanation.

The argument of the essay proceeds from an examination of the issues of historicism and relativism, through a consideration of the first epistemological problems Parsons encounters in his early work and the prob-



lematics of ideal types, to the exposition of his first solution—the means-ends schema (which includes the categories of means, ends, conditions, and norms). The metaphysics of the position is examined; Bershadsky then analyzes the later formulations of Parsons's central scheme contained in the functional "solution" and in that for the study of large-scale societies and their evolution. The essay concludes with a brilliant final chapter, "On Unification of Social Knowledge," in which Bershadsky sketches the logic of practical arguments for the explanation of action and offers his judicious view that social knowledge makes its progress through rational criticism, rather than universal and exhaustive schemes as such.

Bershadsky's sophisticated and searching essay does not end with definite conclusions. One wonders which direction his analysis will now take. The task of inquiring into the foundational structures of social knowledge does require specialized and sustained work; one can only hope that Bershadsky will continue. The critical analysis and reconstruction of explanatory strategies in social science should not only be applied to the work of Parsons, important though it is. Possibly Bershadsky could extend his analytical skill to the study of alternative frames of reference in social inquiry, their cognitive strategies and yield. It is time to turn to the systematization and appraisal of logic in social inquiry; Bershadsky has demonstrated great skill and made a contribution that lets us hope for more.

*Trying Sociology.* By Kurt H. Wolff. New York: John Wiley & Sons, 1974. Pp. vii+662. \$16.95.

Thomas F. Gieryn

*Columbia University*

Kurt Wolff's *billet-doux* to American sociology is a collection of 31 essays written during the past four decades. Widely known for his palpable introductions to Simmel and Mannheim, Wolff now provides a spectrum of his own scholarly interests. Some readers will come away from the book perplexed at not having grasped the essential Kurt Wolff. The essays defy ready systematization; the author is both polymath and stranger.

Evidence for Wolff's polymathic character is the broad range of sociological interests which he "tries." The collection opens with Kurt Wolff, the historian of social thought, providing insightful essays on Pareto, Simmel, and Durkheim. As social researcher, Wolff reports empirical studies of the Jewish community in Texas during the 1940s. Keeping a sociologically informed eye on the academy, he thoughtfully compares intellectuals and cognitive styles in Germany and in the United States. As an overseer of the sociological enterprise, Wolff critically examines the philosophical groundings of the discipline, while suggesting areas for new inquiry, such as the study of social meanings of evil. The anthropologist Kurt Wolff provides sensitive accounts of problems of data collection in his New Mexican fieldwork. The volume concludes with Kurt Wolff, the sociologist of knowledge, urging integration of *Wissenssoziologie* with all

sociological research and theorizing. For over 40 years Wolff has moved freely from subject to subject, guided less by disciplinary paradigms than by a personal, often idiosyncratic, sense of the important problem.

It is perhaps this polymathic quality which makes Wolff reminiscent of Simmel's "stranger": no single culture or sociological specialty can claim him exclusively as its own. He is a member of the "sea-change" generation and, like many another European émigré, is able to combine remoteness from his adopted American culture with a particularly penetrating nearness. An essay entitled "A Memorandum on the United States of America" (1960) is filled with aperçus on the distinctively American brand of "common sense, decency, and optimism" of a kind that recall observations by the European social commentator Walter Benjamin.

Simmel's "stranger" is not "radically committed to the unique ingredients and peculiar tendencies" of a group. Wolff's detachment from mainstream American sociology, coupled with his broad range of interests, has enabled him to anticipate developments which became popular years after he considered them. It was principally Alfred Schutz, of course, who built the bridge between phenomenology and sociology, traveled since by many more sociologists than philosophers. But as early as the 1950s Kurt Wolff was convinced of the large promise of a phenomenologically informed sociology. His idea of the social scientist "surrendering" himself to the phenomenon as it presents itself, not coniforming it to received doctrine, resembles Husserl's phenomenological "bracketing."

As a student of Mannheim, Wolff was concerned from the start with imprints left by sociologists' tacit assumptions on their "objective" accounts. He reminded us during the 1940s and 1950s of the long tradition of "reflexive" inquiry in sociology and philosophy which gained widespread popularity in the 1970s with works by Gouldner and Friedrichs. Wolff also linked up with early "critical" sociology by urging, in this case, anthropologists to discard their cultural relativism when confronted by people living below standards of human dignity who might benefit from resources fieldworkers could provide. It is disappointing that these anticipations remain intimations rather than explorations. Nevertheless, the ideas from 20 and 30 years ago, though sometimes obscured by cumbersome language, do remind us that phenomenological and reflexive sociologies, to say nothing of "critical" sociology, did not first emerge in the late 1960s.

Simmel's "stranger" is not tied down in his action by "habit, piety, and precedent" and thus is well prepared to criticize his adopted group. "Iconoclast" may be more suitable than "stranger" to describe Kurt Wolff the critic. He tells us candidly, even proudly, that his biography of Simmel was commissioned but rejected by editors of the *International Encyclopedia of the Social Sciences*, and wears this decision as a badge of his independence from the standards of conventional sociology. Wolff battles the "neopositivists," an aggregate he would have us believe dominated American sociology in the 1940s. The battle lines are first drawn in a 1946 essay entitled "Toward a Sociocultural Interpretation of American

Sociology," but are still evident in an essay on "radical anthropology" written in 1973. Neopositivist sociology is described as failing to consider subjective attitudes of actors under investigation, avoiding research suggested primarily by pragmatic concerns such as the improvement of the quality of human life, and unable to examine social events in their proper historical context. Neither, it is said, do neopositivists scrutinize their own methods, and so they are not able to discover the hidden premises which figure so importantly in their findings. Wolff offers precious few names linking these ideas with their advocates. He may be battling windmills, for it is the rare sociologist such as Lundberg or Bain, over the last few decades, who might embrace the neopositivism portrayed by Wolff. If Wolff's criticisms appear tired to us now, it is because they are battles which many have waged incessantly for more than three decades.

A final word on the volume's curiously ambiguous title. Wolff offers a brief etymology of the word "trying," noting its usage as "putting sociology on trial" or "one man's attempt at sociology." "Trying" can also mean irritating and difficult, and just as Wolff finds much sociology to be of this sort, so too will some readers find these dense, sometimes erratic essays "trying." In the 15th century, a "trie" was a wire screen used for cleansing wheat from chaff, and thus "trying" meant separating out impurities. Wolff sifts through much material in these essays, and it is safe to suppose that his readers will differ greatly in deciding what is wheat and what is chaff.

*The European Administrative Elite.* By John A. Armstrong. Princeton, N.J.: Princeton University Press, 1973. Pp. xi+406. \$20.00 (cloth); \$9.75 (paper).

*Politics, Power, and Bureaucracy in France: The Administrative Elite.* By Ezra N. Suleiman. Princeton, N.J.: Princeton University Press, 1974. Pp. xviii+440. \$20.00 (cloth); \$9.75 (paper).

Terry Nichols Clark

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With the increasing involvement of the American federal government in all manner of activities, we may ask how public policies change as a consequence. The most salient actor is often the federal "bureaucrat." Similarly, in Europe the national administrator has become an object of increasing public concern. Many semipopular and some reasonably serious works have held that "technocrats" or the "managerial elite" now "rule" in several European countries. Several commentators on postindustrial society observe similar developments in the United States.

Both these books shed considerable light on such issues through empirical study of European administrative elites. Both authors are political scientists who have learned much from Max Weber as well as the behavioral perspective of the 1950s and 1960s. Armstrong is more ambitious

in dealing with Great Britain, France, Germany (especially Prussia), and Russia from the 18th century to the present. His study is based almost exclusively on documents and reinterpretation of earlier work. Suleiman's focus is on contemporary France; a major portion of his study derives from interviews with 135 leading administrators. Both works contain a good deal of data concerning social origins of administrators, their political views, their administrative roles and career patterns, and their impact on public policy. Suleiman goes into more detail on the organizational structure of ministries, agencies, and the Grands Corps; specific linkages with political leaders and interest groups; and a few useful case studies of administrative reorganization. Armstrong's sweep is broader in space and time, but most of his interpretation is focused on explaining "intervention"—the degree of administrative involvement in economic development, especially that of railroads and other basic industries. Although a single issue, this is clearly a central one to many ideological debates about the appropriate role of government, both in the West and in developing areas.

Both authors have effectively synthesized huge quantities of earlier work. In the process they illustrate how extensive the literature, admittedly often semipopular, is in these areas—in contrast with work on French higher education, for example. Many will doubtless read these works as useful overviews; they could do far worse.

Suleiman stays close to the present and to his data, but still draws on them to confront a series of semipopular issues that have been raised: "Is the unrepresentative administrative elite undemocratic? Is it a technocratic elite that is unresponsive to the society?" (p. 373). Do civil servants cooperate with political leaders? Unnecessarily simple dichotomies are occasionally established, for example, "We must ask whether the Grands Corps constitute elements of change in an otherwise rigid system, or whether they tend to oppose change in the interest of stability" (p. 270). Because the data and discussion are organized in large part to focus on such issues, Suleiman's book may become more "relevant" to some non-social scientists, but taken as a whole the chapters do not build toward an integrated set of propositions. Armstrong, by contrast, has a more detached perspective and seeks quite self-consciously to construct a reasonably consistent interpretation, using his many different pieces of data to explain administrative intervention. He does not present a coherent deductive theory, but he comes closer than Suleiman.

Chapter 14 draws together Armstrong's basic analysis. The issue of intervention is reduced primarily to a role definition into which administrators may be socialized in their early training. This becomes Armstrong's basic dependent variable. It is explained with two sets of variables. First comes a "genetic" explanation, based essentially on the balance of interest groups within the society that control selection of administrators. As the balance of interest groups changes over time, so do role conceptions; Armstrong thus differentiates his approach from a national cultural interpretation. This general interpretative position is used to analyze with

considerable sophistication the interrelated issues of legitimating upward social mobility, administrative intervention, and the shifting role of the classics as a core of the educational curriculum from the Gymnasium to Oxbridge. How technical training was made socially acceptable for different national elites in the 19th century, through combination with military training in France for example, is analyzed in some detail, as is the increasing use of economics and other social sciences in the mid-20th century.

Second, Armstrong's genetic interpretation is complemented by five constant factors which in all countries and time periods tend to encourage intervention: (1) metropolitan contact, especially with upper status social groups in a national capital; (2) territorial experience involving administration in the field, at some point after entry into service; (3) administrative integration, based on organizational unity and homogeneous socialization of elite administrators; (4) training in natural science and engineering, especially at the secondary school level; (5) systematic training in economics, at any point in the career. As a rough test of these hypotheses, Armstrong classified each country into four time periods and tabulated, in terms of a three-point scale, the presence of these five factors in each. Although he is generally circumspect about cross-national quantitative comparisons, the results broadly support his interpretation.

Suleiman also generally uses a role-based interpretation but stresses that roles vary as administrators change positions. Thus an administrator will defend his ministry if the ministry is his primary organizational affiliation. But an administrator of similar social origins, education, and early career will shift his allegiances and become more explicitly political in his role criteria if he is named to positions which reward such behavior. This example illustrates briefly a basic contribution of both Armstrong and Suleiman: They show in many ways that despite the widespread stereotypes, social origins per se are quite poor predictors of administrative behavior. Suleiman also argues against an unchanging definition of the administrator's role, in either its Weberian ideal-type or Crozier et al.'s more cultural approach. Just as Armstrong does not enter into specific changes within a career, neither does Suleiman seriously consider many unchanging aspects of the French administrator's role. Suleiman does use cross-national comparisons on occasion to demonstrate specific differences, especially well with Great Britain—the data seem most adequate here. It is also to his credit that he seeks to consider implications of many findings for the United States and other countries. But too often he is satisfied to point out rough similarities between France and the United States (e.g., between the Grands Corps and the U.S. Army Corps of Engineers). Such quick comparisons are often used to denigrate the arguments of others who have held up this country as counterexample to France. But they do not provide the sort of systematic effort at comparison toward which Armstrong strives.

Some specific criticisms: Even though both authors have drawn on a huge administrative literature, in the sections on social mobility they

could have refined their analyses further by considering more recent sociological work. Cross-tabulation is the limit of statistical sophistication in both works. Some of Suleiman's questionnaire items were poor, for example, "How democratic is recruitment into the higher civil service?" (analyzed on p. 104). Suleiman's comments about French higher education are occasionally either out of date or incorrect (pp. 81, 82, 120). Armstrong repeatedly refers incorrectly to "Sci Pol" (e.g., p. 193). Suleiman's informal style can lead to exaggeration and infelicities such as "the political system of the Fifth Republic has conspired to place the deputy in an impotent position" (p. 313).

Still these are unquestionably both major works, and even though the authors occasionally fall short of the best social scientific standards, they repeatedly show up the naiveté and narrowness of much of the semipopular writing on these subjects. Both works will doubtless serve as important references for years to come.

*Mayors in Action: Five Approaches to Urban Governance.* By John P. Kotter and Paul R. Lawrence. New York: John Wiley & Sons, 1974. Pp. ix+287. \$14.50.

Theodore L. Reed

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Urban governance is treated as an organizational phenomenon in this retrospective comparative study of the mayors who governed 20 large American cities during the 1960s. The authors are members of the Harvard Business School faculty, directing material to practitioners with an analytic bent as well as to students of organizations. Their choice of audience is reflected in the lack of detailed footnotes and limited attention to several areas (e.g., political sociology) in which academic work might inform the inquiry.

The major contribution of *Mayors in Action* is the joining of insights from two previously disparate fields, the highly descriptive work on mayoral behavior and the somewhat more advanced theoretical formulations and empirical research concerning complex organizations. The framework presented by John Kotter and Paul Lawrence includes four major variables: the personal characteristics of a mayor, his agenda (what he desires to accomplish), his network (ability to mobilize critical resources), and the city's organizational, social, and economic context. Working from these contextual variables, they develop a "coalignment" model which states that the system is aligned (exhibits a quality like equilibrium) when the four variables are "consistent," for example when the mayor's agenda fits the needs of the city.

Through insightful synthesis of the existing literature, the authors first identify 10 distinct models of appropriate mayoral behavior (e.g., a power-broker model and a "muddling-through" one). Then they ask how well each model enables them to describe mayoral behavior, predict the mayor's

impact on the city, or explain why a particular mayor behaved as he did. Not unexpectedly, the variations observed were not encompassed by even the most catholic model.

The development of an alternative conceptual scheme and its illustration from the rich interview material (collected primarily by Kotter) occupy the bulk of the monograph.

The new model of mayoral behavior is developed in three stages. First the authors dissect mayoral activity, identifying three central processes: agenda setting, network building, and processes used in actual task accomplishment. In each activity they find considerable variation among the 20 mayors. For agenda setting, the differences can almost be classed on a single dimension. The other processes are much more complex. The section contains a number of important insights.

In the second stage, they describe five modal patterns adopted by mayors in approaching problems of urban governance. No single dimension distinguishes among these patterns. Each orientation—ceremonialist, caretaker, personality individualist, executive, and program entrepreneur—represents a cluster of congruent approaches to the three basic processes.

The third element in the new Kotter-Lawrence model is their treatment of larger, more long-run system dynamics which cause one or another mayoral behavior pattern to emerge. Four "contextual variables" are considered—network (including both formal governmental structure and the distribution of power in the community), the mayor, the city, and the mayor's agenda or agendas. The authors go beyond this static structural description by finding that the relationship between each pair of variables is significant for understanding mayoral behavior. For example, "If the future planning in the mayor's agenda is too far out of line with the feasibility and payoff of planning in his domain (city), the situation is unstable in the long run and must change." With varying degrees of success, dependent on electoral processes and rates of change in the city more than on personal skill, mayors attempt to develop or maintain coalignment between these contextual variables. Long-term nonalignment, they assert, will eventually create problems for the mayor.

How well does the new model explain patterns of urban governance? The framework is quite helpful for understanding why the five modal patterns arise, as long as one remembers that it is primarily a heuristic guide, not as fully defined or carefully specified as much work on complex organizations. It is less successful for evaluating the adequacy of mayoral performance—a fact which the authors recognize and discuss briefly. Lacking profitability criteria or other performance measures, evaluation becomes difficult. The authors venture to say only that each of the five types has its own type of impact on the city and that a mayor's success at coalignment, or his lack of it, influences his effectiveness and efficiency in achieving the type of impact characteristic of his modal pattern.

The contributions to the understanding of administrative behavior, options, and contingencies in a previously uncharted area are central in the model. Less central but more important for sociologists are the implica-

tions of this work for the development of organizational theory and research. The research model, essentially that of an expanded exploratory study which moves back and forth from data gathering to conceptualization, is standard for studies of this genre. Its validity is weakened by the decision to base it on recollections of interviewees, bereft of independent archival or statistical measures. The theoretical issues raised in the model are of more significance.

Kotter and Lawrence see a parallel between their work and that of others who have discussed issues of coalignment (James Thompson), congruence between organizational structure and technology (Joan Woodward), personalities (Chris Argyris), strategy (Alfred Chandler), compliance and goals (Amitai Etzioni), and environments (Lawrence and Lorsch). When particular outcomes can be measured, and coalignment is related to greater or less success in achieving those outcomes, research in this area represents an important advance beyond equilibrium models and other metaphysical formulations. Unfortunately, the admittedly imperfect measure of reelection is one of the few independent measures of success in coalignment activities or changes from coalignment available in this study. The same problems which plagued earlier equilibrium models remain, complete with status quo bias and inability to describe what constitutes nonalignment other than through post hoc theorizing.

*Mayors in Action* should be on the reading agenda of those interested in urban governance, for it represents a significant advance in conceptualization and research in a tremendously complex and significant organizational phenomenon. Students of complex organizations will also find it stimulating, and I hope they will seek to improve upon it.

*Ethnic and Racial Segregation in the New York Metropolis: Residential Patterns among White Ethnic Groups, Blacks, and Puerto Ricans.* By Nathan Kantrowitz. New York: Praeger Publishers, 1973. Pp. xv+104. \$12.50.

Alma F. Taeuber and Karl E. Taeuber

*University of Wisconsin*

For more than half a century the nation's second city, Chicago, has been the foremost laboratory for empirical study of urban ecology. New York, the nation's first city and nonpareil ethnic wonderland, has often been ignored. Professor Nathan Kantrowitz has taken the task of analyzing residential segregation among ethnic and racial groups in the New York metropolitan area. His data base is the 1960 census, and his chief technique is examination of tables of segregation indexes. This slim but expensive book treats three topics in the residential ecology of New York: patterns of ethnic and racial segregation, the effect of income levels on these patterns, and the process of change (1950-60) in neighborhood ethnic composition.



Kantrowitz's discussion of patterns of segregation as of 1960 is oriented around two conclusions. (1) New York, despite its exclusion from previous studies, resembles other American cities. Blacks and Puerto Ricans are highly segregated from each other and from every other group. European ethnic groups are segregated, but not quite so highly, from each other and from that census potpourri, native whites of native parentage. (2) Ethnic enclaves are an enduring rather than rapidly diminishing feature of the metropolitan fabric, and "segregation by race is but an intensification of the existing interethnic segregation" (p. 9).

These conclusions are presented by Kantrowitz as a contradiction of what he perceives to be current wisdom—that ethnicity has melted away and that racial segregation is a completely different social process from ethnic segregation. In the mid-1960s, when this study of New York was begun, there was a self-conscious "rediscovery" of ethnicity by many sociologists. In a rare instance of rapid diffusion of sociological lore, the idea that ethnic culture is alive and well in America now provides the *raison d'être* for family comedies on TV, for the structure of political campaigns, and for a national policy of going slow on black civil rights. Unfortunately, the specific conclusions so forcefully expressed by Kantrowitz are not so forcefully justified by the evidence. The 11 data tables in chapter 2 permit many comparisons among segregation indexes. Selection and interpretation of comparisons suffer from the "fully/only" fallacy. To Kantrowitz the segregation index between Norwegians and Swedes is fully 45.8; a reader who sees the same number as only 45.8 will reach a different conclusion. The problem of interpretation is aggravated by the fact that Kantrowitz is interested in drawing conclusions about trends but has indexes for New York only for 1960.

The greatest barrier to sociological analysis of these vital issues using these data is more subtle. Immigration is a continuing process, yet we try to read the data as pertaining to a fixed cohort of immigrants who first concentrate and then disperse at a rate that is to be judged as being fast or slow. For example, in an effort to forecast future trends in ethnic segregation, Kantrowitz asserts that by 1980, ". . . given the census definitions of foreign stock, the immigrant generation will largely have died. . ." (p. 32). In recent decades, however, more than one-third of a million immigrants have entered the United States annually, and these numbers are augmented by "nonimmigrants" (tourists, students, etc., entering outside of the quota system or preference categories) and by illegal immigrants. Roughly one-fourth to one-third of the foreign-born persons enumerated by the 1960 census had entered the country after 1940; this proportion varies widely by age and by ethnic group. These recent immigrants are of quite different character than the "old" waves of immigrants about whom we think we are generalizing. One-fourth of legal immigrants during the 1960s reported New York State as their destination. Our simple theories of immigrant concentration and dispersal are inadequate bases for conceptualizing complex intra- and intergenerational processes, and the specific character of available census data needs careful attention in any

such analysis. The evidence simply will not bear the weight of the sweeping conclusions that Kantrowitz wants to make.

In chapter 3 Kantrowitz considers the effect of income level on residential segregation. The requisite small-area income data are not available for the full range of nationalities, and Kantrowitz has to limit the analysis to three groups: nonwhites, Puerto Ricans, and all others. His findings are congruent with previous research. Within each group, there is marked residential segregation of rich from poor. But income level has very little to do with the between-group segregation. At each income level, from the bottom to the top, blacks are segregated from whites, blacks are segregated from Puerto Ricans, and Puerto Ricans are segregated from other whites. Kantrowitz concludes that ethnic and racial forces are far more important than socioeconomic forces as determinants of residential segregation. His analysis is based neither on the indirect standardization techniques nor the regression models of residential location that others have used. Instead he presents full tables of segregation indexes for all possible comparisons among racial/ethnic groups at every income level. Kantrowitz has devised some novel graphic techniques for summarizing the information in these tables, but the reader will find it quite rewarding to peruse the basic data (tables 3.1 and 3.2).

The third main topic, the process of neighborhood change or "succession," is the subject of chapter 4. Again the need for small-area data confines discussion to nonwhites, Puerto Ricans, and other whites. Census tracts for New York City are classified according to the type of racial/ethnic change experienced between 1950 and 1960. The expansion of Negro population into previously white neighborhoods is shown to have been led by high-status Negroes, while whites of all statuses departed. No such neat pattern is discernible in the data on Puerto Rican residential succession, but severe data limitations hamper interpretation. For various technical reasons it was necessary to exclude from consideration census tracts representing over half of the Puerto Rican population and nearly one-third of the Negro population. Consequently, as Kantrowitz recognizes, chapter 4 "resembles as much a series of vignettes as it does the steps of a logical argument."

Those of us who in the past have eschewed inclusion of New York in census-based comparative urban research can find in the methodological trials and tribulations of Kantrowitz ample justification for our trepidation. But if our sociodemographic data and techniques are not yet sufficient to permit us to dissect New York, they are nonetheless capable of revealing certain features of gross anatomy. The data assembled here deserve careful scrutiny by sociologists interested in urban structure and ethnic relations. Professor Kantrowitz is to be commended for taking on the Herculean task of bringing New York under the knife of the "Chicago school" of urban residential ecology.

*Dynamics of Idealism: White Activists in a Black Movement.* By N. J. Demerath III, Gerald Marwell, and Michael T. Aiken. San Francisco: Jossey-Bass, Inc., 1971. Pp. xix+228. \$10.95.

Michael Useem

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Political protest movements are notoriously resistant to objective sociological analysis. Their roots are invariably obscure, their lives transitory, and their societal effects rarely susceptible to isolation. Moreover, the nature of movements insures that attempts to dissect them will normally be received suspiciously by the movement participant as well as opponent. Finally, researchers themselves often have stakes in the conclusions of such studies. Despite these problems, the authors of *Dynamics of Idealism* have produced a disciplined and detailed study of the participants in a phase of one of America's most important sustained protest campaigns—the civil rights movement of the 1960s.

Revitalized by the sit-ins and freedom rides of the decade's early years, the civil rights movement was approaching a turning point by 1965. The Student Nonviolent Coordinating Committee and other organizations fielded successful integrated voter registration projects in Mississippi in the summers of 1963 and 1964, and a year later the Southern Christian Leadership Conference launched similar efforts in six southern states under its SCOPE program (Summer Community Organization and Political Education). But 1965 also brought a sharp intensification of the U.S. war in Indochina, militant outbreaks of campus protest, the rebellion in Watts, and incipient black power currents in many civil rights organizations. Black activists increasingly questioned the role of whites in their movement, while white activists were discovering they had their own struggles against the war, conscription, and college administrations.

N. J. Demerath, Gerald Marwell, and Michael T. Aiken effectively capitalized on an opportune moment for the study of a movement in transition. Access problems were predictably severe—indeed, the researchers were rebuffed in an initial attempt to research the 1964 voter registration campaign in Mississippi. Drawing upon the lessons of that defeat, the authors returned a year later and adroitly won support from the SCOPE staff to study white participants in the 1965 campaign. Their account of the field research difficulties they encountered is told with exceptional detachment, honesty, and insight, and this passage is recommended to all contemplating comparable field research.

The authors' sources of data included direct observation, interviews, personal documents, and a longitudinal questionnaire survey of white SCOPE participants. Several hundred volunteers completed the form just prior to the summer's experience and again at the end of the session, and much of the book is concerned with evidence from this survey. In addition, four years later about a fourth of the original participants were contacted for brief telephone interviews.

The white activists resembled the campus militants of a later period. Characterized by an upper-middle-class upbringing and progressive but nonsocialist politics, the typical volunteer was deeply committed to social change on a variety of fronts. The civil rights movement provided a vehicle for expressing these concerns, and many of the students saw the demanding, sometimes risky, summer experience as a test and affirmation of their commitment.

Anticipating passage of the 1965 Civil Rights Act, SCOPE emphasized voter registration, and by summer's end the volunteers claimed nearly 50,000 new names on the rolls. Yet despite this impressive achievement, many activists came to feel that time would be better spent on community organizing, and by the end of the campaign volunteers were measuring success by the amount of protest they generated, rather than by the number of voters enrolled. Currents in the movement, together with the community workers' own experiences, combined to move the participants leftward. To the chagrin of the many blacks who remained behind, most whites returned in the fall to their northern campuses, though not simply to their classrooms. The civil rights experience had created a cadre of seasoned, dedicated activists, and many assumed leadership roles in the rebellions which soon erupted around students' issues. However, the movement's legacy was short-lived for most volunteers, and within four years substantial proportions either had entered more moderate political causes or had abandoned activism altogether.

The discussion and analysis in *Dynamics of Idealism* remains close to the data. The authors tend to work inductively, drawing out, where appropriate, the implications of their findings on the dynamics of involvement in a political protest movement. Occasionally, however, their generalizations are inappropriate. The four-year follow-up interviews revealed, for instance, that most of the political dropouts were women, a fact the authors interpret as an indication "that women may not be good bets for sustained activism of any sort" (p. 180).

Unfortunately, the book closes with a chapter that bears little relationship to the authors' research findings. A number of glib assertions are offered on topics ranging from the national power structure (which, the authors maintain, has failed to promote social change largely because of "organizational complexity") to revolution in the United States (the consolidation of which, they contend, "could well become a bloodbath" [p. 197]). An analysis of the origins of the student movement is diffuse and eclectic, even selectively employing long discredited theories about the motivation of student protesters: "[T]he university resembles the parent who suffocates with understanding and forestalls independence with indulgence. Thus, many students are individually guilt-ridden and ambivalent concerning the university" (p. 201). Ironically, having dedicated the book to the children of Martin Luther King, Jr., the authors close with a "memo to educational administrators."

*Dynamics of Idealism* is rich with insights based on carefully assembled information on the micro world of the protest participant, and it addresses

some of the central issues of the relationship between a political protest movement and its membership. With the exception of the final chapter, it is highly recommended reading for those concerned with racism, civil rights, protest, and change in the United States.

*Black Students in Protest: A Study of the Origins of the Black Student Movement.* By Anthony M. Orum. Arnold and Caroline Rose Monograph Series. Washington, D.C.: American Sociological Association, 1974. Pp. v+89. \$2.75 (members); \$5.00 (nonmembers).

James P. Pitts

*Northwestern University*

Major social upheavals have a way of putting research on stratification and social movements to the test. Research which restricts the study of inequality and political consciousness to narrowly conceived indices such as voting patterns and family income can become obsolete by failing to provide data or perspective on sociohistorical context, relationships, and social process. Likewise, scholarly visions which project simple linear developments in structured inequality and political consciousness may be remembered as indices of popular consciousness in an era more than as adequate analyses of societal integration and change. Thus, major social changes in the sixties and seventies threaten to trivialize many previous and current writings on race relations and economic theory.

Fortunately, Anthony Orum's monograph in the Arnold and Caroline Rose series is a careful piece of sociological research and interpretation likely to be noted for its strengths more than its weaknesses. It will have enduring value for those who attempt to put the 1960s and the black revolt into societal perspective. Using NORC survey data collected from 3,423 black seniors in Negro colleges in 1964, Orum examines the personal backgrounds and values of civil rights protesters and their nonprotesting peers as well as the community and campus context of such demonstrations. Data reported in chapter 4 will be familiar to those acquainted with his widely cited article, "The Class and Status Bases of Negro Student Protest" (*Social Science Quarterly* 49 [December 1968]: 521-33).

The monograph begins with a brief introduction to acquaint the reader with the sit-in activities of Negro college students in the early sixties and ends with a more than adequate conclusion which suggests how those activities may relate to the more black nationalist-oriented student activism of the late sixties. In between, there are separate chapters on students and the schools, precollege political socialization, the campus as a basis of protest, and structural factors operating in the demographic and organizational character of black colleges and their communities. In less than 90 pages, the analysis of cross-sectional data is tightly related to a variety of theoretical issues commonly denoted by such labels as status deprivation, mass society, and rising expectations.

Some of Orum's most significant findings are that "the family's socio-

economic status bears virtually no relationship to participation or activism; family stability is only slightly associated with participation; and in contrast to the findings of certain research workers, students from stable homes are more prone to be participants than those from broken families" (p. 36). The analysis also suggests that student activism among blacks in Negro colleges in the early sixties was very much related to the social and economic conditions of local black populations surrounding the black colleges. Finally, the analysis shows that students active in campus organizations are more likely to be active in protest activities than those who are less active in campus organizations, but that the grade-point averages of activists are indistinguishable from those of nonactivists.

One of the principal objectives of the research was to study the characteristic motivation of the black student demonstrator. Thus, it is peculiarly ironic that the survey made no attempt to study students' commitment to the black race. Questions used to infer the motives of protestors largely ignored the possibility that race consciousness (however defined) might be a factor. Orum's conclusion stresses that subsequent to the collection of the NORC data the tone of black student protest became more militant and was visibly influenced by black nationalism. What this survey cannot tell us is whether or not race consciousness was a less significant factor in earlier black student protests when their objectives were more clearly oriented to integration.

In all fairness to Orum, the reason that this subject was not explicitly investigated is obvious. The research is firmly grounded in the sociological literature of the fifties and early sixties. Even by the late forties the political consciousness of social scientists had evolved to a point where racial nationalism on either side of the line of inequality was considered anathema. In any case, the thrust toward racial integration among some sections of the black middle class was prominent, and black resistance to integration was inconceivable to most of its black and white advocates. Accordingly, the research literature of the fifties gave scant attention to possibilities that black supporters of desegregation and integration might be significantly motivated by perceptions of group membership and interest. This is not to suggest that Orum's study or the literature on which it was based was misguided. The point is that future research on the motivation of activists and others among subordinate racial or ethnic groups must look at the experiences of both class and race-ethnic inequality—even in periods of the struggle when only one of the two is emphasized in public advocacy and discussion.

*Interorganizational Activation in Urban Communities: Deductions from the Concept of System.* By Herman Turk. Arnold and Caroline Rose Monograph Series. Washington, D.C.: American Sociological Association, 1973. Pp. x+67. \$2.75 (members); \$5.00 (nonmembers).

J. Kenneth Benson

*University of Missouri*

Herman Turk's monograph is an effort to establish an interorganizational level of analysis. The focus of attention is the city, conceived as a system of organizations and interorganizational networks.

The activation of new interorganizational networks to carry out needed programs depends upon the richness of linkages and potential connections between already established organizations. A city with an elaborate set of linkages already developed or with an organizational apparatus facilitating linkages is more likely than others to initiate programs requiring the activation of new interorganizational networks.

What characteristics of cities affect the likelihood of interorganizational activation? Turk argues that the size and diversification (differentiation) of organizations are crucial predictors. Where organizations are extensive and diverse, their many components tend to be semiautonomous and dependent on other organizations for successful operations. The specialized components, say, of city government, must interact with other organizations in order to get their work done. Turk argues that partial conflict among multiple, semiautonomous, specialized units has great potential for interorganizational activation. By contrast, centralized control in a relatively undifferentiated system provides a barren field for interorganizational collaboration. In addition, interorganizational activation is facilitated by the presence of organizations which express common values of the community and the larger society. Such organizations prevent conflict from becoming excessive and unproductive. Where the organizational structure of the community provides both for partial conflict among many semiautonomous units and for the mediation of conflict by community-wide values, the likelihood of bringing together the resources and interests required for cooperative programs is increased.

The empirical tests of these arguments (which hardly qualify as "deductions" despite the subtitle of the monograph) are accomplished by testing the relationships between measures of need and measures of programmatic response (supply) under varying states of community potential for activation. The units of analysis are the 130 U.S. cities with 1960 populations in excess of 100,000. The prediction, which is sustained in most of the tests, is that the association between need measures (such as poverty rate) and supply measures (such as poverty programs) will be stronger where potential activation is high. Given a need for services, cities with high potential for activation are more likely than others to establish the programs.

The community potential for interorganizational activation was indi-

cated by (1) the size and diversification (differentiation) of municipal government, (2) the strength of political parties (using nonreform attributes of the city to indicate party strength), (3) the importance of community-wide voluntary associations (based on essay responses to form letters addressed to selected informants).

Although there may be some technical-methodological flaws, the monograph is potentially important because the results appear to demonstrate empirically the value of an interorganizational perspective. The monograph could become an exemplar for the rapidly developing study of interorganizational relations, but in my view that would be a most unfortunate development.

The core problem is that the political character of system from which the work proceeds remains unanalyzed. Turk's use of the system concept to interpret his data thus serves as a justification of the liberal faith in democratic institutions and in organizations as functional components. A system, in his view, is a somewhat arbitrarily bounded social unit (e.g., the city) within which a structure (varying in differentiation) can be identified. The system processes information regarding needs (e.g., the percentage of the population living in poverty) and converts the needs into demands (and thus programs) with varying degrees of efficiency. The conversion of needs into programs is dependent upon the interorganizational capacity of the unit. There must be a potential for extensive linkages between organizations; and some organizations must provide guidance regarding generalized values of the system. In such a system the interests of diverse factions are represented, but compromise based on overriding values is possible. We are presented, then, with an empirical and interorganizational version of liberal ideology. It is, I believe, a poor basis for the interpretation of Turk's own data. Consider several alternative interpretations:

1. Networks beget networks. The development of interorganizational networks in a city (or elsewhere) involves the creation of new power centers, that is, concentrations of economic and political resources. These networks may become self-sustaining in that there is a continuous search for activities from which new resources may be derived. Programs such as model cities, antipoverty, and health services provide for this. The connection of this network structure to classic democratic processes may be tenuous or even nonexistent.

2. Big bureaucracies dominate little bureaucracies. The presence at the local level of an extensive and diversified bureaucracy may expose the city to the influence of national bureaucracies. Federal agencies have counterpart local agencies. Relations of interdependence develop between them. The city is then quick to follow initiatives from the federal level.

3. Cities play in bigger games. The city may be seen as an actor in a larger arena of political games. New programs bringing resources to the city from outside are among the payoffs in the larger arena. Both the size of municipal government and the strength of political parties may affect the power of the city as an actor defending its interests in a national



game. Thus, the city may not be the appropriate unit of analysis for predicting the kinds of events studied here. To his credit, Turk recognizes this potential problem (pp. 57-59).

In summary, Turk's concept of system is politically underdeveloped, and I think he tends to fill the gaps with liberal ideology. Granted that differentiation of urban structure is important, how it fits into a larger scheme of things—both macro and micro—is not clear and goes largely unattended in the monograph. A more elaborately developed concept of system—distinguishing levels, linking structural patterns across levels, dealing with power—may be a prerequisite to resolving these issues in future research.

*Private Lives and Public Surveillance: Social Control in the Computer Age.* By James B. Rule. New York: Schocken Books, 1974. Pp. 382. \$10.00.

Kenneth C. Laudon

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Few modern institutions have generated more hysterical armchair comment than corporate and governmental information systems. The motivation behind such commentaries is that information on people qua individuals will be used ultimately to lessen the individual and collective freedoms of entire societies; the historical precedents cited are Elizabethan and Czarist political files, the 19th-century French political intelligence files of Fouché, and, of course, the political bureaus of most totalitarian regimes in the 20th century. The arrival of third-generation real-time computerized information systems in local, state, and federal governments in America has not only heightened these concerns but added a new dilemma: the computers can talk with one another. Hence it is now possible through integrated information systems to bring together rapidly and cheaply individual health, welfare, and other institutional data to make decisions about specific individuals. It is a short step, so the armchair philosophers claim, to the Orwellian nightmare of a society of total surveillance in which every relevant aspect of private life is brought to bear on corporate and governmental decisions. Worse, it is an inevitable step in the logic of how these administrative intelligence systems have developed over the last century. Ultimately the mere existence of the technology, the logic of its development, will shape the political and social institutions of the future.

Fortunately, Rule's *Private Lives and Public Surveillance* spares us such analyses and manages instead to treat much more subtle questions raised by mass surveillance systems. The book is based on five case studies (three British systems: police surveillance, the vehicle and driver license system, National Insurance; and two American systems: TEW Credit Data Corporation and BankAmericard). These case studies are compared along five dimensions: the structure of information collection, storage, and

retrieval; movement of information and decision making; patterns of change; proximity to the total surveillance model; and, finally, the surveillance capacity of each system (size, centralization, speed of information flow, and points of contact). Throughout, the author focuses on three questions: what are the empirically visible changes and trends in surveillance capacity, what social forces impinge on the development of these systems (and vice versa), and what are the implications for personal privacy and political repression?

Rule believes that over time the systems have increased markedly in overall surveillance capacity but not without severe cost problems and loopholes. Information, it turns out, is not cost free, and that fact alone imposes limitations on future growth. Although the British studies were completed before computerization took place, even in the highly computerized American credit data systems only a limited amount of information can be stored and retrieved. Yet despite these limitations, their effective capacity is growing because, contrary to the Orwellian vision, only a few pieces of information are required by large credit-granting organizations in order to refuse credit to potential deviants.

Rule examines only those mass surveillance data banks which function as tools of social control (those in which the primary concern is the detection of agency rule-violators and enforcement of regulations). He believes the growth of mass surveillance systems is intimately connected with changes in social scale. As a society grows in sheer numbers, and as the intensity of social relationships increases, so does the need for mass surveillance systems. In turn, large mass surveillance capacity makes new social relationships possible: a fair and just system of national medical insurance requires surveillance of the benefit population.

The implications of mass surveillance for political repression and freedom are hardly clear cut in Rule's view. On the one hand, mass surveillance is required for the fair and just operation of large social programs. The benefits of such programs are manifest and much desired; moreover, while mass surveillance systems could serve repressive regimes, they do not necessarily engender such regimes. Yet Rule calls for the curtailment of mass surveillance systems because of their repressive potential—they could be put to the wrong use. In the end, "Corporate participation in every moment of every individual's life, no matter how fair or how discreet or how benign, is simply too great a price to pay for obedience" (p. 355).

Rule's theoretical perspective provokes some critical thoughts. It is not clear to me that the capacity of mass surveillance systems, especially in credit reporting, in any way parallels the intense surveillance of small town "grocery store" credit granters. Indeed, the license granted by most Western societies even in areas of large-scale potential deviance is more remarkable. Moreover, Rule ignores large comparative differences in data-gathering traditions among Western countries, differences which have nothing to do with social scale but everything to do with political tradition and culture. The politics of data collection are an important part of

the dynamics of surveillance systems. In the United States this means the federal government has warehouses of information on welfare clients but little information on social service providers (doctors, nursing homes, Medicaid clinics). *Private Lives and Public Surveillance* has made an important contribution to the future debate of these issues.

*Originality and Competition in Science: A Study of the British High Energy Physics Community.* By Jerry Gaston. Chicago: University of Chicago Press, 1973. Pp. xix+210. \$10.95.

Lowell L. Hargens

*Indiana University*

This book reports results from an intensive study of the British high-energy physics community carried out during 1967-68. During his stay in Great Britain, Jerry Gaston interviewed 203 members of a population of 220 physicists at universities and laboratories, obtaining publication lists, career histories, and information about the institutional settings and sources of support for research in high-energy physics. Gaston used these data to replicate and extend the work of R. K. Merton and W. O. Hagstrom on such topics as relations between contributions and rewards in science, the sources and consequences of competition for priority, and patterns of communication among scientists. His book is therefore of interest both as a case study of a scientific community and as a source of comparative information on topics previously examined in studies of scientific communities in the United States.

Two themes are prominent in Gaston's interpretations of his results. First, many relationships between individuals' scientific contributions and eminence, on the one hand, and the prestige of their institutional affiliations, on the other, appear to be weaker in the British high-energy physics community than in American scientific communities. Gaston argues convincingly that the weaker relationships in Britain are the result of governmental policies which discourage institutional competition for scholars and restrict the numbers of researchers produced by British universities to the extent that the Ph.D. is conferred upon only those of fairly uniform and, one hopes, high levels of scholarly ability. British high-energy physicists are an elite produced by the operation of a sponsorship mobility process, and Gaston claims that the British system minimizes many of the dysfunctional features of more competitive academic systems, such as job insecurity and the replacement of scholarship by entrepreneurship. By granting early (by U.S. standards) tenure and restricting opportunities for personal and institutional competition, the British system tends to eliminate distractions associated with the nonscientific elements of scientists' job situations. It is still true, however, that British high-energy physicists are motivated by competition for priority in the presentation of research results, and Gaston shows that they are "scooped" in their research about as often as physicists in the United States.

Although the British system of support for science may discourage the development of personal and institutional rivalries, certain practices which are prominent in more competitive scientific communities are still present among British physicists. Gaston shows, for example, that contrary to expectations they tend to be as secretive about their current work as physicists in this country. He suggests that the small size of the British high-energy physics community makes its members more or less continuously visible to one another. This condition leads to reticence in informal communication because scientists are afraid of damaging their reputations by informing others of results which may later turn out to be erroneous. Thus, although it flows from different sources, secretive behavior, which hinders the efficient operation of scientific communication systems, appears to be as prevalent in the British as the American physics community.

The second major theme of Gaston's book concerns the differentiation of high-energy physicists into various types of theorists and experimentalists. The author demonstrates that this division of labor has profound consequences for individuals' positions in the reward structure of the community, their patterns of communication with other physicists, and their experiences in scientific competition. Informal ethnographies of the various types of physicists are presented in a chapter describing their different working arrangements and in John Ziman's foreword. Gaston shows that it is not the abstract theoretical physicists who enjoy the greatest eminence in the high-energy physics community but "middle-range" theorists who serve as a bridge between experimental physicists and more abstract theoretical physicists. This finding confirms some of Hagstrom's hypotheses about the determinants of specialty prestige in science. Sociologists of science have sometimes neglected this sort of division of labor within disciplines, and Gaston's work should make such neglect less frequent.

While the title of the book suggests that the problem of understanding originality in science will be one of its central concerns, that problem actually receives little attention. Aside from this minor misrepresentation, however, the book is well done and provocative. It makes important additions to our understanding of the social structure of contemporary physics, and it deserves a careful reading by students of the sociology of science.

*The Academic Labour Market: Economic and Social Aspects of a Profession.* By Gareth Williams, Tessa Blackstone, and David Metcalf. Amsterdam: Elsevier Publishing Co., 1974. Pp. vii+566. \$18.50.

Oliver Fulton

*University of California, Berkeley*

This book reports on the findings of a series of surveys of British universities conducted between 1968 and 1971: two surveys of university faculty (one mail, one interview) during the year 1969-70 and two surveys of applications for faculty positions advertised in 1968-69 and

1970-71. The "economic and social aspects" of the profession which were investigated are personal background, career history and qualifications, rewards (monetary and nonmonetary), and a variety of attitudes concerning teaching and research, students and student activism, academics' own jobs and their careers, and broad educational and political issues (such as the appropriate size of the British university system). The authors are a sociologist and two economists, one of whom analyzes the profession as a labor market while the other is interested in the effects of the massive expansion of the ten years preceding the survey, primarily from a manpower-planning perspective.

The book is a collection of essays more than a coherent whole. Though alleged in the foreword to be an example of the effectiveness of multidisciplinary research, it shows few signs of coordination among the three authors, even at the instrumental design stage. Its intended audience is not obvious: although most of the chapters on the operation of the labor market would be quite opaque to noneconomists, many of the early chapters take pains to be accessible to lay readers. Most important, neither the foreword nor any of the two prefaces and two introductions provides a clear statement of the overall themes that might link the authors' interests, and one looks in vain for a concluding chapter which might tie some of the threads together.

The "sociological" sections of the questionnaires contained a number of items repeated from the survey of the profession conducted in 1964 (A. H. Halsey and Martin Trow, *The British Academics* [Cambridge, Mass.: Harvard University Press, 1971]), and the chapters by Blackstone cover many of the same broad subjects as the earlier book, though often providing time-change data. These chapters include the following subjects: political attitudes; opinions on the size and scope of the British university system; attitudes toward research, teaching, students, and the career of the academic; and an essay on women university teachers. All rely entirely on cross-tabulation (almost always two-way tables); the implications are well drawn out, though one could wish for more multivariate analysis. The last three chapters are especially interesting intrinsically, although the chapter on women comes to the slightly half-hearted conclusion that discrimination "cannot be proved to exist," despite convincing evidence provided elsewhere in the book, as, for example, in Metcalf's regression analysis of factors affecting salaries. There are some omissions: for example, a book which spends so much time on the distorted age and promotion structure of the profession should have examined interfaculty relations as well as faculty-student relations.

Insofar as there is an overall theme, it is provided by Blackstone and Williams's description of the profession's response to its own recent and extraordinary history. In its rapid growth during the 1960s, the British higher-education system was not exceptional; but the academic profession succeeded, in the short run, not only in containing an astonishing amount of the expansion within the elite (university) sector, but also in avoiding any visible deterioration in the defining qualities of that sector.

Specifically, for example, student selectivity, measured by formal qualifications, did not fall; instead, the increase in qualified students wishing to attend universities fueled the expansion. But even during the most hectic expansion of student numbers, universities made no attempt to take advantage of possible economies of scale. On the contrary, staff-student ratios actually rose. As a consequence, the profession at least doubled in size between 1960 and 1970; staff numbers probably increased by 20% in 1965 alone; at the time of the survey, nearly a third of the profession had joined it within the past three years.

The economic consequences are clearly spelled out by Williams; the broader social consequences are referred to by both Blackstone and Williams. In brief, the factors which encouraged the profession to improve the staff-student ratio, that is to expand even faster than demand—namely, central governmental planning and a steeply incremental salary scale—contained the seeds of its downfall. Successive British governments have turned against the universities; salary costs increase inexorably even without expansion, as do the promotion bottleneck and other inequities. Just as the protection of the economic structure of the profession during expansion insures that it will come under great strain when expansion slows, so the universities' defense of their other elite qualities may also have contained seeds of their downfall. Elite institutions can survive if they are truly elite; policymakers are their loyal alumni, and their distinction is so obvious (and relatively cheap) that they are supported by the population as a whole. But an overgrown system with elite values is in grave danger, and the authors reveal all too clearly that elite values have persisted inappropriately. British academics are as opposed as ever to further expansion (which, for many, amounts to kicking down the ladder behind them). They place chief importance on research and pay lip service to teaching; their proposals for increasing the weight of teaching amount to a retention of the status quo. They are sympathetic to student activism but not prepared to change their traditional role of authority vis-à-vis students. The authors conclude that the universities' "preliminary concern is preserving their elite status" and accuse them of having had in the 1960s "jam on both sides of the bread." Thus, in the 1970s, they turn out to be costly, arrogant, and still full of dissidents with whom they will not come to terms, but whom they also will not exclude. They are now suffering from the inevitable loss of popular esteem and political power.

*Power, Persistence and Change: A Second Study of Banbury.* By Margaret Stacey, Eric Batstone, Colin Bell, and Anne Murcott. London: Routledge & Kegan Paul, 1975. Pp. xii+196. \$15.50.

Ann Swidler

*Harvard University*

*Power, Persistence and Change* is a disappointing, frustrating book, particularly in contrast to Margaret Stacey's original study of Banbury, *Tradi-*

*tion and Change* (Oxford: Oxford University Press, 1960). At the time of the first study (1948–51) Banbury was a relatively traditional market town, entering a period of rapid industrial expansion. After 15 years of industrial growth, the in-migration of a large industrial labor force, and the creation of new suburbs and housing developments, Banbury provided an excellent site for a study of social change. Yet the second study of Banbury, while it parallels the first in research design and thematic organization, is less interesting and tells us much less about social change than did the original study.

*Power, Persistence and Change* is based on more than two years of fieldwork by three investigators, as well as a survey of 1,400 respondents. The authors present data on neighborhoods, family life, politics, religion, and class, with particular emphasis on the network of face-to-face relationships which defines each of these institutional spheres. Yet this emphasis on social networks masks the absence of any useful theoretical framework for understanding social change.

*Tradition and Change* described social change in terms of the tension between traditional and nontraditional spheres. In *Power, Persistence and Change* this distinction is abandoned, with the comment that "there is no local social system any longer definable. The demarcation between the locality and the outside world has indeed decreased, such that identifiable local social relations are not discernible in any local holistic sense" (p. 4). If the traditional social structure and the values it embodied have really disappeared, we are offered no explanation as to how or why; nor are we given any sense of what such a change might have meant for the people of Banbury.

*Power, Persistence and Change* offers no new theoretical framework to replace the traditional-nontraditional distinction. Instead the authors pursue various issues concerning which they feel there is "something particular to contribute to sociology as a whole or to a sub-discipline" (p. 5). A description of the increased size and impersonality of workplaces (chap. 2, "Banburians, Immigrants and Work") leads to a discussion of the fit between workers' preferences for plants of differing sizes and their actual work situations (pp. 23–25). An extensive description of interaction patterns in three neighborhoods (chap. 7, "Neighbours and Neighbouring") concludes that interaction patterns are determined more by shared economic and social roles than by the physical layout of neighborhoods (pp. 93–103).

The concluding chapter, "Inequality and Order: Social Class, Social Status and Power," is marked by methodological as well as theoretical confusion. The authors ask whether, "in addition to being divided into classes based on economic position, the population is divided into classes based on position in the housing market" (p. 123). They claim that "if positions of greater freedom or self-determination in the work situation went with positions of greater freedom and self-determination in the housing situation, sharp divisions of the society into economic levels might result" (p. 123). The poor sociological logic of this analysis is compounded

by misinterpretation of data. The authors find, as we might expect, a substantial relationship between occupational status and whether people own or rent their homes (p. 124, table 9.2); but, without presenting any measures of association or tests of statistical significance (here or anywhere else in the book), they declare that "the correlation is not high" (the  $\gamma$  for owning vs. renting is actually a healthy .427), so that "occupational status and housing status do not run together so closely as to produce sharp divisions between economic levels" (p. 123).

Many conclusions are not supported by the authors' own data, and there are glaring inconsistencies between tables and text. We read that "the long assumed close and increasing association between education received and occupational status achieved applies only to the geographically mobile" (p. 127), yet the relevant table shows only that better-educated and higher-status respondents are more likely to be newcomers to Banbury. Similarly, the authors report the surprising finding that "for women education correlates more closely with occupation than it does for men" (p. 128), yet when we turn to appendix 5, as the authors suggest, we find no data on the relationship between education and occupation for either women or men. Again, on page 129, the authors assert that "eighty-seven percent of sons now in classes I and II had no education beyond 15 years old *and* had fathers in class III or below," while the actual figure should be 37%.

Despite many errors and confusions, *Power, Persistence and Change* contains a few interesting findings, though little use is made even of these. Particularly striking are the changing relations between power and class in Banbury. An analysis of links among the leaders of Banbury's churches, political parties, and voluntary associations shows that, while in 1950 only one common member linked the governing boards of higher- and lower-status organizations, in 1967, 44 out of 148 links crossed the "occupational-status frontier" (p. 46). This increase in visible class integration is at least partly explained by the incorporation of the Labour Party into the town's leadership (p. 47), but it may also indicate some blurring of traditional class boundaries.

Power has slipped away from Banbury, so that local events are increasingly shaped by the decisions of large national and international corporations and by regional and national governing bodies (pp. 56-59). Simultaneously Banbury has seen "a multiplication of specialist pressure groups" (p. 57). Yet here, as in many places throughout the book, it would have been useful to have findings from Banbury put in context by comparison with other community studies and with related studies of social change. As it is, we are often hard pressed to decide whether we are to consider specific changes (for example, the increased proportion of married women who work outside the home [p. 105]) results of Banbury's particular spurt of growth, or consequences of broader patterns of social change.

To compound its other faults, *Power, Persistence and Change* is badly written. It has neither the charm nor the clarity of *Tradition and Change*. This is particularly puzzling since Margaret Stacey is the senior author



of both volumes. I suspect that many of the weaknesses of *Power, Persistence and Change* come from the attempt to speak to various specialized subdisciplines in sociology. In that effort the authors have sacrificed the rich sense of the people and culture of Banbury which might have made a valuable contribution to our understanding of social change.

*Societal Development: Five Approaches with Conclusions from Comparative Analysis.* By Szymon Chodak. New York: Oxford University Press, 1973. Pp. viii+357. \$12.50.

Rae Lesser Blumberg

*University of California, San Diego*

If the year were, say, 1965, and if I were a "functionalist" sociologist, I would be delighted with this book. From that perspective, this could be considered a work of good and useful scholarship. In the middle of that ironically named "development decade" (the gap between rich and poor nations actually grew), U.S. social scientists of the orthodox mainstreams of economics, sociology, and political science were propounding rather diverse and seemingly competing theories of what "development" or its synonyms (e.g., "modernization") meant. Many of those social scientists also were actively advising on how "they" of the Third World could best pull themselves up by their bootstraps and most quickly be made like "us" of the "developed" countries—the advanced capitalist nations of the West and Japan. Who then could have doubted the value of a book which ably pulled together most of those varied propositions and prescriptions?

Specifically, Chodak undertakes a comparative analysis of five approaches to societal development (taken from the above orthodoxies plus classical theories of societal evolution) which skillfully shows their points of complementarity. For each he summarizes a large amount of literature and usually manages to present a new perspective of his own. He personally stresses the view of societal development as a process of "growing systemness" (which is more or less the second approach), but he manages to extract a key "variable" from *each* approach, and these he proposes as the framework for a still-to-be-formulated comprehensive theory of societal development. These variables are described in a too-brief last chapter ("Variables of the Sixth Approach"), but that is all. He makes no explicit attempt to develop the "Sixth Approach," the elusive general theory.

The approaches he subjects to comparative analysis are (1) evolutionary theories, (2) the view of development as "growing societal systemness" (involving simultaneous increase in differentiation and interdependence), (3) the opinion that psychological factors constitute the reason why development occurs in given cases, (4) theories of economic development and political development, and (5) the concept of modernization. Following the presentation of the second approach, he compares cases of

"growing systemness" in the capitalist West and Japan, the Soviet Union, Africa, and "international relations." Development—"growing societal systemness," to Chodak—is not only inevitable, it is also "irreversible and directional" (p. 55) (but what about the fall of Rome?). To study societal development, he proposes the following "variables" composed of paired factors: (1) conflict within consensus, (2) differentiation within growing interdependence, (3) insecurity within security (he views development as *generated* by this), (4) change within continuity, and (5) economic and political development.

Chodak is a political scientist, but his bibliography is commendably multidisciplinary as well as impressively large and multilingual. The international references reflect the fact that Chodak has lived and worked in his native Poland, plus the Soviet Union, sub-Saharan Africa, England, Canada, and the United States. One need not wonder, "which side is he on?" His emphasis of the politically totalitarian and economically inefficient aspects of the USSR and the East European socialist nations, versus an often idealized portrait of the individual freedom offered by the "Western capitalist democracies," comes through clearly. But he does emphasize that development is a mixed blessing under *both* capitalism and East European socialism, generating pollution, crime, and other problems. And his portrait of the West does show some of capitalism's warts: "The political freedoms and the freedom to make money as established and exercised in capitalist societies were based on and generated and promoted economic inequality in the first instance, and social inequality in the second instance" (p. 81).

Even from my assumed mid-sixties functionalist perspective, I notice some readily apparent flaws. For example, one would expect a bit more progress toward a theory and measurement of societal development than Chodak provides. The short list of "variables" in the skimpy last chapter are not really *variables*, and the connections between them are not even speculated about. The author says even less about the measurement of development, noting that at least two indicators seem needed: "one for differentiation and specialization, the other for the degree of interdependence." But which indicators? Perhaps—but perhaps not, he adds—some measure of "the average number of specialized socioeconomic roles performed by individuals" can provide a start toward a scale of development (p. 75). As the only other counsel, he suggests that these two indicators may also correlate with GNP (a measure he and the "mainline" economists he reviews accept with few reservations as a satisfactory yardstick of economic development).

But it is not 1965. Since then, the functionalist-tinged propositions and positions Chodak treats have come increasingly under attack from many sides as ethnocentric, irrelevant, and/or empirically wrong. The net result is a book whose omissions, distortions, and biases severely limit its worth. Chodak's curiously dated and naively narrow views can be grasped from a single quote. Just before he turns to psychological factors as *the* generating force of development, Chodak notes that the gap between

industrialized and underdeveloped nations has widened. "Yet the new nations had just obtained the most important instrument for development—their independence, their fate in their own hands" (p. 159). To believe, in 1973, that mere independence gives control of a country's fate with respect to development is to believe in a world in which extranational factors are largely irrelevant—a world in which, if the underdeveloped countries sink further behind, the fault must lie in something *they* lack or are doing wrong. This is Chodak's world, the world reflected in the development approaches he presents. He seems totally unaware of the host of theories criticizing his world that have recently emerged.

To be specific, then, this is a book that ignores the possibility that development of some nations and underdevelopment of others can be part of the same historical process (cf. Griffin, Wilber); that the policies of a Third World nation's planners or its aspiring entrepreneurs' need for achievement generally are overwhelmed by extranational factors stemming from the advantages of the First and Second Worlds of development. Chodak almost totally ignores the stratification of the world economy that negatively affects terms of trade and/or "aid" for underdeveloped nations (although even such a nonradical economist as Han Singer was writing about this back in 1950). And he does totally ignore theories of dependency (Frank, Furtado, Dos Santos, etc.) and theories of neocolonialism and imperialism (Magdoff, Baran, Rhodes, etc.). Furthermore, he is silent about the growing role of (mainly U.S. based) multinational corporations in distorting development in their target countries of operation (cf. Müller, Weisskopf) while causing increasing problems back home in the "mother country" (e.g., the second U.S. dollar devaluation, "runaway shops" which transfer jobs out of the higher-wage United States, inflation-spurring administered prices in oligopolistic industries, etc.).

And that's not all Chodak ignores or slights. His chapter on evolutionary theories implies that they are untestable and constitute a dying breed even in anthropology. He thus ignores all the recent conceptual and empirical work on societal evolution (e.g., Lenski, in sociology; Harner, Carneiro, Lomax, Naroll, Sahlins and Service, Fried, and Harris, in anthropology). He is all but silent on China and large areas of the Third World, including the Indian subcontinent and Latin America. Perhaps it's just as well, for the only two mentions of Latin America I noticed involved erroneous statements. First, he claims (p. 83) that the standard of living in the Soviet Union is no higher than that of Latin America—apparently excluding educational access, health care, and other social services from consideration, while providing data on average Soviet meat, dairy, and clothing consumption that in fact exceed anything I have ever seen for Latin America. Second, he asserts, ignoring counterexamples, that "the significance of military rule in Africa is different from that in Latin America, for instance. In Latin America upheavals are connected with social revolutions. Not so in Africa" (p. 274).

He is also almost totally silent about women. The book begins: "From the moment he became self-conscious, man. . . ." That semantic style

continues throughout, and almost the only reference to females is to note that in the USSR, "the wives and daughters of the workers . . . have still to wait long hours in . . . queues" (p. 105). He seems unaware almost all those wives and daughters are *themselves* workers (Soviet women form a *majority* of the work force).

Saddest, he does not realize that even nonradical economists are proposing measures of development that take income distribution and social welfare into account, as well as mere GNP or GNP per capita (e.g., Seers). On the most basic variable of the book, then, he ignores the fundamental question: development for *whom*?

*Peasants, Politics, and Revolution: Pressures toward Political and Social Change in the Third World.* By Joel S. Migdal. Princeton, N.J.: Princeton University Press, 1975. Pp. x+300. \$15.00.

*The Greek Peasant.* By Scott G. McNall. Arnold and Caroline Rose Monograph Series. Washington, D.C.: American Sociological Association, 1974. Pp. vii+112. \$2.75 (members); \$5.00 (nonmembers).

Daniel Chirot

*University of Washington*

The Migdal and McNall books treat a similar subject: peasants in poor countries. Migdal attempts to create a general theory of social and political change in peasant communities in Asia and Latin America. In general, he succeeds very well. McNall's monograph is limited mainly to observations about Greek villagers, particularly those in the two communities he studied. While Migdal, too, has done fieldwork, he uses it only to illustrate particular points. He also takes many examples from monographs by others and ties together nicely a great deal of the growing literature on peasant movements.

Although the two works are obviously quite different in terms of style and goals of research, that is not really what separates them. Rather, the key difference is that they fall on opposing sides of an old argument. McNall joins a long list of distinguished writers who believe in the efficacy of values in promoting social change or, conversely, in retarding it. Migdal puts peasants in a specific world context and shows that villages have distinctive class structures which are related to the class structures of the larger societies in which all villages exist. The interaction between world context and class structures explains the direction and extent of change.

Thus, for McNall, Greek peasants are poor and unable to advance themselves because they believe in a particularistic form of religion, because they are fatalistic pessimists, because they are jealous of each other and do not cooperate well with other villagers, and because they believe that whom you know counts more than what you know in promoting personal advances. In other words, they are decidedly not "modern."

Many examples of nonmodern, presumably maladaptive behavior are given. These show that Greek peasants do not think like rational, bureaucratic beings and are therefore entitled to full membership in the large club that contains Banfield's Southern Italians, McClelland's under-achievers, and Inkeles's traditionals.

For Migdal, things are quite different. The past several centuries, and particularly the past hundred years or so, have witnessed an irresistible attack against villages by the modern market and by the forces of the Western-dominated world capitalist system. Migdal labels these forces "imperialism," an evocative, but possibly overly emotional term. Formerly isolated villages, which either sealed themselves off from the outside world or were sealed off by their lords, who acted as the sole intermediaries between them and the outside, were thrust into the world and stripped of protection. The previous pattern of relative equality between villagers was broken as some gained from contact with the outside while most lost. Communal property, which had always provided a cushion for the village poor, was turned into private property and alienated to persons more interested in making profits than in preserving social equilibrium. Bitter internal conflicts broke out within villages as a new class structure was created by the new economic and political contacts with the outside world.

Villages have varied widely in their responses. Some have managed to remain relatively isolated. Others have broken down entirely and become mere dormitories in which children are raised and an unemployed or underemployed reserve labor force can be maintained without costing governments or enterprises too much in social expenditures. (That seems to be the situation of McNall's villages.) Yet others have emerged with new viable agricultural patterns. In some places villagers have become revolutionaries, while in others they have sunk into passivity. A number of variables explain the differences. Migdal's strong point is his careful discussion of these key variables and the explanatory power of each one. The quality of soils, the types of products grown, distance from markets, and the availability of new technologies all make an important difference, as do the date and rapidity of "imperialist" penetration and the outcome of the various class struggles it has set off.

But whatever the outcome, in almost all cases the majority of peasants are losers. Either they are driven off the land into the cities to become poorly paid workers or, worse, they remain stuck as poor rural proletarians to be exploited by the few who make the successful adaptation to the intruding modern world. Given this, the potential for revolution is high if the proper organizational framework can be created. A revolutionary organization faces the essential task of protecting peasants against the vagaries and injustices of the "imperialist" market.

Unfortunately, Migdal is a bit vague about the last point. Although he explains how revolutionary organizations succeeded in mobilizing peasants in China and Vietnam, he does not explain why this process occurred in some places and not in others. This is not a crucial flaw; Migdal's other main points are so well taken and so clearly explained that the book is

likely to serve as a major theoretical work in this area for a good many years. The work is an essential foundation to a theory of peasant revolution but not yet a fully elaborated explanation of the phenomenon.

Migdal's conclusions lead us in a quite different direction from McNall's. Most peasants, by the very nature of the transformation that hits them from the outside, get hurt. Nevertheless, all villages have some individuals who succeed in exploiting the new situation, either by leaving quickly or by staying in the village to dominate their less capable or lucky brother peasants. The notion that cooperation within the village or general adaptation to the modern world can help the village as a whole is doubtful. The only form of cooperation which can do this is political organization in order to *force* the intruding world to offer better terms. But no single village can make much of a difference; in fact, only a larger revolutionary organization can succeed in this endeavor.

*Police et politique au Québec.* By Guy Tardif. Montréal: L'Aurore, 1974. Pp. 494. \$13.95.

Everett C. Hughes

*Boston College*

"Contrary to expectations, the group most significant to the chief of police is not composed of his 'clients,' his 'peers,' or even his 'men,' but of 'his masters,' the politicians" (p. 39). "A good many chiefs maintained that they spent much more time 'fighting with the city council than struggling against crime'" (p. 44; my translation). Thus are named the characters in the work drama of chiefs of police in the municipalities of Quebec, ranging from small rural towns to the metropolitan greater Montreal. I daresay the United States counterparts of these French Canadians would tell much the same tale. The interviewer is fully one of them: a French-speaking native of Montreal who left school to join the Mounties. Somehow he managed a baccalaureate, mastered English, and saw Canada. Then he went to the school of criminology at l'Université de Montréal and read sociology—including Mead (*sic*) and almost everything on the sociology of work and careers. With this equipment he set out to study the police chiefs of his province, accepted as one of the boys, but also—with his six feet two and his reputation as a Mountie—able to keep his distance while retaining the confidence of the people he interviewed.

This is a fantastic formula for a field investigator. The trouble is that only once in a blue moon will such an investigator turn up to study any particular profession. While we should, of course, be training people who can study any occupation or social setting, we should also keep our eyes open for the rare individuals, those in-groupers who have developed the ability to combine their personal and professional experience with the social scientist's qualities of seeing the data from a certain distance, the distance necessary for conceptualizing and comparing. Tardif has succeeded in doing this.

The reader may let himself go. The account is lively, sometimes colloquial (the author lets the *bonshommes* speak as they do), but the frame of reference is always there. I think the book about equally important to students of social organization and to students of police systems and politics. The phases of the chief's career are worked out in some detail. Many get into the profession by accident. Some don't last long. The career contingencies are handled well.

Let English-speaking social scientists be aware that if they neglect Quebec's social science they are courting ethnocentrism to a crippling degree.

*The Uses of Mass Communications: Current Perspectives on Gratifications Research.* Edited by Jay G. Blumler and Elihu Katz. Beverly Hills, Calif.: Sage Publications, 1974. Pp. 318. \$17.50 (cloth); \$7.50 (paper).

Jane Trowbridge

*University of Chicago*

Those familiar with the functional approach to mass-communications research will recognize that the "uses and gratifications" model is not a newcomer to the field. In fact, the editors briefly trace its "childhood" in the 1940s and 1950s to "a sort of coming of age" in the 1970s (p. 13). In fairness to the practitioners of this approach, a final judgment of its utility should perhaps be suspended until it reaches full maturity.

E. Katz, J. G. Blumler, and M. Gurevitch preface this collection of articles with an overview of the uses and gratifications approach which will leave some readers with the uneasy feeling that something escaped them. As the authors explain, much of the research carried out in the last few years in the United States, Britain, Sweden, Finland, and Japan shares an interest in "(1) the social and psychological origins of (2) needs, which generate (3) expectations of (4) the mass media or other sources, which lead to (5) differential patterns of media exposure (or engagement in other activities), resulting in (6) need gratifications and (7) other consequences, perhaps mostly unintended ones" (p. 20).

This general scheme must be understood in light of five underlying assumptions. First, the audience is conceived of as active; that is, an important part of mass media use is assumed to be goal directed. Second, in the mass-communication process much initiative in linking need gratification and media choice lies with the audience member; for example, it is children that use television, not the television that uses them. Third, the media compete with other sources of need satisfaction; thus, the extent to which an individual's needs are satisfied by the media versus functional alternatives can be expected to vary greatly. Fourth, in respect to methodology, the audience members are considered to be sufficiently self-aware that they can validly report their interests and motives in their own media consumption patterns, or at least recognize them when presented

with possible alternatives. Finally, value judgments about the cultural significance of mass communication should be suspended while audience orientations are explored on their own terms. The model does not invite a revival of speculations on "low-brow" media fare (pp. 21-22).

To a certain extent, the uses and gratifications model can be said to have developed in response to the early preoccupation with "effects" studies and the unconvincing results they yielded. The upshot has been two distinct types of research. On one hand, interest has developed in audience needs and gratifications per se and the relationship they bear to an individual's psychological makeup and sociodemographic attributes.

A second, more promising line of investigation looks to the needs of audiences and their gratifications from media consumption as intervening variables in a more traditional "effects" model. It is certainly plausible that individuals in a position to benefit from information passed along by the media would show a greater tendency to be affected by the media. For example, in a study by F. G. Kline, P. V. Miller, and A. J. Morrison ("Adolescents and Family Planning Information: An Exploration of Audience Needs and Media Effects," pp. 113-36), no knowledge effects from a media campaign about family planning were originally found; such effects did appear, however, when account was taken of the needs of the audience for information. To date, relatively little research has been carried out in this vein, despite the fact that this would seem to represent the potentially greatest contribution of the uses and gratifications model to the general field of mass-communication research. For years, communication researchers have been perplexed by the data they have collected indicating that the mass media—constituting one of the most pervasive institutions in modern industrial society—have minimal effects on their audiences. One even suspects that some researchers have abandoned this line of research, not entirely from a resigned acceptance to the "no-effects" results, but perhaps in frustration that their research tools were not succeeding in teasing out the effects. If the uses and gratifications model eventually succeeds in contributing substantially to overcoming this impasse in communications research, it will certainly command a new respect among those within the field of communication.

The editors can hardly be accused of a biased, one-sided presentation of the uses and gratifications approach. To the contrary, they have invited their critics to scrutinize and expose the shortcomings of the model and of the empirical work to date. J. W. Carey and A. L. Kreiling, and P. Elliott come down heavily on the approach, the former because of its disregard of the cultural and symbolic aspects of media consumption, the latter for its incompatibility with conflict theory. Other criticisms raised by one or more of the contributors are that the functionalist bias of the approach is inherently conservative; that the approach is mentalistic and individualistic, failing to deal with the larger social structure; that the approach is atheoretical, drawing its structure from the empirical data; that the methods used to uncover the functions of the media may in fact be reconstructing reality. While this is hardly an exhaustive list, it does



suggest some of the major criticisms which should be answered in future work.

The organization of the book is sound: the preliminary overview is followed by a series of six empirical studies and, finally, eight analytical articles. But the overview suffers from the lack of a straightforward explanation of the approach. The editors acknowledge this problem of identity in the foreword to the book, though they believe the volume offers "a shared answer" to the simple question, "What in essence is this art of so-called gratification research?" Their answer is, "a research strategy that can provide a home for a variety of hypotheses about specific communication phenomena and a testing ground for propositions about audience orientations stemming from more than one sociological or psychological theory" (pp. 14-15). To all but the practitioners of the approach, this answer must surely seem ambiguous. The fault may lie with the eclectic nature of the work conducted to date, now being fused under one heading. However, until the diverse themes can be drawn together and presented to other social scientists as a more cohesive statement, the approach is unlikely to gain widespread recognition within the field of sociology.

*Scientific Knowledge and Sociological Theory.* By Barry Barnes. London: Routledge & Kegan Paul, 1974. Pp. x+192. \$12.25 (cloth); \$6.00 (paper).

Fred E. Kätz

*Tel-Aviv University and State University of New York at Buffalo*

This book is scholarly. It has an appealing thesis and an exasperating exposition.

The thesis is that the core activities of scientists are interlaced with social and cultural factors. These activities are as thoroughly linked with social and cultural forces as are biases, mental distortions, and irrational acts. Sociologists of knowledge, such as Mannheim and Merton, have shown social determinants to be at work in certain aspects of science. In particular, they have shown the influence of such determinants on the directions of science, some areas being cultivated and others neglected. But they have believed that certain of the scientist's activities are exempt from social determination. Objectivity itself and the unshakable pursuit of verifiable knowledge somehow remained uncontaminated by social and cultural forces. Barnes challenges this belief. He claims that even objective truth has its share of social determinants. Or, conversely, scientific objectivity can be retained even in the company of social and cultural factors. To a sociologist this is a welcome thought.

But the exposition is tortuous. The book contains insightful comments amid unrestrained philosophical excursions and in-house scholarly debates. We are offered the statements that "the greater part of the beliefs which constitute our accepted knowledge are readily shown to derive from *the-*

ories rather than being entirely the product of experience . . ." (p. 9), that "theory . . . cannot be held to *derive* from observation and experiment . . ." (p. 49), and that science "can no longer be seen as commonly validated by a uniquely rational method" (p. 45). But we are also obliged to read about metaphorical ways of thinking, about internal and external contingencies in ongoing science, about ideology and beliefs, about functionalism, and more.

The book is not short of ideas, yet no idea becomes very compelling. This may be due to the author's taking one of his own ideas too literally. He claims—rightly, I think—that in science empirical evidence has been highly overrated, especially as a source of theory. But this claim ignores the other side, the fact that rational argument coupled with fleeting, here-and-there use of empirical data cannot produce convincing scientific results. Only when imaginative inputs are coupled with disciplined and orderly use of data can one expect to produce persuasive scientific results.

In short, the book is firmly within the "European variant" of the sociology of knowledge, of which Merton despaired so long ago but which continues to supply us with tantalizing perspectives.

*Deviants and Deviance: An Introduction to the Study of Disvalued People and Behavior.* By Edward Sagarin. New York: Praeger Publishers, 1975. Pp. xi+458. \$8.95 (cloth); \$2.95 (paper).

Frank R. Scarpitti

*University of Delaware*

Although the study of deviant social behavior is as old as American sociology, only within the past 20 years has it emerged as a distinct field of study. During this period, a number of critical articles and monographs have changed sociological thinking regarding deviance from a traditional social-problems orientation to a new concern with the process of deviant action and ensuing reaction. This change has been reflected in deviance textbooks as well. Earlier texts were concerned primarily with particular types of behavior, such as crime and mental illness, which happened to fit into the author's definition of deviance. Recently, however, texts have rejected this format and reflected the discipline's greater concern with a more conceptual and analytical approach. Instead of containing chapters on specific types of deviance, such books focus on the exposition of an analytic framework or the analysis of specific considerations relevant to deviance. Sagarin's text attempts the latter.

This book is divided into six major sections or long chapters. The first deals with issues surrounding the definition and dimensions of the concept of deviance. It is here that the author presents his case for viewing the study of deviance as the study of disvalued people and behavior. In section 2, various theories and explanations are reviewed and evaluated. The presentation is particularly well balanced and provides an excellent overview and critique of both sociological and nonsociological attempts to un-

derstand social deviance. Section 3, "Deviance and Sickness," asks the question, "Is the deviant sick?" While providing the reader with an answer (yes and no), the author presents an interesting discussion of the relationships among deviance, mental illness, and disability. The fourth section examines several methodological problems in the sociological study of deviance, including the issue of research ethics. Section 5, "Survival Patterns and Social Control," is concerned with ways in which society deals with deviants and ways in which deviants deal with society. A substantial portion of this chapter appears to be based on the author's earlier book, *Odd Man In*. The final section deals with a number of social-policy issues, decriminalization, victimless crimes, and the relationship between law and morality. In addition to 413 pages of text, the book contains a guide to the literature in the form of the contents of some 20 recently published readers on deviance.

As a text, billed as an introduction to the study of deviants and deviance, this very long and sometimes tedious book presents some problems. For instance, there appears to be no logic to the organization. Each section stands as a separate essay with no particular connection to the others. Hence the content is not cumulative, with one concept and construct building upon another. One cannot help wondering why the author chose the topics which are included and not others. We have no way of knowing. A related problem is perhaps even more serious. Concepts important to an understanding of social deviance and about which there is little misunderstanding among professional sociologists are sometimes used in a casual, even idiosyncratic fashion. On page 32, for example, the student is told, "If one were to use the system of categorization developed by Robert Merton (1938) in his famous paradigm, the concepts might be differentiated somewhat as follows: 1. Predatory acts . . . , 2. *Mala Prohibita* . . . , 3. Law-abiding deviants and deviance . . . , 4. Normative acts . . . , 5. *Mala in se*, when not illegal. . . ." Following each concept are pluses and minuses under columns headed "crime" and "deviance" and examples of each. This, unfortunately, is the first mention of Merton's "famous paradigm," the fame of which may not have spread to students in introductory deviance courses who conceivably have not yet heard of conformity, innovation, etc. To read this book with understanding, the student should have prior knowledge of original material in order not to be confused by the author's interpretation and modification of it.

The book could have benefited from more serious consideration of alternative definitions of deviance. Although the author's definition is quite traditional, alternatives have been proposed, but they are given little consideration here. Although the arguments of a number of people are subtly interpreted to support the theme of disvalued people and behavior, the interpretations are sometimes questionable. But that is probably just another symptom of what I find most objectionable about this book: its moral tone is arbitrary and rigid. Though greatly concerned about research ethics the author believes it legitimate to use covert methods to study some groups but not others; he criticizes the labeling perspective

for its extreme relativism; he finds some groups clearly deviant and others, though so labeled, clearly not; and he believes that "some acts are inherently so evil that they must be outlawed, whether or not the acts or their illegalization meet public support" (p. 382). The fervor of some of the author's value positions may, unfortunately, cause readers to reject the book's many worthwhile qualities.

The greatest value of this book lies in its attempt to deal with conceptual and methodological issues often ignored in deviance texts. The author's discussion of several such issues is unusually good, for example the role of the labeling perspective in contemporary deviance theory and methodological problems in studying deviants. Although no new theory is presented, the book contains a number of insightful statements and thoughts which will cause the reader to ponder many preconceived notions about deviance and which should stimulate lively discussion and debate.

*Urban Renegades: The Cultural Strategy of American Indians.* By Jeanne Guillemin. New York: Columbia University Press, 1975. Pp. x+336. \$10.95.

*Indian and White: Self-Image and Interaction in a Canadian Plains Community.* By Niels Winther Braroe. Stanford, Calif.: Stanford University Press, 1975. Pp. x+206. \$8.50.

*Success and Failure: Indians in Urban Society.* By W. T. Stanbury, assisted by Jay H. Siegel. Vancouver: University of British Columbia Press, 1975. Pp. xxxii+416. \$17.95.

Murray Wax and Rosalie Wax

*Washington University*

Together, these three monographs begin to provide a rounded portrait of Indians in modern Canadian and United States society. Individually, each study has natural, as well as conceptual and methodological, limitations. A married woman, Guillemin engaged in fieldwork with the Micmac in Boston and Nova Scotia; mostly, she worked alone, but toward the end of her project she was accompanied by her twin sons. A married man and "city boy," Braroe engaged in intermittent fieldwork among a small band of Plains Cree in a rural area of Saskatchewan; occasionally he was assisted by his wife, a linguist. Stanbury directed a survey research team which interviewed 1,000 Indians residing off their reservations in the urban centers of British Columbia. Guillemin organizes her account with a conceptual vocabulary deriving from the fields of social organization and racial relationships, augmented by a strong tinge of radical sociology. Braroe organizes his account within a dramaturgical framework. Stanbury prefers to work with "hard data" and eschew speculation, so his findings are presented with a minimum of interpretation.

Guillemin writes fluently, forcibly, often polemically. She tells almost nothing of the basic facts of when and where she conducted her fieldwork,

except that it is clear that she attempted to participate in Micmac social life and thereby she and they had problems with her sexual role (because she was an isolated woman who rejected masculine attachment). Nevertheless, hers is the best description in the literature of the roles and careers of modern Indian women. Integrated therewith are both a description of the roles and careers of the Micmac men and an analysis of how the whole system of social interactions—including partying, drinking, flirting, brawling, and shacking up—functions to establish networks of communication and assistance among an impoverished people scattered over a wide territory amidst an alien if not hostile environment. This analysis is a contribution to the discussions of poverty and its “culture” and, equally, to the burgeoning field of network theory. We only wish that Guillemin could have the funds, energies, and time to continue her work among the Micmac in order to refine and test her analyses; especially now it would be relevant to observe Micmac encounters with non-Indians and to interview the corporate agents (welfare workers, personnel officers, police, teachers, et al.) with whom they must interact so often. Some of her polemic might then become more focused, and much of the rest might be discarded.

Braroe is candidly sentimental, perceptive, and witty in his description of his fieldwork, which totaled almost two years and involved three separate trips during the 1960s and early 1970s. The project grew upon him, and he seems to have become fascinated with how the Cree and their white neighbors engage in reciprocal strategies to manipulate, deceive, and defraud each other. Perhaps even more than the Micmac, this band of Plains Cree maintains the native language as the primary means of domestic communication; likewise it practices a rich cycle of religious and ceremonial observances. Yet one aspect of the interracial deception is that native language and ritual are outside the awareness of neighboring whites, who scorn the Cree as being no longer Indian (in the traditional sense which whites recognize) without having undergone the transmutation into individualistic and industrious whites. Thus their neighbors see the Cree as deracinated and irresponsible, while the Cree reciprocate by regarding whites as ungenerous, mean, and irreverent. The dramaturgical system would appear to be useful for presenting and interpreting these interactions; but, because the Cree have their own language and ritual system, they inhabit a distinctly different symbolic world and have a different sense of selfhood from their white neighbors. Hence a dramaturgical analysis of Cree-white interactions requires a complex process of interpretation between two symbolic worlds. Accordingly, we wish that Braroe could have the funds, energies, and time to continue his work among the Short Grass Cree in order to refine and test his analyses; it would be especially useful if he could develop a high fluency in spoken Cree. For we are not so sure as he is that “the subject of this book has to do with the adjustment of Short Grass Indians to the fact that their moral worth is denied by Whites.” As he presents them, these Cree are

engaged in a rich and autonomous process of communal living instead of simply responding to white disparagement. But we can offer this criticism precisely because his book contains so many vivid descriptions and frank discussions.

In the heated atmosphere of Canadian discussions of Indian affairs, Stanbury chose to make his contribution via the discovery and presentation of the facts about the social and economic conditions of urban Indians in relation to the dominant society. The questionnaire he used was designed to collect information on demographic facts, employment, income, education, housing, geographic mobility, urban career, language and culture, health, and social life. The population to be surveyed was "status Indians" (ones recognized by the government) who were adult, resident in British Columbia cities, and recorded on B.C. band lists. This target population was estimated at 6,700, and the sample interviewed was 1,095 (not precisely random, but a good try under difficult circumstances). Many of the 288 pages of text are rather prosaic translations into words of a set of tables that occupy an additional 100 pages. Under the circumstances, the text is not light reading, nor is it easy to summarize, and it will likely find its major use as a reference work. Nevertheless, we may find a comparison between Stanbury's findings and those of the fieldworkers.

As all three researchers would agree, the central economic problem of the Indian peoples studied is the limited land base of the reserves. The small acreage of fertile land means that only a modest population can be supported therefrom, and since the Cree and the Micmac have no (other) capital, limited vocational skills, and negligible political clout, they suffer considerable hardship. Securing only minimal welfare assistance from the Indian Affairs Bureau, they are forced into the labor market as proletarians, rural or urban, performing casual labor. The question can then be raised, how much of the present styles of living of these Indians can be traced to (a) traditional culture and ethos, (b) their condition as proletarians, or (c) their experiences with racism and discrimination? Guilemin and Braroe tend to emphasize the last alternative, arguing that because Indians are the objects of derogation and disparagement by the members of the dominant society—especially its corporate representatives (police, welfare workers, etc.)—much of the way that Indians comport themselves is a response to a racist social environment which demeans them. This contention of the fieldworkers contrasts sharply with the finding of Stanbury (p. 399, tables 3-9, 3-10). His B.C. Indian respondents were asked how they were treated by non-Indians in stores and cafés, in hotels, and in government offices, as well as by landlords and the police. From these responses, a Perceived Discrimination Index was constructed which could range from a minimum of 0 to a maximum of 5. Of his sample, 49% scored 0; 37% scored 1; 13.4% scored 2; and 7.7% scored 3 or more points. Evidently, Indians in the cities of British Columbia do not perceive themselves as being the objects of discriminatory treatment;

and, if this finding can be generalized to other Canadian (and U.S.) Indians, then the two fieldworkers have not accurately reflected the attitudes of the peoples studied.

Insofar as there is a discrepancy between these findings, it cannot be resolved into a difference between methodologies (fieldwork versus survey research), since from fieldwork among the Sioux and the Cherokee during the 1960s, our own research teams came to conclusions similar to Stanbury's. We had anticipated that Indians would be aware of the derogatory attitudes of many local whites and that such awareness would strongly affect their conduct. Instead, we found that Indians were preoccupied with struggling to survive under conditions of great hardship, and that, beyond this, they were greatly concerned with the dramatic intricacies of their communal existence. Since they appraised most whites as childish, irresponsible, and mean spirited (although sometimes smart), Indians did not seem to care whether whites admired or denigrated them, except insofar as such attitudes impinged on their own immediate plans of action. Instead of caring about whether or not they were ranked as equal with whites, these Indians were threatened by the notion that they might be considered identical with them. Sioux and Cherokee defined themselves as peoples wholly distinct from whites (so that the liberal vocabulary of discrimination was irrelevant). As distinct peoples, they had special relationships with the United States government (and the universe as a whole), and a predominant concern was that the government should recognize and fulfill its share of the obligations due them. Meanwhile, though living under hardships, Sioux and Cherokee did not consider themselves poor; and they coped with their difficulties by social instrumentalities which were of respectable vintage.

On the matter at issue, neither Guillemin nor Braroe presents evidence which is pertinent or good. Guillemin's explicit words (p. 73) are more tempered than the general tone of her book: "Racist prescriptions and proscriptions make the dominant society appear as the chief perpetrator of minority culture. My own belief is that the human tendency for cultural diversity is shaped rather than created by racism." But this statement is neither documented nor directly elaborated. To confirm his thesis of how Indians are demeaned, Braroe describes a number of incidents, but most show that the whites of the town of Short Grass are contemptuous of Indian drinking and that—when necessary—they communicate this derogation as they seek to evade Cree solicitations for handouts. Most of these solicitations are rude and nagging intrusions, whether judged by the etiquette of Plains white men or traditional Indians, and the system of interaction could be analyzed more appropriately in the context of the study (or sociology) of Indian drinking than in that of racism and discrimination. Braroe does also argue that the covertness with which the Cree employ their language and conduct their ceremonials is a "covering" in response to disparagement, but for this behavior there are explanations other than a dramaturgical concern with "profanation of the self."

This review is not the place for a protracted and painstaking compari-

son of studies of modern Indians. Criticism, debate, and more research are required. Given this state of affairs, it might seem that we were too sanguine in asserting initially that conjointly these three studies provide the basis for a rounded portrait of modern Indian life. The point is that students can now begin to advance beyond speculation and political sloganeering toward pertinent and patient social analysis.

*God's Blueprints: A Sociological Study of Three Utopian Sects.* By John McKelvie Whitworth. London: Routledge & Kegan Paul, 1975. Pp. v+258. \$23.00.

William Sims Bainbridge

*University of Washington*

*God's Blueprints* is an intelligent and sociologically useful description of three utopian movements: the Shakers, the Oneida Community, and the Bruderhof. Although Whitworth conducted a brief ethnographic excursion into the contemporary Bruderhof, he relies almost exclusively on published sources, both historical studies and the publications of the sects themselves. His book is of value either as an informative introduction to these groups or as a supplement to the various other books and articles currently available. It provides a clear survey of the historical material and contributes a number of interesting analytical insights.

The 19th-century Shakers and Oneida Perfectionists and the 20th-century Bruderhof Anabaptists have been the focus of considerable interest in recent years. Among the most unusual and thoroughgoing of utopian experiments, their communities serve both as standards for the construction of new ones and as natural laboratories for the testing of social-scientific theory.

Whitworth calls these groups "utopian sects," by which he means "religious groups which partially retreat from the world with the avowed intention of eventually restructuring society according to a better model" (p. 239). He finds a basic tension between their tendency to retreat into an introverted isolation from the sinful larger society and their desire to expand into it as evangelists and saviors. In his detailed presentations of the three movements, he examines closely the pressures in both directions. He emphasizes the interactions between them and the outside world, since both sides of their ambivalent motive seek to establish particular relationships with outsiders.

In the long sections on Shaker and Oneida history, Whitworth analyzes the groups' responses at first to apparent success, then to obvious failure. Both came ultimately to feel that they set very important examples for the world, even if they were unable to reform it. It is not clear that the Bruderhof is really the same order of phenomenon as the other two. Like them, it is organized as a separate, communal society with a specific religious vision and a theocratic authority structure. However, its founder, Eberhard Arnold, was not a charismatic figure in the same sense as the



Shakers' Ann Lee and Oneida's John Noyes. Lee and Noyes created radical social ideologies by the projection of very personal needs and conflicts, whereas Arnold led a group of like-minded people in the construction of a fundamentalist subsociety that drew from pervasive traditions in German religion.

Whitworth is at his best when describing the details of historical development and styles of social interaction highlighted by the special features of these groups. For example, there are several brief but revealing discussions of the effect of psychological and economic dependence on their growth and decline. Dependent persons make dedicated but often unproductive members, and the passivity required of good members in stable periods of a sect's history may serve it ill during times that produce or demand change. Whitworth's discussion of the long debate between Shaker traditionalists and social reformers on how to reverse the movement's decline is particularly interesting.

The lucid writing in *God's Blueprints* is facilitated by the author's analytical scheme which, however, is also the book's greatest limitation. Following the classical tradition of E. Troeltsch, H. R. Niebuhr, and B. R. Wilson, Whitworth attempts to use ideal types such as "utopian sect" as analytical concepts, when they are at best orienting terms. For such innovative phenomena as Oneida, even the word "sect" is only a metaphor. One avowed purpose of Whitworth's book is to develop further the system of ideal types invented by his predecessors. The actual effect is to undercut the system by showing that it is inadequate to encompass his rich data and interpretations.

Whitworth presents much of the psychohistorical analysis essential to an understanding of Oneida and the Shakers. His many brief discussions on the microsociological level constitute adequate social psychology and would have benefited from a recognition that they were the heart of his study and deserved an overall theoretical framework to match. The movements are fundamentally social and only superficially religious.

It is a tribute to the care and skill with which *God's Blueprints* was written that sociologists from a number of schools can apply their own systems of analysis to the wealth of material contained in it.

*Solidarity in a Slum.* By Joseph Tamney. Cambridge, Mass.: Shenkman Publishing Co., 1975. Pp. 182. \$10.00 (cloth); \$4.50 (paper).

Jerold Heiss

*University of Connecticut*

Let me begin with a warning. The potential reader who is drawn to this book by the publisher's description of it is bound to be disappointed. The jacket blurb notwithstanding, this is not a "study in which philosophic tenets and speculations are brought to bear on problems of everyday life." Joseph Tamney does not postulate "cogent and humane theories on topics ranging from marital life and child raising in the family, to the politics

of the community, neighborhood and city organizations, and religious structures." It is untrue that a full airing is given to "the slum residents' concern for the dilapidated physical appearance of their community and the resulting social friction." The product is not as advertised.

With this said let us turn to what the book actually is, for I certainly do not want to evaluate it in terms of a copywriter's statements. Simply stated, Tamney reports, for a sample of 623 women living in a Milwaukee slum area, the distribution and correlates of several forms of what he calls "involvement."

In the first chapter involvement is defined as a "sense of oneness" (p. 1). This sense may be viewed positively or negatively by the subject: because a person is involved with another or others does not necessarily mean that he or she likes the other or that he or she is happy with the situation. I got the idea that one is involved when there is a feeling that "we" are all, for better or worse, "in the same boat."

The rest of chapter 1 is devoted to the presentation of a theory of the sources of involvement, a theory which contains such postulates as "the extent of involvement depends on the amount of similarity, power, and knowledge" (p. 7), and "the extent of involvement depends on the physical distance between objects" (p. 6). Some of the postulates are interesting and have a ring of truth to them, but in general the effort at theorizing must be deemed less than successful—too frequently the justification or explanation provided for the postulated general relation does not adequately put curiosity to rest.

But no matter, for we hear almost nothing further about the theory. As the author notes, most of the propositions are not tested by the research. Contrary to what Tamney says, furthermore, knowledge of the theory, whose value is simply assumed, does not help the reader understand the choice of issues studied. Few of the independent variables seem to have any relevance to those in the theoretical postulates.

After a general description of the area and people studied, there follow four chapters concerning involvement with others in general and, more specifically, involvement with family, neighborhood, and nation. Tamney presents marginal distributions as well as material describing intrasample variation. The independent variables used most frequently are age and race, but attention is also devoted to geographical mobility and, to a lesser extent, household composition, marital and work status, etc.

Despite the lack of a coherent organizing principle, these materials are of potential interest to sociologists studying the social organization of slums. The reader's reaction to them will depend, however, on the degree to which he or she accepts the validity of the operational indices of involvement. My reaction is one of extreme skepticism. Understandably, there are no formal validity tests, but I feel that many of the indices lack even face validity. To give a few examples: Is involvement as defined by the author indexed in answer to the question "Who are the three people you think about the most?" Are persons who think about dead people, or people they are not in contact with, alienated (read "uninvolved")? Given

the fact that involvement can be negative or positive, is it appropriate to use the question "What makes you feel proud?" to elicit an index of involvement if, as the author recognizes, at best this indicates only positive involvement?

I do not wish to insist on the validity of this criticism. Perhaps I am being too literal or hard-nosed. I suspect, however, that many other readers will conclude as I did that to a large extent the author is not measuring what he says he is. They will also end up wondering what is the common thread in the many indices used.

To end on a more positive note, I would mention that the chapter on national involvement is the strongest in the book. There are measurement problems, to be sure. When a factor analysis failed to produce a factor that the author felt could be considered national involvement, he simply chose the item which, essentially, asked respondents whether they felt proud, indifferent, or angry when they heard people praise the United States. Also, I am unconvinced that voting should be considered a sign, rather than a consequence, of involvement. Nonetheless, in this chapter Tamney does a searching analysis on several independent variables—region of birth, mass-media contact, size of birthplace, voluntary association membership—which seem to be of greater relevance to his theory than the ones emphasized earlier. I would look forward to additional work on the subject matter of this chapter.

*The Effects of Income on Fertility.* By Julian L. Simon. Chapel Hill: Carolina Population Center, University of North Carolina, 1974. Pp. iv+210. \$5.00.

Judith J. Friedman

*Cook College, Rutgers University*

The research touching on fertility change is voluminous and multidisciplinary. This makes reviews which organize the findings of numerous studies around a common theme extremely useful. Julian L. Simon provides such a review in this Carolina Population Center Monograph. Simon, an economist, focuses on the question: How does change in national income affect fertility? Assuming economic conditions are the central exogenous variable, he then distinguishes between their short- and long-term effects. In the short run, he argues, increasing income means that families have, on the average, more resources; when other conditions are constant, this means more children. Over long time periods, however, an increasing national income means far-reaching changes in the society's structure. These structural changes ultimately mean smaller families.

Simon combines the distinction between short and long time periods with that between "more developed countries" (MDCs) and "less developed countries" (LDCs) to designate four different situations, and then discusses the impact of income change upon fertility for each situation in

turn. He begins with short-term effects in MDCs, citing both time-series analyses and cross-sectional studies. The cross-sectional research presents a problem: simple cross-sections rarely show a positive relationship between income and fertility. Simon points out that such tables mix short-term effects with long-term ones and argues that we need fine cross-classifications. In support, he presents some original analyses of 1960 *U.S. Census of Population* data. He then briefly considers the short-term effects in LDCs. He concludes each chapter with a discussion of incentive payments.

Simon next considers the long-term effects of income change for LDCs and then for MDCs. His argument that the major long-term impact is indirect, through various structural changes, opens up vast amounts of research concerning LDCs. He focuses on three changes: urbanization, rising education, and declining infant mortality. Long-term change in the MDCs presents the opposite problem: that of little research. I found the time distinction least clear in this chapter. The monograph concludes with a chapter summarizing the preceding four. There are, in addition, three appendices, two of them about difficulties in examining change using cross-sectional data.

Simon's sources are extensive, although, understandably, not exhaustive. The stress on economic issues and hence on economic research increases the monograph's value to noneconomists; and I appreciate the fact that he reproduces many tables, some from obscure sources. Most readers will learn of interesting studies. In addition, the organization of the literature brings out several important ideas, such as (a) the value of studying the impact of income change (and other variables) separately for couples at each parity and (b) the need to refine the prediction that, in the short run, higher income will mean higher fertility.

Despite all this, I found aspects of the monograph disappointing. First, the literature review itself has, of course, places needing expansion. In particular, Simon does not review in depth the growing number of detailed case studies about the European fertility decline, and his discussion of this early decline suggests a simpler and more uniform process than such case studies reveal. Second, I missed seeing more concern with the differences among societies, especially among LDCs. Third, Simon uses the microeconomic theory of fertility behavior as an organizing framework, but his discussion of its central concepts is scattered through the monograph; readers unfamiliar with the theory are likely to find this confusing. Further, I found his definitions of some concepts unsatisfactory. For example, he defines taste (p. 107) as "a preference that stems from the individual's *own* experience, rather than being suggested or dictated to him by the group, as are values and norms." While tastes and norms certainly are not identical, the concepts do overlap. Similarly, I am puzzled by his assertion that the pleasure from children will be "invariant over time" (p. 108). Finally, certain sections of the monograph are not written clearly (e.g., pp. 74-75), and there are some annoying errors in table headings.

In summary, Simon's monograph provides a convenient and organized review of many scattered studies which relate income to fertility. It will

be a useful reference work for people concerned with numerous aspects of fertility change, and it should stimulate refinements in theory.

*Workers' Participation in Industry.* By Michael Poole. London: Routledge & Kegan Paul, 1975. Pp. x+198. \$15.00.

Lee Braude

*State University of New York at Fredonia*

In a study that is at once synthetic and hortatory, Michael Poole contends that attempts to maximize workers' involvement in and control of the determination of the conditions of their work have not been fully successful because they have been insufficiently informed with a sense of the sociological. To remedy this lack Poole proposes to "concentrate on participation [i.e., involvement in and control over the conditions of work] as a dependent rather than as an independent variable and by so doing provide a rather more reliable guide than has hitherto been available to the proposals most likely to achieve viable democratic institutions in the workplace. To this end . . . we seek to clarify the nature of power and its relation to the issues of participation and control. The central proposition to be developed here is that workers' participation and control is a function of certain underlying or latent power forces and a climate of values which may or may not be conducive to evolution along these lines" (p. 9).

Chapter 2 develops this assertion. Any distribution of power is legitimated through the manifest power of the parties to a relationship, through "latent power," the development by the parties of power bases or sources of sufficient potential to effect a redistribution of power when required, and through the generation of a framework of ideology and values that lends symbolic credence to or rationalizes the drive toward superordination. After examining assumptions implicit in the work of some proponents of participation as well as some who denigrate such efforts, Poole moves "on to lay the foundations of an explanatory model which will bring our analysis 'out of Utopia'" (p. 36). Following a scheme of analysis provided by G. S. Bain (*The Growth of White-Collar Unionism* [Oxford: Clarendon Press, 1970], pp. 184-88), the model consists of three propositions: (1) Workers' participation and control are functions of latent power and values. (2) Latent power is a function of economic and technological factors as well as political action of both government and workers. (3) Values relative to worker participation and control are functions of existing levels of participation, latent power, political action, and ideologies.

Chapters 3-6 constitute a "test" of the interpretive efficacy of the model. Data are drawn largely from the European work world. Chapter 3 examines proposals for intensified worker involvement initiated by management. The author concludes that a managerial ideology oriented to the professionalization and institutionalization of particular managerial ex-

pertise militates against worker control. However, as worker power has increased, management has grudgingly surrendered at least some of its prerogatives in the decision-making process. And the author expects that managers (particularly those at low levels) will be willing to relinquish even more power in return for higher output and freedom from conflict.

Chapter 4 examines attempts at participation initiated by workers themselves, whether directly or indirectly through "representative participation." It is clear to the author that "despite diversity of origins, of form and of content of these particular schemes, very similar causal elements can be adduced to explain their principal characteristics" (p. 118). He finds the main elements of his model confirmed again, along with "the . . . derivative argument that a rise in the level of latent power of working people does induce mounting expectations among them in the course of which questions about workers' participation and control attain a new significance" (p. 118).

Chapter 5 relates the attitudes of labor organizations and their officials to worker participation within the proposed explanatory frame. To the degree that union ideologies and attitudes of officials are congruent with democratically oriented workers who could serve as a focus for union support (latent power), unions will support increased worker participation and control—but only if such involvement will not detract from union officials' efforts to maintain and extend their own influence. Consequently, Poole asserts, few participation approaches have stemmed directly from union officers.

Chapter 6 considers the role of political parties and government intervention in facilitating worker involvement and control. The author asserts that an ethic of worker determination cannot be instituted on a permanent basis by the workers themselves, by the organizations that represent them, or even by both acting in concert. However, although legislation has fostered "a climate of opinion conducive to democracy and has impinged on economic forces and power balances which are fundamental in workplace relations, its *direct* relevance for participation is somewhat questionable. What is more, these statutory provisions can themselves be understood as specific responses to contemporary conditions and their success [seems] ultimately to hinge on those key elements . . . identified in this study" (p. 161).

Chapter 7 presents both conclusions and prospects. The model is confirmed; thus runs the conclusion. Prospects follow logically. Only participation schemes that augment the latent power of workers and stimulate values conducive to participation can survive. Transformations of business organizations, particularly the proliferation of the multinational corporation, may encourage cooperation across frontiers and engender collapse of paternalistic business practices. Greater participation of governments in economic planning may foster increased worker participation. Values and ideologies may, of course, be less responsive to direct attempts at change, but changes in them too may eventuate so long as a high standard of living can be maintained.

Professor Poole strains to make a case. That he does so attests more to his ability to marshal his data than to either the explanatory power of the model he presents or the cogency of the analysis. But because his book brings together considerable material on worker involvement and determination that may be unfamiliar to the non-European student, it merits attention.

*Cause and Meaning in the Social Sciences.* By Ernest Gellner. Boston, Mass.: Routledge & Kegan Paul, 1973. Pp. vii+228. \$10.00.

William F. Woodman

*Iowa State University*

*Cause and Meaning in the Social Sciences* is a collection of five long essays, five short papers, and five book reviews, all of which have been previously published. The papers are not in chronological sequence, but instead are arranged (as I. C. Jarvie and Joseph Agassi suggest in the preface) in accordance with Professor Gellner's developing "philosophical system," the shape of which is altogether unclear to me. The preface and the introduction stress that the book is aimed at a general social science audience, although references are primarily to the field of anthropology and (less frequently) sociology.

The two essays most closely related to the topic of causality are "The Concept of Kinship" and "Ideal Language and Kinship Structure." In the former, the author details some causal links between lineal descent systems and biological, physical, and extrasocial influences on social structures. In the latter paper, the reflexivity between social structure and the logical structure of language is explored, in much the same manner, incidentally, as Ward Goodenough's analysis of Pawnee kinship structures. The attempt to specify a "generic level" of social organization occupies the author in "Explanation in History" as well as in "Sociology and Social Anthropology," which takes as a starting point a distressingly narrow conception of sociology.

Issues not related directly to causation, but to the craft of the social sciences, are explored in "Concepts and Society." Here the basic problem is defined as the translation of the subject's conceptual reality into a functionalist model. Gellner is not altogether consistent in his treatment of phenomenology, however, for in some places he seems to be in sympathy with a Peter Berger and Thomas Luckmann—like approach while in other places he is plainly critical of that same perspective for its lack of methodological rigor. The article "The New Idealism—Cause and Meaning in the Social Sciences" laments the fact that social scientists (unlike physical and biological scientists) must conform to the expectations of their subjects. Other essays deal with such wide ranging topics as the role of philosophy in sociology, reviews of books by P. A. Sorokin (favorable) and Werner Stark (unfavorable), and the role of temporality in social theory and historicism.

The most serious shortcomings of the book are these. First, there is really very little that is new brought to bear on old debates, although it may be that time has simply tarnished the essays' novelty. Second, there are many issues raised, lamented, and then left unresolved; but then again, that may be a function of the dialectical and unresolvable nature of the subject matter. Finally, the book's most serious problem is that it is not genuinely a consistent treatise on causation for anthropology, much less for the social sciences. Granted, it does raise some key questions concerning the genesis and functions of generic social organization, but the analysis is piecemeal, disjointed, and, by now, largely dated. The book must be read, though, with the last point firmly in mind, for substantive discussion of causality is not the main business of the book.

*The Black Crusaders: A Case Study of a Black Militant Organization.* By William B. Helmreich. New York: Harper & Row, 1973. Pp. x+186. \$2.95.

Chaim I. Waxman

*Livingston College, Rutgers University*

Reflecting upon the United States of the late 1960s, one can hardly but take note of the extent to which the American public feared a phenomenon about which it knew so little. With all the media coverage of black militant organizations, there was an incredible paucity of reliable data about the makeup and goals of any of them. William Helmreich's book helps fill this knowledge vacuum by providing us with a detailed study, based upon personal observation, of the formation, structure, beliefs and goals, and ultimate disintegration of one such organization in "Central City," "a large city located somewhere in the United States" (p. 34). The organization, the Black Crusaders, had a brief life span of six months, August 1968–February 1969, and the author had the unique opportunity to be intimately associated with the leaders of the organization for most of that period.

The Black Crusaders was a grass-roots organization; most of the leaders and the approximately 200 members were lower-class blacks from Central City. The major stimuli for the group's formation were the conditions of despair and frustration in the black slum; the feeling among the leaders of the need to develop black consciousness and pride; the influence of the national black movement; and the leaders' hostility toward the police, whom they accused of brutalizing blacks. The group had a paramilitary structure, complete with a prime minister, generals, majors, etc., and uniforms, marches, drills, and training in self-defense. For females, there was a sister organization whose major activities were community services.

One of the most interesting and important chapters in the book is that which deals with the beliefs and attitudes of individual leaders, the official goals of the organization, the discrepancies between the two, and the effects of these discrepancies upon the organization. The ideology of the



Black Crusaders was based upon a radical critique of the oppressive Capitalist system in American society. Yet their approach was not revolutionary, in the sense of trying to violently overthrow the system, but reformist: they felt that the most effective method was to gain political power and change the system from within. This they hoped to accomplish through the establishment of a black political party. To this end, which was never realized, they supported and campaigned only for those political candidates who they felt would act in the interest of the black community.

The Black Crusaders were well aware of many of the difficulties in gaining political power through the existing political process. They felt that many of their problems were essentially the same as those of the black movement in the nation as a whole, and at least some of the leaders of the organization threatened to use violence should they fail to achieve their objectives through the officially legitimate processes. But, as Helmreich shows through interviews with a number of leaders, there was disagreement among them as to the wisdom of employing "any means necessary," indicative, perhaps, of a fundamental ambivalence concerning the whole matter of violence. Moreover, the discrepancy between personal attitudes and official rhetoric was a manifestation of frustration resulting from an inability to make meaningful changes. It is also possible that they engaged in inflammatory rhetoric as a means of pressuring the city administration into making improvements in the black ghetto.

A major area of concern which, according to Helmreich, resulted in the organization's demise, was the police issue. The Black Crusaders, like many black ghetto residents, were fearful of and hostile toward the police. The police were seen as the enemy, and some of the leaders called for an end to the patrolling by police, white or black, of black communities. The Crusaders saw themselves as the protectors of the neighborhood, especially from the primary enemy, the "racist cops"; Helmreich suggests that one of the reasons for the paramilitary activities of the Crusaders may have been rooted in their self-image as protectors.

The attitude of the police toward the Crusaders was, to say the least, not favorable, and Helmreich relates an incident where he personally was mistreated by the police because of his association with the Crusaders. One of the two major reasons suggested for the disintegration of the Crusaders was the hostility of the Central City power structure and its use of the police as a tool for verbal and physical harassment of the organization. Surprisingly, while Helmreich does identify lack of support by the community, including the black community, for the organization as the second reason for its dissolution, he implies that the problems with the police were the primary cause. Perhaps his personal involvement with the organization and his personal confrontation with the police because of his association with the Crusaders did not permit him to weigh adequately the implications of the community's attitudes, which ranged from indifference to hostility. From the evidence presented, it appears highly doubtful that the Crusaders would have fared much better even had the police and city officials been less unfavorably disposed toward them.

In his "Methodological Note" at the end of the book, Helmreich briefly discusses his reasons for using fictitious names and places throughout the study. In terms of reader interest and the book's impact, it would have been unquestionably more effective had authentic names been given. However, Helmreich argues rather forcefully that he had made a commitment to the Black Crusaders to protect their identities, and this commitment took precedence over his concern with the study's impact. While the reader might feel somewhat shortchanged, he or she cannot help but admire the author's sincerity. True, the use of pseudonyms makes it somewhat difficult to authenticate some of the specific findings; but, on the other hand, the case study's very anonymity lends to its being compared with similar experiences in other communities. What now must be determined is the extent to which this study represents what happened in such cities as Los Angeles, Oakland, and Chicago. And, most important, what needs to be explored is the relative importance of community, as compared with "benign neglect" by the power structure, in the careers of militant organizations similar to the Black Crusaders.

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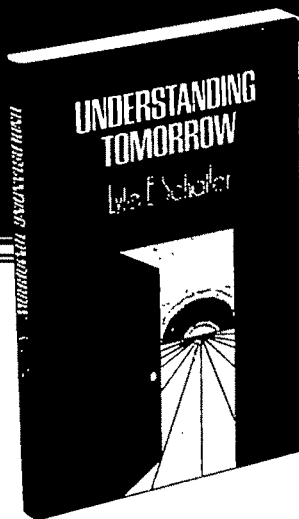
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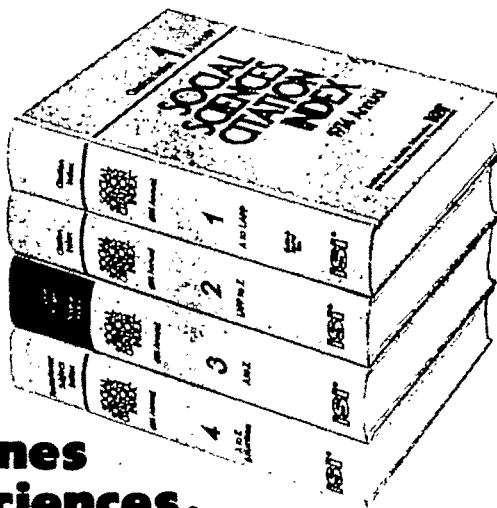


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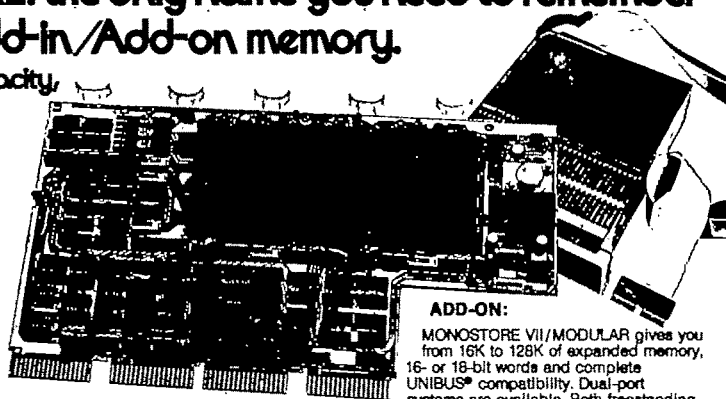
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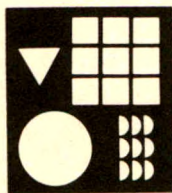
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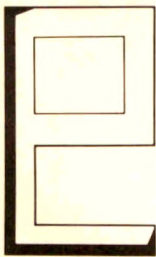
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